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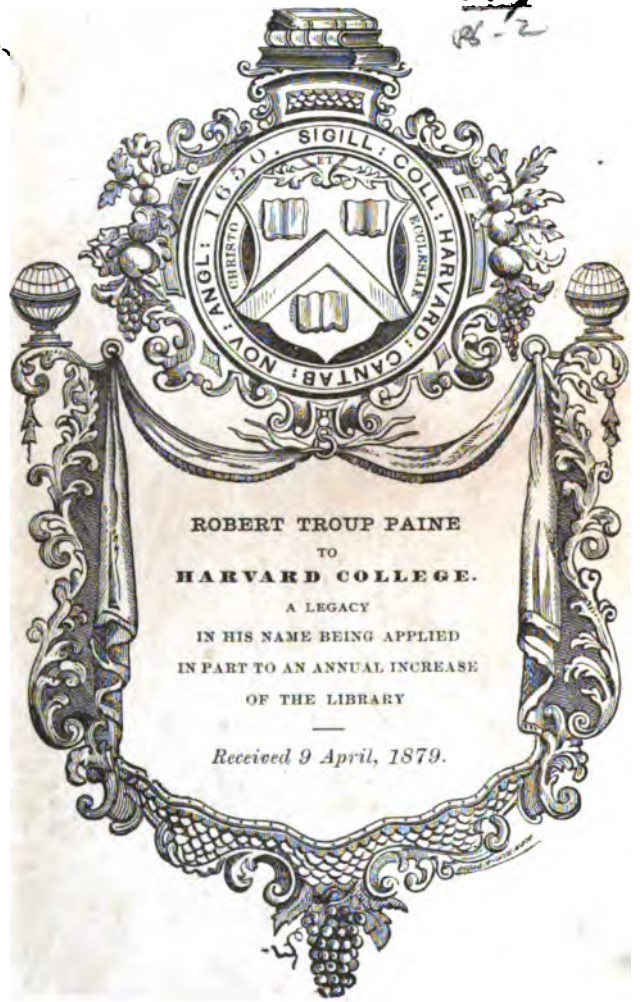

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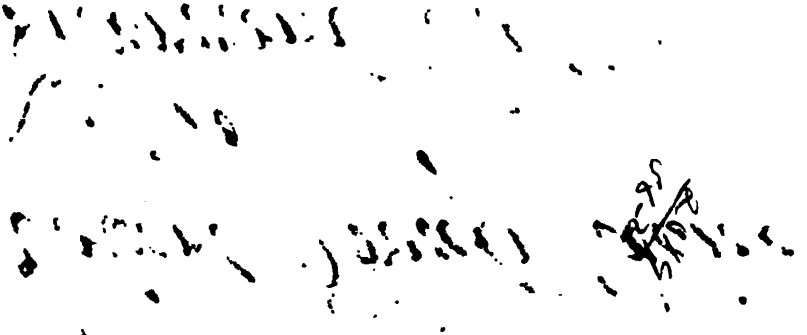
COMMENTARIES.



MEDICAL AND PHYSIOLOGICAL

COMMENTARIES.

BY

MARTYN PAINE, M. D. A. M.

——Vitæ tam vires quam actiones expono. — *Biblia Naturæ.*
Morborum quoque te causas et signa doceo. — *Virgil.*

IN TWO VOLUMES.

VOL. I.

NEW-YORK:
COLLINS, KEESE & Co. 254 PEARL-STREET.
LONDON:
JOHN CHURCHILL.
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TO

THE AUTHOR'S FATHER, THE HONORABLE

ELIJAH PAINE, A.M. LL.D., (Harv.) A.A.S.

Νηπιος 'ος-ις γονεων επιλαθεται. — *Soph.*

Inscribed to the Memory

OF

JOHN WATTS, M.D. A.M.

LATE PRESIDENT OF

THE COLLEGE OF PHYSICIANS AND SURGEONS, NEW-YORK.

EMINENTLY PHILOSOPHICAL AS

A PHYSICIAN, AN ARDENT PHILANTHROPIST, AND

A PATRIOT IN PRINCIPLE, HE WAS

AN ACKNOWLEDGED ORNAMENT OF HIS PROFESSION.

PREFACE.

It may be proper to say, that nearly the whole of our first volume was printed before the expiration of the last year. This will explain the occasional references to late authors which might otherwise appear out of place.

It may be thought that our articles on the Philosophy of the Operation of Bloodletting, the Pathology of Venous Congestion, and the Humoral Pathology, are too much extended. But, as the two first of these subjects have received no attention from authors, we felt it necessary to illustrate our doctrines by a wide range of observation. Nor could we, in any justice to ourselves, or to the distinguished philosophers who differ with us upon the humoral pathology, have pursued a different course.

In our first essay, we have endeavoured to show that the great question relating to the Vital Powers is in no respect a speculative one; and, since all our other subjects revolve about it, we have made that article, also, of unusual extent. What we have said of the *nature* of the vital principle is certainly much less obvious than its *existence and properties;* but it is, nevertheless, a subject of no little interest to the philosopher, who, we doubt not, will impartially consider our facts and arguments. We do not profess that more than reasonable conjecture can be attained as to the *nature* of that principle, in the light in which we have regarded it. We have thus adverted to this subject, as it is the only one upon which we are disposed to allow that we have arrived at prob-

able conclusions without demonstrative proof, or at least, such proof as we must be contented with in the science of life. One of the greatest drawbacks upon philosophy is the prevailing propensity to reject all evidence which is not of a purely sensible nature; and, it is especially upon the ground that the *vital principle* has not been rendered as obvious to the senses as the material part which it animates, that its specific essence has been denied. All authors, however, in discoursing upon vital actions, adduce a variety of facts which as clearly denote the existence of such a principle, as do the more tangible manifestations the organization itself. The conclusive nature of the former proof may be shown in its acknowledged application to the spiritual part of man. Every physiological step which is taken by the opponents of a vital principle is a tacit admission that such a principle exists.

It has been suggested to us, from a source in which we have much confidence, that we may not have done justice to Mr. Abernethy's views of life, as stated in our first volume, page 79. We have certainly endeavoured to avoid all misapprehension of an author's meaning; and that any misconstruction may be detected, we have almost invariably appended notes, by which the reader may be enabled, at once, to exercise his own judgement upon the subject. Our marginal references are, therefore, necessarily very numerous. In quoting so many authors, we may have accidentally, but never intentionally, substituted some words of our own. In respect to Mr. Abernethy, we think a fair representation has been given of his meaning; and our only motive for having thus adverted to this philosopher, was that of exemplifying the importance of a constant regard for fundamental principles, by exhibiting a departure from them in a distinguished instance. If Mr. A. defended, in the main, the doctrine of life as laid down by Mr.

Hunter, he should not have contradicted it by a palpable negative; and this, too, enforced with a view to correct "a misapprehension that had existed," as to *his* doctrine of life.

In endeavouring to substantiate our doctrines, we have preferred the facts of others to our own; and, this is the more advantageous, as they have often had little or no reference to the conclusions we have derived from them, and especially so in the numerous instances in which we have employed them in opposition to doctrines they were intended to sustain. In the humoral pathology, particularly, we have resorted to the latter mode of demonstration, which must be allowed to be the most conclusive.

In many of our quotations, we have substituted Italic for Roman letters; but, as we have frequently neglected to say so, we wish it, where the sense may be affected in our favour, to be considered as probably our own emphasis, — having taken this liberty to save the necessity of comment.

We have referred to an appendix on the advantages of learning to a physician; but, having very far exceeded our intended limits, it has been omitted.

We have endeavoured to notice all the typographical errors of any importance.

New-York, June 1, 1840.

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ERRATA.

Page 14,	eighth line from top,	for	*deny*,	read	affirm.
40,	eleventh	"	*what*,	"	which.
55,	twenty-seventh	"	*phenomena*,	"	phenomenon.
70,	thirty-fourth	"	*voice*,	"	speech.
74,	thirty-first	"	*of each*,	"	to each.
74,	forty third	"	*Addison*,	"	Alison.
85,	thirty-ninth	"	*diversibility*,	"	divisibility.
163,	twelfth	"	*not his*,	"	his not.
274,	thirty-ninth	"	*p.* 7,	"	p. 22.
297,	sixth	"	*last mentioned author*,	"	Dr. Armstrong.
313,	twenty-ninth	"	*ub*	"	ut.
387,	tenth	"	*has shown*,	"	was shown.
398,	thirty-fifth	"	*reminds*,	"	remind.
419,	forty-second	"	*vol. ii.*	"	vol. xi.
461,	sixth	"	*climited*,	"	eliminated.
503,	forty second	"	*auxiliary*,	"	axillary.
544,	eighth	"	*Does this not arise*,	"	Does not this arise.
707,	forty-third	"	*M. Gras*,	"	Mr. Crosse.

The foregoing we believe to be all the typographical errors of any importance.

THE VITAL POWERS.

SECTION I.

WHAT is life? "Life," says Bichat, "is the assemblage of the functions which resist death."(1) This eminent physiologist, therefore, considers life a *result*, not a *cause*.

And thus Mr. Lawrence: "The primary or elementary animal structures are endowed with vital properties; their combinations compose the animal organs, in which, by means of the vital properties of the component elementary structures, the animal functions are carried on. The state of the animal, in which the continuance of these processes is evidenced by obvious external signs, is called life."(2)

Here the doctrine is stated, in the former part of the foregoing extract, which we shall endeavour to illustrate and defend. We shall see that it is more or less admitted by most physiologists, whilst strangely enough, "the vital properties" are at the same time declared to be merely imaginary existences, and that a living animal is constituted of a particular structure and certain actions. These actions are said to be the essence of life.

The foregoing definition, though commonly received, can be admitted only in a popular sense; and the very account given by Mr. Lawrence implies that his last induction is not philosophical. It is for the philosophical construction that we shall contend, since this is of the utmost importance in a practical sense; whilst another great motive for endeavouring to indicate the distinction, is to divest the phenomena of life of the imputed agency of the physical forces. We shall start, indeed, with the fundamental principle, that the whole science of physiology, and of medicine, is primarily concerned with the natural and morbid conditions of the vital forces. Those conditions are denoted by the vital phenomena,—which, therefore, though only results, are yet the interpreters of the varying states of the vital powers, and our guide to the application of agents whose action is alone upon those pow-

(1) Recherches Phys. sur la Vie et la Mort. p. 2.
(2) Lectures on Physiology, &c. p. 77.

ers. As these powers may be variously modified by artificial or other agents, so also will their consequences or vital symptoms, with the organic lesions to which the altered forces may lead, constantly inform us of their varying relations to their natural standard.

In all our investigations, we shall have a constant reference to the powers of life;—and it may therefore be proper to explain, in this place, the general sense in which we shall employ different terms that may relate to this subject.

The words,—*vital principle*, *vital power*, *organic force*, *organic power*, are used synonymously, and refer to the universal cause of animal and vegetable life, and are employed as collective terms.

Vital properties, *vital powers*, *and vital forces*, signify the various elements of the vital principle or vital power, as they are manifested in *irritability*, *contractility* or *mobility*, *vital affinity*, *sensibility*, and *sympathy*. These several properties have, also, their specific modifications in different parts.

The word *law* will be used with different imports, the nature of which will be manifested by the subject itself. The same may be said of *cause;* and whenever the qualifying word *final* is not prefixed, the *efficient* is intended.

Some able writers have lately appeared, who, admitting that life consists of a certain series of phenomena peculiar to organized matter, and having endeavoured to explode the entire doctrine which regards the forces upon which those phenomena have been supposed to depend,—have proceeded so far as to affirm that the Deity himself is the immediate cause of all the phenomena of nature. The latter construction has arisen, in part, from the irresistible conviction that actions of all kinds require a certain power for their development. With this class of reasoners it will be difficult to argue, since their doctrine is a matter of faith, and not of reason. There is no common ground betwixt us. We will say, however, that whilst we equally acknowledge the superintending care of the Creator, His method of governing the material world consists as perfectly in the agency of certain forces appertaining to matter, as the matter consists of something distinct from the Deity. The existence of both depends equally upon His will;—that is to say, the Maker of the Universe having brought them into existence, it is His will that they shall so continue, and that the forces of matter, like the mind, shall operate after a certain manner, and according to their respective en-

dowments. Any thing beyond this we believe to be sophistry: — and all writers, who deny to living matter an "organic force," begin to expound the actions of life through the medium of such a force, the moment that those actions become the subject of consideration.

Others, again, who justly hold a high rank as philosophers, contend against the "vital principle," the "organic force," and all analogous powers; but, allowing that there is such a thing as life, which is made up of its phenomena, and seeing no other way in which the actions of life can take place but through the agency of something that is distinct from the essence of matter itself, affirm that matter is endowed with certain "properties" which govern its phenomena, and which, in living organized matter, are susceptible of impressions from what they very properly denominate "vital stimuli." They also admit that those stimuli are the remote causes of vital actions, whilst the properties in concert with the organized matter are the immediate source of the phenomena.

This is exactly our doctrine; only, we generally employ "vital force," "vital powers," &c., in preference to "property," to contradistinguish the forces of life from those of chemistry and physics; and because we think their import is, in every sense, more lucid, and therefore more intelligible.

The whole reasoning, therefore, of the opponents of the doctrine that relates to the forces of life, appears to us but little better than sophistry; and we shall find that the moment they enter upon the actions and the various results of life, they are as much employed about the "vital principle," as ever was Mr. Hunter himself. They may continue to disguise the question under the substituted name of "vital properties;" but, there is no distinction. Even they, who suppose that there is nothing intermediate between the Creator and the actions of life, have their "vital principles," or their "chemical forces," in as full operation as ever did Van Holmont or Stahl their "intelligent soul."

But what is most remarkable, after treating philosophy in the foregoing manner, they take it upon themselves to affirm that the whole matter is now fixed, and none but a blockhead can presume to disturb the quiet of the subject. (1) We may say,

(1) The following paragraph, published since the foregoing was written, presents the latest position of our subject:

"Regarding life, in the abstract, as synonymous with vital action," says the British and Foreign Medical Review, (a) "or, in any one living being, as the aggregate of phe-

(a) January 1839, p. 171.

however, that not a single fact has been elicited to illustrate the subject that was not equally enjoyed by Hunter and Bichat; and sure we are, that better reasoners and better arguments have not appeared since their day. Who then shall say, that I have settled the question adversely to those giants in mind, and none but the ignorant can pay the homage of his respect to a doctrine which my happier stars have exploded?

Still we shall see, that however the philosophical view of life may be derided, every writer betrays the entire inadequacy of his own hypothesis, by reinstating the general doctrine of Hunter, as soon as he approaches the vital phenomena. Were it indeed otherwise, he would be unintelligible to others, nor would he understand himself.

As to the existence of an electric or some analogous fluid that forms the substratum of the forces of life,—that is another question. We do not believe it ourselves, and shall endeavour to controvert it. But we believe that there are forces, or "properties," if you prefer, appertaining to matter which are distinct from the essence of the matter itself. In living matter these are denominated "vital properties," by the greatest adversaries of the "vital principle;"—and it will be also one of our objects to contradistinguish those properties from the matter which they animate.

Returning to our inquiry, "what is life?" and to the consideration of its definition by Bichat, and the philosophers of his school,—we consider the *functions* as being merely the result of peculiar forces operating upon organic matter, and that life virtually consists in the co-existence of these forces and that peculiar substratum. The forces are, to a certain extent, in a passive state, when not excited by their appropriate stimuli. But they are still the essence of life; and whilst they endure, whether in an active or seemingly passive condition, life is constituted. The sensible functions, therefore, are only the manifestations of those forces, and they will depend upon the varying states of the forces.

nomena by which that being is characterized, the author, (Mr. Carpenter,) shows, that, instead of looking for its cause in an imaginary vital principle, or organic agent, presumed to exist for the sake of explaining the phenomena, we ought to study the properties which organized structure enjoys, and the agents which produce their manifestation. Some observations are made in refutation of the doctrine of a vital principle, and we do not think them supererogatory; for, although the hypothesis could have hardly been expected to survive the fine scientific thrusts of Dr. Pritchard's classic weapon, or the strokes of Dr. Fletcher's more truculent blade, it seems even yet not quite extinct."

Müller says, that "life itself, namely, the manifestations of the organic or vital force, begins under the influence of certain necessary conditions." (1) Here the necessity of an organic force is obscurely admitted; but the operations of this power are only the manifestations of life. Such, too, are the peculiarities of the forces of life and their manifestations, that the construction cannot be defended, as has been attempted by analogies derived from the inorganic world. Life, for instance, is compared to motion, and its cause to gravitation. We shall see, however, that life is present when there is nothing analogous to motion, and that it is constantly operating, under particular circumstances, without the concurrence of any appreciable motion, and in a very different way from gravitation. Again, on the other hand, chemical forces have no existence in relation to any one particle of matter; but, when another of a different kind is brought in apposition, the action of the forces is developed by their concurrence, and a remarkable effect follows. But there is no analogy in this case with life. The powers of life are constantly inherent in every particle of living organized matter, and, as we shall show, they are always more or less in operation, though no motion be the result.

"The fœtus," says Bichat, "is never in action, because, in seeing, in hearing, in smelling, and in tasting nothing, it is not disposed to exercise the touch in any way." Yet, all the faculties upon which those functions depend, exist as well before as after birth. They were not, however, in "exercise." Thence, it appears to us that our author was wrong in saying that "the fœtus resembles the man who is born without those senses," and that "it possesses no animal life." (2) Still less is it just to argue a minor immorality in destroying the child before than after birth. (3) And yet, if we admit the foregoing premises, the conclusion will also follow, that the soul has no existence till the brain begins to receive impressions, and its intellectual operations shall have commenced. According to our author's rule, if there be no judgement, reflection, &c., in the perfect fœtus, it is like one born without a head. But our author's position is here indefensible; and since, therefore, the fœtus or a new born infant has as much a soul as man, we argue, that if the child sees, hears, tastes, smells, and feels, as soon as it enters the world, the properties on which those functions depend, had a full existence in

(1) Elements of Physiology, vol. i. p. 29.
(2) Op. Cit. pp. 193, 198. (3) Ibid. p. 199.

the fœtal state at the time of birth. There was nothing wanting but external agents and its independent life to call them into action. They are in no respect created by its new condition. And so it is with the soul; only in the former case, so far as manifestations are concerned, there is much greater proof at the time of birth of the antecedent integrity of the animal powers, than of the soul. If we admit, therefore, the latter, we must the former. We may as well deny that the soul has no existence in the fœtus, because it manifests no act of intellection, as that there is no life where there is not constant action. And if it be admitted in any one case, that certain vital powers may be present, without action, the principle is applicable to all other cases. However "the blow, which terminates the life of a man, were to destroy his organic life only, and suffer the animal life to subsist without alteration, such blow would be regarded with indifference," (1)—it is nevertheless true, that the organic constitute all the fundamental principles of life. Bichat's suppositious case, upon which the argument in respect to the fœtus rests, is therefore a mere creation of fancy, and is to be only found in the world of spirits. Its very sophistry shows, that the whole conclusion in respect to the animal powers, and the spiritual condition of the fœtus, is without foundation.

What we have now said of the animal, may be more strongly affirmed of the organic powers. There is life in a seed, an egg. It is only necessary to apply the appropriate stimuli, and vital actions take place. "From the instant the actions begin, they go on with as much precision as they ever will." (2) Müller says, "the quiescent state of the vital principle, as it is seen in the egg before incubation, or in the seed of plants before germination, must not be confounded with the state of death; it is also *not life*, but a specific state of capability of living. Life itself, namely, the manifestation of the organic or vital force, begins under the influence of warmth, atmospheric air," (3) &c. In the first place, here are manifest contradictions, necessarily growing out of the denial of life to the egg and seed; for what is the "vital principle," "organic force," "vital force," but life? They are surely something more than "a capability of living."

(1) Op. Cit. p. 199.

(2) Bichat's Recherches, &c., p. 202.

(3) Elements of Physiology, vol. i. p. 28.

In another place, our author endeavours to show that the blood, serum, &c., are "evidently endowed with *life*;" and that "the impregnable part of the egg, the germinal membrane, is a *completely unorganized* aggregation of animal matter, and

Dead matter possesses the *capability*, inasmuch as it becomes living matter when subjected to the action of the vital forces. But again, if the egg possess not life, and has only "a capability of living," it has not the forces of life, for this would be life itself; and if life be produced by warmth, air, &c., it would then follow that these agents are capable of creating these forces; which is contrary to all philosophy. We maintain, however, as a ground of this reasoning, that the phenomena of life are only its results, and we hold that the distinction is something more than speculative, as we shall endeavour to show farther. But Müller says, that the blood has *life*;—why not then the embryo, in which the manifestations of life may be developed in the highest degree? In a popular sense it may be well enough, perhaps, to call the effects of life, life itself,—just as we say the sun rises and sets. But when we come to the subject of digestion, and some others, this doctrine will not answer our purpose.

The "capability of living," as expressed by Müller, can, therefore, mean nothing else than the powers of life. There can be no creation of these powers by the action of foreign agents,—no action upon "a capability of living;" nor can such agents develope the actions of life but by operating on the powers of life. To speak of their operating upon "a capability of living," is to us an unintelligible subtelty. Besides, we have palpable evidence of the complete existence of the forces of life in the egg, and seed, in their resistance of the forces of chemistry. (1) "From my experiments," says Mr. Hunter, "it appears that a fresh egg has the power of resisting heat, cold, and putrefaction, in a degree equal to many of the more imperfect animals." (2) If heat or moisture

nevertheless is animated with the *whole organizing power* of the future being, and is *capable of imparting life* to new matter, although soft and nearly allied to a fluid." (a) We have no objection to this, except to the affirmation that the "impregnable part of the egg is completely unorganized." Nor have we taken our author into our text, but to enable us the better to explain our own views; and we have introduced this note to show how well an author may reason, when he reasons naturally.

Our author has also another statement in this place, which, as we shall see, conflicts not a little, with views which he subsequently expresses,—namely, "the blood must be regarded as *endowed with life*, for its actions cannot certainly be comprehended from chemical and physical laws."

(1) Müller thinks otherwise, but gives no reason.

(2) Exper. &c. with respect to the power of producing heat. Exper. 39, 40, 41. Also, Lectures on Surgery, and M. Serres' Anat. Comparée du Cerveau, t. 2, p. 224. Mr. Hunter's argument in proof of the vitality of the egg, and vegetables, drawn

(a) p. 144.

be applied to a living seed, those chemical operations, which take place in a dead seed, are not only wholly resisted, but all the phenomena of life are brought into full developement. Are these resistances produced by a mere "capability of living?" Or, being exactly the same which occur in the highest ranges of life, do they not depend upon the same absolute forces,—or upon what, in the latter case, we denominate life? Do not the resistances of the egg and seed, of which we have just spoken, depend as much upon the forces of life, and their constant activity, as the more ample results? These resistances stand in the same relation to the powers of life, as nutrition, secretion, &c.

The latter functions, however, are said to be life,—according to which rule the *resistances* of the egg and seed of the chemical forces should be equally the life of those objects,—which appears to us absurd. Hence it seems, also, that "the form of the organic matter does not determine originally the mode of its action; and this is farther shown by the fact, that "the matter from which all animal forms are produced, is at first almost without form." (1) This may be obvious enough in respect to the egg and seed; but the application of the principle is more extensive, as we shall endeavour to show when we speak of the gastric juice.

From the foregoing facts alone in relation to the egg and seed, it is obvious that Bichat and many other philosophers are wrong in saying that "death is resisted by the functions." Indeed, Bichat states that "the vital powers continue for sometime after the loss of the functions" (2)—thus contradicting his definition of life. Decomposition is the only test of absolute death; and by some it is considered the only proper criterion for consigning the body to the tomb. Junker defines life as a state "opposite to putridity." (3)

The resistance to foreign agents, however, does not necessarily imply action in its ordinary sense. The vital principles of the

from their power of resisting cold, has been ingenuously, but hypothetically, opposed by Mr. Down. (a) In our Essay upon Animal Heat, we have shown by experiment, that Mr. Hunter was right,—at least as it regards vegetables.

(1) Muller's Physiology, p. 26.

(2) Op. Cit. p. 296.

(3) Conspectus, &c. See, also, Winslow on the Uncertainty of the Signs of Death, &c. Louis's Lettre sur l'Incertitude des Signes de la Mort. Vogel de Affect. Corp. Hum. p. 461. Lommius, Obs. Med. l. 2, p. 83. Horst. Hist. Morb. l. 8.

(a) London Med. and Surg. Jour. No. 272, p. 265.

seed may be brought into sensible action by certain causes, — and they resist the action of other causes without any other sensible result. But the latter no more implies the existence of vital action than the former, which supposes the forces of life to be quiescent till some demonstration is elicited.

It may be said as well that gravitation consists in the return of a comet, as that life is made up of certain movements. Both are equally the effects of distinct and independent forces, of which one is gravitation, and the other life. The only difference is, one force operates uniformly *per se*, — the other is generally brought into action by foreign causes. In treating of life, we begin with its phenomena or ~~visible~~ signs, — just as in treating of gravitation, we consider the motion of the heavenly bodies, &c. And so of all other forces. From the phenomena of life, we ascend to their causes; and here we find the essence of life, which is no more a result than gravitation.

In the fœtus, nutrition is out of all proportion to decomposition; and yet the principles upon which the latter depends, start into vigorous action at the moment of birth, whilst the growth goes on with undiminished rapidity. There is, therefore, no such thing in the fœtus as, according to Bichat, a "concentration of all the powers of the economy upon the systems of circulation and nutrition;" since, also, in regard to the powers of animal life, "fœtuses without heads possess an organic life as active as those which have no defect of organization." (1)

Remove the heart and all the blood from frogs, and they continue to leap about as usual. (2) Haller affirms the same thing of salamanders and torpedoes. Even fowls, cats, dogs, &c. have run about after the removal of the heart. (3) Here the vital forces, being deprived of their accustomed stimuli, appear to exert an independent action, and to approach, in this respect, the physical forces.

We might mention more remarkable instances of the foregoing nature, were we not averse to extremes. Thus, Dr. Mitchell of Philadelphia, having inflated the heart of a sturgeon, (ancipenser) "hung it up in this state to dry, — when it began to move, and continued for ten hours to pulsate regularly, though more and more slowly; and when last observed in motion, the auricles had become so dry as to *rustle* when they contracted and *dilated*." (4) We

(1) Bichat, Op. Cit. pp. 226, 513.
(2) Spallanzani on the Natural Hist. of Animals.
(3) Haller's Element. Phys. t. 1, s. 4. l. 5, p. 486.
(4) Dunglison's Human Physiology, vol. ii. p. 148. — 1830

wish this singular fact to be borne in mind, as we advance in our inquiry into the nature of the vital forces; for however unusual it may be, it reaches very far into the merits of our subject. We find, also, a very similar statement made by Dr. Marshal respecting the vital force as manifested in the heart of another animal.(1)

The foregoing considerations would lead us to regard life as a cause, and to define it as consisting of certain specific properties appertaining to organized matter, which are more or less capable of resisting the destructive agencies of inorganic matter, and the forces to which it is liable, and of protecting against them the matter in which they are inherent. We thus embrace the elements of a good definition; philosophical accuracy ,brevity, and a peculiar, universal characteristic. Other characteristics might have been substituted, as, that the forces of life are capable of being acted upon by foreign agents; or, that they only manifest their effects when acted upon by certain agents.(2)

It cannot, therefore, be said, in an abstract sense, that the forces of life are the primary cause of organization, till it be shown that organization is not the substratum in which the forces are originally inherent. He, who created the powers of life, associated them with ~~the rudiments of~~ that organization which they were destined to unfold. The rudiments have been perpetuated in connection with the living forces since they came from the hands of the Creator, and are the present source of all animated beings. This plan of unity and consistency extends through all matter possessing life, — since it would be a violation of one of the most obvious analogies of nature to suppose it otherwise. The moment inorganic matter is brought into a state to receive the vital force, however low in degree, we hold that it must be more or less organized. If chyle, blood, semen, the gastric juice, &c. possess life, so also must they possess organization. It is, moreover, the result of universal experience, that inorganic matter is totally destitute of any part of the principles of life.

It is certainly possible that the forces of life may originate their primary instruments of action, although opposed to the dictates of reason. Vessels are the earliest signs of development in the incubated ovum; and whenever extravasated blood or lymph

(1) On the Morbid Anatomy of Hydrophobia, p. 250. *Note.*

(2) In the case of the dried hearts just mentioned, we apprehend that heat and the air acted as stimuli to the forces of life. In the examples from Haller, the animal heat was probably the stimulus.

When heat promotes the operation of the forces of chemistry, its effect appears to be upon the matter itself, by separating its particles, &c.

become organized, they are always in contact with vessels. On the other hand, however, we have reason to think that matter may be organized and possess life without vessels, as the blood, gastric juice, &c.

Hence we think that Mr. Hunter has failed "to show that organization and life do not in the least depend upon each other," although it be obvious that "life can never arise out of or produce organization."(1) Nor do we think Mr. Millingen more happy in his criticism of Hunter's doctrine, where he states, that "the fact is, organization is the result of life." Indeed, this principle is contradicted in the same sentence, when he says that, "without organization life cannot be transmitted." This is palpable enough, and carries us back to the first era of living matter, when organization and life were cotemporaneously created. In all our inquiries upon this subject, we irresistibly start with organized matter; and whilst Millingen is endeavouring to show that "organization is the result of life," in the sentence which follows this quotation, he says, that "the embryo could not be developed, did not the fluid that animates possess a principle of vitality which it communicates to a body previously organized." And what is not a little *apropôs*, he goes on in the next sentence to say, that "in this confusion, the word *life* has been sometimes applied to the power, and at other times to the result." (2)

"The living principle" appears, therefore, in a philosophical sense, to be neither the result of organized structure, nor, as stated by Millingen, Prout,(3) and others, the cause of organization. Both have coexisted since they were the product of Creative Power, both are necessary to the vivification of dead matter, and the co-operation of both to the farther development of each. Whenever, therefore, we speak of the organizing of matter, we necessarily imply its endowment with the properties of life; and, *vice versa*, the communication of such properties to matter equally denotes its organization. They are, therefore, convertible expressions. But organized structure may remain entire, for an indefinite time, after being deprived of its vital forces.

Before we leave the simple condition of life as manifested in the seed and egg, we may state a conclusive proof of their endowment with specific forces, upon which the foregoing phenomena depend. This consists in the disappearance of those

(1) On the Blood, &c.
(2) Curiosities of Medical Experience, p. 216.
(3) Chemistry, Meteorology and the Function of Digestion, b. 3, ch. 1.

phenomena under particular circumstances, whilst the organized structure remains unimpaired; nor can the phenomena be reproduced. Just so it is with the most highly organized beings. A period arrives, when the phenomena of life cease forever, and the organized structure begins soon afterwards to separate into its ultimate elements. Here, then, are two facts which demonstrate the existence of positive forces on which the actions of life had depended, and by which the elements of the organized matter were held in a peculiar state of combination. Those forces, also, were as perfectly distinct from the matter itself, as the soul, which has vanished with them; otherwise, the actions would have been perpetual, and the elements of the organized structure would have remained without change. There appears to be no possibility of avoiding these conclusions. But the facts now stated are only the beginning of a series whose import is the same, and equally irresistible. We might, also, here draw upon analogy, as supplied by the phenomena which result from the union of the soul and organized matter, for a proof that the organic functions are carried on by specific forces distinct from the matter itself.

Although we have just seen that Mr. Hunter affirms that "organization and life do not in the least depend upon each other," he states in another place, that "the arrangement for preservation, which is life, becomes the *principle* of action, not the *power* of action, for the power of action is one step farther. The power of action must arise from a particular position of those living parts; for, before action can take place, the matter must be arranged with this view." (1)

Here, then, in the first place, Mr. Hunter makes the mere "arrangement for preservation" to constitute life. It would therefore follow, that heat and other vital stimuli, by acting upon such arrangement of matter, ~~is~~ capable of creating the organic force, which is contrary to analogy. But we have already seen, by Mr. Hunter's own facts, that the egg possesses capabilities in its passive state analogous to those of living animals, and which can be nothing less than the vital force.

Again, we would ask, what is the difference betwixt the "principle of action" and "the power of action?" "Before action can take place," says Mr. Hunter, "the matter must be arranged with this view." Now what is it that produces this new arrangement

(1) Lectures on the Principles of Surgery, p. 20.

but "the power of action?" It is, therefore, as much the power of action before the new arrangement is effected, as afterwards.

It is evident that the formation and maturity of the egg and seed must be effected by the "power of action." But when they have acquired their full maturity, and the vital stimuli are withdrawn, by a law which is peculiar to this state of organized matter, the vital forces, or the "power of action," become dormant. This is an extraordinary peculiarity in relation to the germ, and is manifestly intended for the preservation of the species. It may exist in this quiescent state for ages. We know that it has in seeds for two thousand years. (1) But as soon as the vital stimuli are applied, those powers, which had been once in action, are again set in operation, and new changes follow. From this time, the actions cannot be suspended as a whole, without the complete extinction of the organic force itself. This circumstance, by the way, separates, entirely, the vital and physical forces, and shows us how fallacious are all illustrations of life which are drawn from the forces of inorganic matter.

The history of the seed and egg probably supplies one of the most remarkable illustrations of design that can be found in nature, — especially that of the former. They are the only instances where the entire forces of life cease their ordinary operation without becoming extinct; and were it not for this interval of repose, the species would probably disappear, — since, even if the vital forces carried out the development of the seed into the plant, the chances of preservation would be infinitely diminished, — and since, also, such as ceased their operation at the maturity of the seed, are supposed by the proposition to become extinct.

Here, then, is a great and specific final cause to be answered, the very nature of which proclaims the continuance in the dormant seed of those forces on which its growth and maturity depended, and which are ultimately destined to go on with the final purpose of nature to rear another plant, that other seeds may follow.

The farmer looks with anxiety towards the heavens when the time of harvest has come, lest the scorching sun may not properly "kill his grain." He is sometimes disappointed. The rain falls, and there is no check to those actions which had brought the seed to maturity; but the vital stimuli urge them on, and the forces of life pass the ordinary limit of quiescence without a

(1) Treverinus' Biologie, and others.

momentary suspension of their actions. These actions, and all their results, are exactly the same as after the seed has been dormant for a thousand years; and their uninterrupted progress in the former instance shows an identity of force before and after germination, and thus connects the principle, on which a renewal of action depends, with that of uninterrupted action. This illustration also shows, that a perfect integrity of the vital forces exist during the state of quiescence; and it is, moreover, opposed to all analogy, that such forces may be reproduced after they shall have become once extinct.

Something like the foregoing is seen in the hybernating animals during their state of torpor. The influence of cold upon the forces of life nearly extinguishes all vital actions; but that life is undiminished, is sufficiently manifest when the farther and more profound operation of cold re-establishes all the phenomena of life in their highest vigour.

We may also take an intermediate gradation of the "power of action," as exemplified by Mr. Hunter, and show by analogy, as well as by admission, how the *principle* and *power* of action are the same. Thus, says Mr. H., "many parts of an animal appear to have *little action;* yet they are as much endowed with life as the more active parts; such, for instance, as tendons, elastic ligaments, &c." and so of the "fresh egg, which has no vital action." (1)

We find the following statement in the last No. of the British and Foreign Medical Review: (2)

"The dependence of the vital properties on the structure, Mr. Carpenter enforces by a consideration of the nature of death; showing, that when integrity of the organization is maintained by the continuance of its vital action, (particularly nutrition,) the change of structure consequent on the cessation of the action necessarily involves the loss of vitality. *Molecular* death, so designated and distinguished by Dr. Symonds, from *systemic* or *somatic* death, may, in most cases, be said to consist in the cessation of vital action in the part, because the latter, as we have just observed, necessarily deprives it of its vital properties by producing its disorganization."

In the first place, it may be remarked, that here also, Mr. Carpenter fully concedes the existence of peculiar forces or properties, which disappear at death, and are, therefore, something distinct from organized structure itself; for it cannot be admitted, as

(1) Op. Cit. p. 22.

(2) January, 1839, p. 173. — We notice Mr. Carpenter's opinions, without the advantage of reading his work, on account of his high reputation, and the encomiums of his able reviewer.

it certainly cannot be shown, that a change of structure occurs before those properties disappear. The very test of their existence, the vital actions, shows, by their sudden cessation, that there has been an assumption to sustain a hypothesis. Those "vital properties," therefore, which have been so much condemned when spoken of under the name of "vital principles," or "organic force," are exactly our vital properties, as they were, also, those of Hunter and Bichat; or, as we more generally call them, "vital forces," or "vital powers." The candid reviewer admits, what we have already urged, in proof of the distinct nature of the forces of life, and of life virtually consisting in those forces, that "there are exceptions to this statement in the case of seeds which retain their vitality for an almost indefinite period, though it cannot be said that they are the subjects of vital action; and also in *certain animals* of comparatively simple organization, in which life (the vital actions) may be suspended for a considerable time without deprivation of vitality."

Here, then, is all that is necessary to show the true merits of the question. It is reduced to a nutshell,—just as in seeking for any great law of development, we go back to the most simple condition of organization. But we apprehend that the great principles in relation to life are especially universal, and admit of no "exceptions."

If, therefore, vital actions may be suspended in certain inferior animals, without a "deprivation of vitality," (and this even after drying them,) we are entitled to the conclusions, that the "vital properties" constitute life; that the actions are only an effect of those properties operating on organized matter; and that the former, at least, may be suspended without a "change of structure."

But Mr. Carpenter is said to have shown, that the integrity of the organized structure, as well as the vital properties, are maintained by the continuance of vital actions. That their "cessation necessarily deprives the organization of its vital properties by producing its disorganization." This, however, is palpably contradicted by the admitted facts which we have just stated,—and the principle, therefore, can have no foundation.

Admitting this, however, to be correct, there would be no function appertaining to the vital properties,—since the power of privation ascribed to the vital action in the foregoing quotation, implies the dependence of the vital properties, as well as the organized, structure upon that action. But this appears to be only a metaphysical subtlety; since, from what we gather from

the reviewer, Mr. Carpenter has, at other times, as much regard for the important duties of the "vital properties," as any other vitalist.

We think we do not misapprehend our author;—but to make the matter clearer, and to show another exception to the supposed great law of nature, we will add the following extract:—

"Mr. C., and others, have found that the wheel-animalcule may be reduced by desiccation to a state in which vital action not only appears extinct, but which would seem quite incompatible with its continuance; yet, on the restoration of moisture, life returns. In both these cases, (the former being one,) the structure is not readily susceptible of decomposition from the operation of ordinary external agents; and, consequently, its integrity is not dependent *on the continuance of vital action*, and being unimpaired in organization, it retains its vital properties."

If there be any consistency, therefore, in the fundamental laws of nature, neither the vital properties nor organized structure have any primary dependence on vital actions; but, on the contrary, vital actions depend wholly upon the integrity of the former. Indeed, we know not how it be possible to conceive of action without power as its cause; and to make power, or property if it be preferred, to depend on action, with which the action itself appears to be associated as an effect, is beyond the reach of our imagination.

That Mr. Carpenter will sanction our construction of the universality of fundamental laws, and the inductions we have made, we have little doubt from his own doctrine upon this subject, as expressed in the following words:—"Our belief," he says, "in the uniformity of nature, which leads us to seek for a common cause, when a number of similar phenomena are presented to our observation, is based not only upon experience, but upon the conviction which every believer in the existence of the Deity feels of his immutability." This conclusion is strengthened by the subsequent reasoning. (1)

Again, if the vital properties and organization depend on vital action, in a majority of instances, as in the higher animals, what is it that produces a cessation of action when death takes place? Not a loss of the vital properties, nor a change of structure, for these are, by the premises, consequent on the cessation of action. But we are also told that vitality exists whilst the vital properties and organized structure remain; from which it would clearly result that there should be no such event as death.

The supposed change of structure, also, as a necessary con-

(1) British and Foreign Medical Review, January, 1839.

comitant, or concurring cause of death, in a majority of cases, is in every sense, and *prima facie*, hypothetical. It appears to us not to possess a shadow of proof in its favour. (1) Will it be maintained that there is a change of structure produced by a division of the medulla oblongata, or by a dose of prussic acid? And yet the animal is dead on the instant.

Again,—"let it be borne in mind," says Mr. Carpenter, "that when a *law* of physics or of vitality is mentioned, nothing more is really implied than the simple expression of the *mode* in which the Creator is *constantly* operating on inorganic matter or organized structures." (2)

This is very well as it regards the creation of all things by the Deity, and their dependence, in a general sense, upon His will. But, as we have already said, His "mode of operating on inorganic matter, or on organized structures," consists as perfectly in the agency of certain forces appertaining to that matter, as the matter consists of something distinct from the Deity.

"Mr. Carpenter," it is also said, "argues on the superfluousness of a controlling or presiding agent, (the vital principle,) intermediate to the will of the Deity and the phenomena of vital actions, when the latter can be reasonably assigned to the reciprocal relation between the *properties* which belong to organized structures, and the *stimuli which* excite them." (3)

Here, then, after all, is nothing more than Hunter's and Bichat's doctrine of life. By the "vital principle," they mean equally with our author, "vital properties,"—however Mr. Hunter's phraseology, for the purpose of emphasis, may be sometimes extravagant. But take the different parts of his work in connection, and his meaning is clearly what we represent.

Again,—those philosophers supposed equally with Mr. C., that the "phenomena of vital actions could be reasonably assigned to the reciprocal relation between the properties which belong to the organized structures, and the stimuli which excite them,"—and, indeed, they supposed nothing else. Those "vital properties," were Mr. Hunter's "vital principle," whose actions he at all times supposed to depend on certain "stimuli." Nor did he "require any agent to adjust and regulate the actions which ensue from their mutual dependence."

We may take, also, the foregoing quotation to show by the

(1) Farther on, we shall see that this is fully admitted, even by those who expound the actions, and other results of life, upon chemical principles.

(2) Ibid. p. 172. (3) Ibid.

argument, as well as admission of our author, that neither the vital properties, nor the organized structure, depend upon the vital actions; but, on the contrary, that the vital actions depend wholly upon the vital properties,—since the former "may be reasonably assigned to the reciprocal relations between the *properties* which belong to organized structures, and the stimuli which excite them." It is also clear from these premises, that the vital properties must cease antecedently to the cessation of vital action; and that, therefore, vital action being nothing but an effect, the vital properties must constitute life. By the same admission, it appears, also, that organized structure does not depend on vital action.

By what process do we alter the vital actions when medicinal agents are employed? Do we operate upon the actions? Certainly not, you reply; but upon the "vital properties;" and then follows a change in the "vital actions." It is, therefore, manifest, that the action, or phenomena of life, are merely effects; whilst it is equally evident that the "vital properties" have as much a real tangible existence, as Mr. Hunter's "vital principle."

But, again, by the foregoing extract, we learn that the "vital properties" are "excited by stimuli;" and our author devotes a chapter to their consideration. They do not act upon the matter composing the structure. That, indeed, is plain enough when they are applied to an animal just killed by a division of the medulla oblongata. The "vital properties," therefore, must be some very real substance, either spiritual or material, which is as distinct from the matter composing the structure, as the soul, or the principle of instinct, which was simultaneously separated from the organization along with the properties, by the division of the medulla.

This is a matter for the understanding,—not for speculative philosophy; and when we are told that "vital stimuli" excite the "vital properties," and that vital actions are the result, we rationally infer that they excite *something;* (1) and that when, as in the animal destroyed in the foregoing manner, when no change can be imagined in the organized structure, the same stimuli

(1) In Dr. Gallup's work on the Institutes of Medicine, (*a*) recently published, and which has reached New-York, just as our printer is arriving at this page, we find a peculiar coincidence in language at this place which induces us to affirm that we had neither seen the doctor's work, nor had the least understanding of his views, when our own were written in the language now employed.

(*a*) See Gallup's Institutes of Medicine, vol. i. p. 45, 1839.

not only fail to produce the same actions, but produce others of a totally different nature, and which had been before entirely resisted, we as rationally conclude that *something* has taken its departure from the organization. That *something* it is clear, also, had not only been the essential cause of the "vital actions," but had also resisted the forces of chemistry, whose operation is now promoted by the *same* stimuli which had been the remote cause of the *vital* phenomena.

"Mr. Carpenter," says the Reviewer, "speaks of the compounds 'being converted into organized tissues, and endowed with *vital* properties,' &c., and again, of a process concerned in assimilating, *organizing*, and *communicating vital properties* to nutritious matter.'"

The criticism which follows appears to us exceedingly just, since the act of organizing implies the communicating of vital properties. But the expressions show how difficult it is for an intelligent mind to adhere to a hypothesis that is everywhere contradicted by nature. The very terms employed are in conflict with the reasoning. The vital forces predominate in the minds of all, as something in which life essentially consists. We are told, also, that Mr. Carpenter, "after deducting those actions which are unequivocally physical, (?) as well as the more questionable processes of organized chemistry, allows that there remains a vast chaos, which must be regarded as *essentially* vital; by which we presume he means that they are not only confined to, but that it is *impossible* that they should occur in any but *living organized structures*, (1) since they require *properties* which are *not to be met* with in *any other substances*." (2)

Mr. Carpenter, then, and all other rational philosophers, have as much a "controlling or presiding agent" as Mr. Hunter, however they may differ as to the nature of that agent; for, in conceding that certain properties are necessary to vital actions,—that for this effect the properties must be excited by stimuli, which exert no direct action on physical forces, and that under circumstances of death, those actions can be no longer produced, although the organized structure remain entire,—it follows, *de facto*, that the "properties" are distinct from the organized mat-

(1) Here the reviewer himself naturally enough falls into the supposed error which he wishes us to avoid, and speaks of a "*living* organized structure;" so evident is it that there may be *dead* organized structure. But this is different from the act of organizing, which implies the bestowing of life.

(2) Ibid. p. 175.

ter itself, and control the actions of that matter; whilst they are totally distinct from the properties of dead matter. We consider the "properties," or "forces," or "principle," however, as intimately associated with the living organized matter, as the soul,—and probably in much the same manner; whilst both are equally liable to a complete separation from the matter itself, and, therefore, in no respect belong to the inorganic world. From all which it follows, that the "vital properties," being something superadded to matter, like the soul, and, like the soul or the principle of instinct, leaving that matter under analogous circumstances, must be regarded, like the soul, as a specific existence, to which specific functions appertain.

Suppose, however, the "vital actions" constitute life, and are necessary to the existence of organized matter, it is obvious, from the premises, that they either are, or depend upon, some real existence, without which neither life nor organized matter can be produced or be maintained. And that they are something real, and not an illusion,—something distinct from the matter itself, is farther evident from their admitted necessity to the existence of organization, as well as from the fact upon which that admission is founded, that life and organization disappear when those actions cease or cannot be reproduced. The "vital actions," therefore, would be the same as our vital forces. It would be only an unhappy change of terms.

So, also, if we recur to the other construction, that the "vital actions" depend upon "vital properties," and these last are necessary to organization, the same reasoning, and the same conclusions, must obtain.

It follows, therefore, that if the destruction of organization, or "a change of structure," or "*molecular* death, may in most cases be said to consist in the cessation of vital action in the part, because *somatic* death necessarily deprives it of its vital properties by producing its disorganization," — it follows, we say, that *something* entirely foreign to inorganic is indispensable to living organic matter, and may be abstracted from it. When this abstraction is made, the organic matter not only loses all its vital phenomena, but manifests others of an opposite nature, and is, moreover, resolved into its ultimate elements. Now that *something* must be real, substantial, at least as much so as the soul,—and we can imagine no better name for it than the "vital principle,"—whose proper import we shall still endeavour to indicate.

It appears to us, as we have endeavoured to show in another place, that the attempt to establish a perfect analogy betwixt the nature and the relation of the "vital properties" to organized matter, and those of the properties of inorganic matter, have been a fruitful source of materialism. We shall have some comments upon this subject, in another place; but we may now say that able and learned men appear to have been conducted by this physical view of life, to a similar consideration in respect to the intellectual functions of the brain; and it certainly appears to us just as rational to explain the phenomena of intellection by the supposed physical properties of the brain, as, in the other case, the actions of life by any of the ordinary "properties" of matter. But when the foregoing hypothesis is made to involve the contradiction that the properties of organized matter, and even organized structure itself, depend upon the "actions of life," which, it is at the same time admitted, result from those properties and that structure, we are led to conclude that the question is merely raised for ingenious speculation,—and the more so, when we find that in all practical discussions, the hypothesis is abandoned for the only intelligible theory.

We have already explained, that we are almost indifferent as to the name which the forces of life shall receive, so long as authors continue to apply the same import to more favourite words than "vital principle," or "vital powers," though we have an instinctive aversion to "vital actions." And whenever they come to consider the actions and the results of life, we have no doubt they will continue to lose sight of any direct agency of the Deity, and even of the chemical forces,—submitting to the necessity of supposing that vital actions imply the existence of vital powers, upon which they radically depend. At least, this has been so far universally true, however the subject may have been disguised by controverting the general doctrine of life, as laid down by the efforts of the rarest genius.

As to a "spiritus archæus," or an "intelligent soul," neither Hunter nor Bichat had any such dream; nor has it, within our knowledge, been a doctrine of recent times.

The more, therefore, we investigate the subject, the more are we satisfied that life consists in the integrity of the vital properties, associated with organized matter,—that the vital actions are only the results of life, or of the foregoing conditions,—that the vital properties, or forces, are essentially distinct from organized

matter itself,—and that organized structure may exist entire without the properties of life, although the former is necessary to the existence of the latter.

It is the peculiar province of the vital forces to carry on the rudiments of organization to a most astonishing degree of interstitial development; and to this consideration we would particularly invite the attention of the chemical and mechanical philosophers, who expound the phenomena of life by chemical and physical laws.

The forces of physics and of chemistry produce precise and uniform results; whilst those of life are attended by an endless variety of modifications. This is, in itself, an ample proof that their essence is wholly distinct. One set of forces operates mainly upon inorganic matter, and are *not acted upon.* The other forces operate exclusively upon organized matter, are more or less influenced by the former, and are acted upon by foreign causes. (1)

The former are always the same under all circumstances; the latter exist in distinct modifications of kind according to the varieties of texture which they animate, and they are constantly modified in their nature by an almost endless variety of foreign agents, and they may be extinguished by those agents. (2) Their alterations constitute the essential pathology of disease. We may calculate the results of one with perfect accuracy;—we look with prophetic ken to the return of a comet through a long vista of ages. But instability is the great characteristic of the vital forces, and the exact phenomena which we may contemplate at this moment may vanish at the next, never to be repeated. The physical and chemical forces are always operating *per se*, with entire independence, and their existence is perpetual, as their nature is unchangeable. The vital forces, on the contrary, generally produce visible actions only when they are acted upon, and the mind itself is one of the agents. They become more or less passive the moment those influences are with-

(1) It may be said that we have admitted, in another place, the possibility that the chemical forces may operate in the living system in subordination to the forces of life. But, this in no respect implies that the chemical laws are directly influenced in their operation by the forces of life. When the latter determine the relation of elementary principles in such a way as to favour the operation of the former, these act as at all other times. The influence of the vital forces is exerted upon the materials, through the agency of the vessels, not upon the foreign powers.

(2) Thence the importance of the precept, that "a physician should know the properties of all things." (*a*)

(*a*) Celcus, l. 2, c. 18, p. 96. Hippocrates, Vet. Med. 36, 18. Homer, in Odys. 4, v. 230.

drawn, and they may be extinguished in a moment by those influences. They have a specific final cause, which appertains exclusively to organized matter. This final cause, which consists in carrying on the processes of life, has a natural end, when the vital forces become extinct, and give place to the full operation of the forces of chemistry. The latter then lay waste what had been the work of the former, and thus prepare new materials for the complex process of ultimately fitting them for the work of the vital forces in other animated beings.

These are broad and important distinctions. They place the respective forces in opposition to each other. The distinctions indicate not only the nature of the forces, but the rank of all matter.

Powerful efforts are making to substitute the forces of chemistry and other physical powers for those of life. We shall oppose this doctrine in all our future disquisitions; and it is therefore proper that we should, in this place, point out the distinctions as well as we may. In all that relates to the treatment of disease, a proper apprehension of the forces of life is of the highest importance. The physician who should proceed upon the chemical or physical doctrines, would accomplish little through the agency of those powers, which operate upon mere matter. He would be greatly baffled in his efforts to alter the secretions, vary the morbid actions, &c., did not his remedies concern other forces than such as his imagination contemplates. Who, indeed, would be tolerated, should he openly carry his physical doctrines into the treatment of disease, and talk of addressing his remedies to any of those powers? And yet, since he accomplishes nothing excepting through the forces of nature, and since he must operate upon the forces which produce the results, it is manifest that he is wholly concerned with powers which he pronounces visionary. We do not develope actions in the living animal by operating upon the matter of which he is composed; for the moment he is dead, no agent can elicit a single phenomenon of life. Something has vanished upon which all its phenomena depended.

Bichat, in alluding to the distinction between the physical and vital forces, asks, "what would you say, if in explaining the motion of the planets, rivers, &c. they should talk of irritability and sensibility? You would think it absurd. It is equally absurd, in explaining the animal functions, to talk of gravity, impulse, inequality of the capacity of tubes." (1) And yet Bichat

taught that life is alone constituted by the vital actions. Just so in regard to the chemical forces,—only, as we shall endeavour to show, the absurdity is even greater. Who would be tolerated that should say, this and that inorganic substance united, or others separated, in virtue of their sympathy, irritability, sensibility, &c.? For, they who maintain that the animal machine is governed by the forces of chemistry, apply these designations to the forces as related to the living body; and this for the manifest reason, that phenomena, so peculiar as those of life, require peculiar terms. The whole art of medicine consists in producing certain impressions upon forces that are wholly unlike those of chemistry and physics; and he would be considered mad who should talk of modifying the operation of galvanism, magnetism, chemical affinity, gravitation, &c. by an emetic or cathartic. It must come to this, if you will have it that those forces preside over organized matter, and perhaps, even, if they be allowed to have a subordinate agency.

Shall we say that our agents operate upon forces that are essentially vital, but the results are brought about by another set of forces totally different in their nature, and upon which our agents exert no effect? This would involve the greater paradox of supposing that the vital forces operate upon the chemical and physical, and that the results are determined by a chain of causes which have no known alliance, but which, on the contrary, are known to be hostile to each other. And should it be admitted that the vital forces establish a primary change, through the actions which they develope, and the materials from which the various products are formed, philosophy teaches us to seek for no other causes for the progressive changes, till these can be shown to be insufficient.

If we are asked how we know the existence of the "vital forces," we reply, by precisely the same means as the advocates of the chemical and physical forces defend their knowledge of the latter. (2) Would not the metaphysician laugh at you, were you to

(1) General Anatomy, vol. 2, p. 23, and his Researches on Life and Death, c. 7, p. 80.

(2) It has been lately said, that "some have supposed that the attraction of affinity, or the force of capillarity, was the power in question, operating in an unusual manner, under unusual circumstances; but the majority of medical writers *have cut the knot, instead of untying it*, and assert that it is a peculiar force, recognised under the title of vital force, life, or nature." (*a*)

(*a*) Prof. Draper, in American Journ. of Med. Sciences, vol. xxi. p. 290.

ask him for any other demonstration of the existence of mind than its manifestations? (1) or yourself, should we demand any other evidence of chemical affinity, gravitation, &c. than the effects which they supply? And do you not distinguish one from the other, and regard them as wholly distinct forces, by the differences in their effects? What more can you desire in respect to the forces of life? We see that some philosophers are wavering, whose co-operation is worth having. Thus Dr. Hall:—"the immediate effect of the causes of inflammation is plainly physical, as well as vital, *if it be vital at all.*" (2) Such must ever be the confusion when the forces of physics and of life are brought to the explanation of any given phenomenon.

Keeping in view, therefore, the peculiarities which we have noticed in respect to the vital forces, there is no difficulty in comprehending the formation of healthy and morbid products. In the natural state of the secreting vessels, they admit only such constituents of the blood as are suited to the nature of their irritability. When this property is altered by disease, its relation to the constituents of the blood is changed; new ones are admitted, and new combinations formed; and these will be as infinitely diversified as there are shades of difference in the modifications of the powers. Will physics or chemistry offer as good an explanation? But there are many constituents in the secretions which do not exist in the blood, and this implies a decomposing, and recombining power on the part of the vital forces and their instruments of action. This complex subject we shall examine in other places, especially when we speak of digestion.

Whether "*power*, *property*, and *quality*, are, in the physical use of those terms, exactly synonymous," according to Dr. Brown,(3) will depend upon the exact import which different men ascribe to each term. Being arbitrary, their meaning must be fixed by common consent. The two former terms are usually employed in a common acceptation, though *power* more exclusively refers

(1) "What is the soul?" said another to Marivaux. "I know nothing of it," he replied, "but that it is spiritual and immortal." "Well," said the querist, "let us ask Fontenelle, and he will tell us what it is." "No," cried Marivaux, "ask any body but Fontenelle, for he has too much good sense to know any more about it than we do."

We know that the soul is a simple substance, to which we ascribe many properties, which give rise to a variety of phenomena, from which alone, our knowledge of its existence and nature is derived.

(2) On the application of irritants to the web of the frog. c. 5.

(3) Inquiry into the Relation of Cause and Effect. Also, Lectures on the Philosophy of the Human Mind, vol. i. lec. 7 & 9.

to the principle upon which motion and other sensible results depend. *Quality* has, in part, the import of *property*, but more limited. We give this name to hardness, fluidity, &c. not to sensibility, irritability, gravitation, &c. The powers being the proximate causes upon which the phenomena depend, Dr. Brown proceeds to argue that it is an "illusion, however universal, to suppose the powers of nature to be something more than the mere series of antecedents themselves." (1) Now, to our mind, no such illusion exists. On the contrary, the "powers of nature" are "universally" considered the "antecedents." The disagreement consists in one sect of philosophers considering the "antecedents" as specifically different from the mere essence of common matter, and the other sect recognising no distinction. One regards them as absolute existences, *sui generis*, the other considers them as nothing at all. We belong to the former "universal" sect; and cannot, therefore, agree with this profound metaphysician, that "powers and properties are only the substance itself, considered in relation to various changes that take place, when it exists in peculiar circumstances." (2) Divesting this proposition of its metaphysical subtlety, it is manifest from what is universally known of the phenomena of life, that they depend upon something totally different from the matter through which it operates. Of this we can entertain no doubt, when we consider a living animal in relation to one suddenly destroyed by prussic acid, or a division of the modulla oblongata, &c. The matter, and organization, and physical properties are the same in each; but the most remarkable of the whole,—the weights and pulleys, upon which the different movements depended in the living, have entirely disappeared in the dead, subject. Whether these forces be spiritual, ethereal, or fluid, is unimportant in a practical sense. We are only concerned about their operations as denoted by their phenomena,—about the manner in which they are influenced, and the modifications to which they are liable.

And yet with all the multiplied proof of the independent existence of the vital forces, and of their specific nature, it is remarkable that professed physiologists, of the present day, should endeavour to throw an ambiguity about them, and to substitute forces of which no more is known, and which, at the same time, it is admitted, are incapable of explaining the phenomena of life.

(1) Lectures on the Philosophy, &c. vol. i. sec. 7.
(2) Inquiry, &c. ut cit.

Nay more,—they cannot even frame their objections, without fully admitting our doctrine of life. This we have already seen; and thus, Dr. Bostock:—"These facts (certain phenomena of life) are very curious and important, and they clearly prove that there is a difference in the mechanical and chemical relations of living and dead matter; a difference which, I fully admit, we are not able to explain or account for. It is the same kind of difficulty which occurs with regard to the contractile and sensitive functions of the muscles and nerves, that they are both of them totally destroyed by the *extinction of life*, although for some time afterwards, neither of these organs seem to have undergone any alterations, *either in their chemical or physical properties*. In this, as in the other analogous cases, the doctrine of the animists proceeds upon the principle, that no modification of the laws of chemistry or mechanics can account for the phenomena, and that it is consequently necessary to assume the existence of some new agent to meet the emergency. But I may remark, as I have on former occasions, that by this proceeding we throw no *new light* upon the difficulty, and that in reality, we are only employing a different expression to announce the fact, and one which is less simple and intelligible."(1) But however "simple and intelligible the laws of chemistry and mechanics," was it not "fully admitted that they are not able to explain or account for" the phenomena? Nor can they explain a single phenomenon of life. But we shall endeavour to show that the forces of chemistry and mechanics are less "intelligible" than those of life; nor is it in the least an "assumption," as stated by our author, that the vitalists infer the existence of powers wholly different from those of "chemistry and mechanics," since the phenomena of life are wholly different from those of inorganic matter,—and since, also, the existence and nature of the forces of chemistry and mechanics are alone inferred from their phenomena. The proof is clear, and tangible. Where the results of power differ so materially from each other, it is as good a ground of argument, that the phenomena depend upon specific powers in one case, as in the other; and, if it be a cloak of ignorance in either instance to *assume* the existence of powers, it must surely appertain to him who attempts an explanation of the phenomena, by assuming forces with which such phenomena have no known connection.

(1) Elementary System of Physiology, vol. ii. c. 10, § 4, p. 409.

SECTION II.

THAT we have not been assuming any ground in what we have now said as to the encroachments of chemistry upon the science of life, is, perhaps, sufficiently obvious from the foregoing authority, and may be admitted without objection. But since we shall be much concerned with this subject in subsequent essays, we think it important to state the general theory in relation to chemistry, and the facts by which it is supported, in the language of another able philosopher of the chemical school. We refer to Dr. Prout.

Adopting the opinion and language of Berselius, "as the history of organic existence," he says, "a living being, considered as an object of chemical research, is a laboratory, within which a number of chemical operations are conducted. Of these operations, one chief object is to produce all those phenomena, which, taken collectively, are denominated *life;* whilst another chief object is to develope gradually the corporeal machine or laboratory itself, from its condition in the existence of an *atom*, as it were, to its utmost state of perfection." "This," says Dr. Prout, "is the history of organic existence"! (1)

(1) Chemistry, Meterology, and the Function of Digestion, considered with reference to Natural Theology, book iii. ch. 1.

We shall see, in another place, that some physiologists have taken up this theory in a practical sense, and profess to have succeeded in forming muscles out of albumen, and insects out of silex, by the aid of galvanism. According to the foregoing doctrine, too, there was no necessity for a First Cause as it respects animals, — so only that He provided "atoms" and chemical forces. The fluid ovum springs originally from the concurrence of "atoms" and the chemical forces. The semen, in virtue of the coadaptation of its own chemical virtues, forms a chemical union with the fluid ovum, which, by the same physical influences, is gradually fashioned into all sorts of exact forms, and thus "developed into an animal machine or laboratory, from its condition in the existence of an atom, to its utmost state of perfection." The new formed animal now possesses the same form, functions, sensations, constitution, disposition to the same diseases, and a soul, which distinguished the parent. If the horse copulate with the ass, a mule is the chemical result, which is nearly incapable of carrying the forces of chemistry any farther, as it respects procreation. So, also, of the bull and mare, the ass and cow, &c. Nature here generally restrains the chemical forces, and puts an extinguisher upon the monstrous product.

Such is the noble work of chemistry; whilst it is the peculiar province of the chemist to expound the manner in which "the corporeal machine or laboratory itself is gradually developed from its condition in the existence of an atom, as it were, to its

A mere glance, it would seem to us, at the nervous system,—its complexity, distribution, and astonishing phenomena, even in relation to organic functions, would satisfy one that there is something very essential left out of this definition of life. Again, if heat be applied to a new-laid egg, we soon discover that an organized, self-moving being, growing interstitially, is the result. But if another egg chilled by cold, or subjected to a current of electricity, (that great "essence of life,") be exposed to the stimulus of heat, we find it speedily resolved into its ultimate elements. Here, it is manifest, that both results are equally brought about by the agency of heat; and it is equally clear, therefore, that it has acted through totally different forces in the two cases. The former is the organic, the latter the chemical forces; and the difference is so vast, that it seems almost absurd to suppose that the latter had any connection with the development of the chick. If, then, this be conceded, it is plain, by the clearest analogy, that all the subsequent processes of life are carried on by the same organic force, and that those of chemistry are hardly likely to be superadded at any stage of existence.

Again, "we may view," says Dr. Prout, "an organized being

utmost state of perfection" in body and soul. "The chief object of the chemical operations," afterwards, "is to produce all those phenomena, which taken collectively, are" vulgarly "denominated *life*." "This is the history of organic existence."

We are told by another, who maintains that "the formation of any one of the peculiar *secretions*, the essential proximate constituents of which do not exist in the blood itself, presupposes the operation of a *special* chemical apparatus, whether this be a membrane or a gland," — we are told, that "however various the form of their elementary parts, all secreting glands without exception, (not only those of the human body, but all met with in the animal kingdom) follow the same law of conformation, and constitute an uninterrupted series from the simplest follicle to the most complex gland." (*a*) Does this most astonishing harmony, and unity of design, and development, appear like the ordinary effects of the chemical forces in the arrangement and combination of atoms? Every species of animal, too, has its specified conformation. But "no one kind of conformation," says the author just quoted, "is peculiar to any one gland. Perfectly different glands may have a similar elementary structure, as is the case, for instance, with the testes and the cortical structure of the kidney. And similar glands have often a perfectly different structure in different animals. How various, too, is the elementary structure of the liver in the animal series; in one case being represented by *simple cæca*, — in another by *tufts of cæca*, — and others again, by *bunches of cells*, by a *spongy mass*, — or, lastly, by *branched ducts* ending in *feather-like terminal twigs*," (*b*) to which our author affixes an "!" It is certainly *very astonishing;* and when we consider that all this variety of apparatus produces in all a very analogous secretion, and always the same in each species, it must appear appalling to any theory that chemistry may devise. But more of this hereafter.

(*a*) Müller's Elements of Physiology, vol. i. pp. 431, 456. (*b*) Ibid. p. 460.

as a piece of intricate machinery, adapted to the physical and chemical properties of matter," &c.

And now for the proof of the foregoing theory of life. "The most determined skeptic," says Dr. Prout, "cannot assert that there is *any* necessary relation, or, indeed, *any relation whatever* between the *mechanical arrangements* and *the chemical properties to which they administer*. There is *no reason* why the chemical changes of organization should result from the mechanical arrangements, by which they are accomplished; neither is there the *slightest reason* why the *mechanical arrangements*, in the formation of organized beings, *should lead to the chemical changes of which they are the instruments*."

Thus discarding every ground for the chemical theory, and even despairing of the aid of galvanism in such a state of absolute contradictions, the chemist is driven for help to the Great-First-Cause. (1) "The existence," says Dr. Prout, "of *things so entirely dissimilar*, and *having no kind of mutual relation*, can be explained only on the supposition that a *will exists somewhere*, and also a *power to execute that will*." We can readily imagine that the direct agency of such a Power would be constantly necessary to maintain in action such a machine as has been described by Dr. Prout; but for our humble selves, we can see that nothing more is wanted in our machine than the powers of life; which, we submit, will better enlarge our conceptions of Infinite Wisdom and Power, than the assumption of a machine betwixt which and its forces there is "no relation whatever."

We shall not, therefore, be suspected of irreverence, when we say, that according to the foregoing statements, the living organized system appears not to equal "*in design*" many of the contrivances of man. This will be more fully illustrated when we come to speak of the attempted solution of the phenomena of life upon the catalytic principles. And that we are in no respect unfair in this representation appears not only from the facts now before us, but from the attempt of some to create the instruments of vital actions by the chemical agent by which their functions are said to be carried on.

We have seen that it is the opinion of Dr. Prout, that "there is not the slightest reason, why the mechanical arrangements in

(1) In our second volume we shall farther advert to Mr. Carpenter's views upon this subject.

the formation of organized beings should lead to the chemical changes of which they are the instruments." For this fact we think a reason may be assigned. All organized bodies are composed of certain dissimilar parts, yet all adapted to the condition and actions of each other, and contribute more or less to each other's functions; whilst it is just the reverse of this in inorganic bodies. There is no sort of relation betwixt them, unless in symmetry of form, — whilst their modes of increase are totally different. The organic force, in connection with matter, is creative. It exists in the egg and seed, and may be brought into creative action by certain chemical stimuli, and go on when those agents are withdrawn. This force exists, also, in every part; but such is its dependence upon a mutual co-operation of many parts, that when one of them is removed, the force may be destroyed in the whole. The organization of each species is forever the same, — the vital and physical results always the same, — and, what could not exist if chemical laws operated, the temperature in all the higher animals is uniform in each species, though varying in different species. We should, therefore, infer *a priori*, that living organized and inorganic matter are governed by dissimilar, if not opposite, forces.

But what strikes us as being a most remarkable conclusion from the premises laid down by Dr. Prout, and after so full an admission that there is not the least appearance of any unity of design, is the declaration that the foregoing is "the only possible explanation of the subserviency of mechanism to chemistry, in the processes of organic life. And what is this explanation, but *our argument of design*, in terms that seem absolutely irresistible?" (1)

Nor can we forbear noticing Dr. Prout's argument in favour of "the chemical changes of organized beings." "The performance," he says, "of the chemical changes within organized beings, through the interposition of mechanical arrangements, as has been stated in a former part of this work, establishes, beyond a doubt, that these chemical changes have a real existence."

Without saying that the latter clause of this quotation is but a repetition of the first, we shall be saved the trouble of proving, that Dr. Prout failed of showing the adaptation of the mechani-

(1) Op. Cit. ch. 4.

Dr. Billing has a very similar account of animal life, and like Dr. Prout, is disposed to regard it with satisfaction.(a)

(a) Principles of Medicine, p. 20.

cal arrangements of organized beings to chemical changes; since, in endeavouring to defeat the sophistry of the "skeptic," he had declared that "there is no reason why the chemical changes of organization should result from the mechanical arrangements, by which they are accomplished; neither is there the slightest reason, why the mechanical arrangements in the formation of organized beings should lead to the chemical changes of which they are the instruments."

Such must ever be our position, when we attempt an explanation of the phenomena of the vital forces by the instrumentality of other powers with what they hold an absolute hostility. The foregoing admissions are sufficiently conclusive, in themselves, that all the phenomena of life depend upon causes that have no relation to chemical forces.

But it is not our purpose, at any time, to criticise the opinions of an individual. We have chosen to bring our own views in contrast with those of more able men, that, in so doing, the merits of our adversaries may have their proper weight. We shall constantly select, for this purpose, the best authorities, and exhibit their strongest arguments and facts. This will also enable us the better to lay the whole subject before the reader, whilst we endeavour to meet the most serious difficulties as we go along.

There is a paragraph soon following the foregoing quotations which is too inviting to escape notice. "The desire of the physiologist," continues Dr. Prout, and this in a work intended to illustrate the wisdom and power of God,—"the desire of the physiologist to ascribe to the agencies of inorganic matter those operations carried on within living bodies, is merely a display of that innate propensity of the human mind which leads us to seek after first causes."

Now we feel ourselves entitled to declare that the *physiologist desires no such thing*. It is the *chemist*, and not the physiologist, who "ascribes to the agencies of inorganic matter those operations which are carried on within living bodies;" and few have carried the doctrine farther, as we have shown in another place, than Dr. Prout. The physiologist flatters himself that he mounts a step higher in his approximation to the First Cause of all causes, when he abandons the "agencies of inorganic matter," and opens a world of life carried on by living powers.

Whilst, however, the physiologist admits that all is exact, harmonious, and sublime in the world of mere matter, he perceives, in the higher department of nature, another and a higher creation, where all analogy ends, but that which serves through the medium of matter alone to maintain an unbroken chain. Nor is it the least of his gratifications, that a power has been vouchsafed to develope this stupendous plan; and by tracing the organized world from its incipient formation, and by comparative observations amongst the various species of which it is composed, to demonstrate an astonishing uniformity in their mode of construction, (different, though uniform, in animals and vegetables,)—the same, or often a greater exactness than prevails in the inorganic world, and the same uniformity of the laws by which the whole is governed.

Let it not then be said, that "chemistry, in its connection with the laws of life, would become the most sublime and important of all sciences;" (1) but considering the utter failure of the former in all its pretended relations to the latter,—that it has brought confusion into physiology, and absurdities into the philosophy and treatment of disease,—that it cannot explain one phenomenon of life,—or yield two consistent analyses of the same organized part, or even of its secretions,—or recombine the elements it dislodges,—that it confounds animal, living and corrupt, with a variety of vegetable substances,—and reduces the organized machine to a mere chemical laboratory;—let us, then, maintain for the physiologist that "sublime and important" ground, which the chemist would vainly occupy.

"The changes which characterize living beings, and which in their totality constitute their life, are capable of being referred to certain general laws expressive of their uniform conditions. Organized structures possessed of vital properties, on the one hand, and the elements of the inorganic world on the other, afford these conditions; and by the study of all the phenomena which are presented by the actions thus resulting, the science of physiology will, we doubt not, be gradually built up and perfected." (2)

It was doubtless, in part, an appreciation of the powers of life, as contradistinguished from those of mere matter, which led Hippocrates to say that "a philosophical physician is like a

(1) Sir H. Davy's Essays on Heat, Respiration, &c.
(2) British & Foreign Medical Review, vol. vi. p. 106.

god." Nor can we imagine a more gratifying consciousness to the physician, than that, by laborious application he comes, at last, to enjoy that philosophical knowledge which distinguishes him from the rest of his race,—that he has some glimpses at a system of forces and actions, of which the profound in other sciences have no just conception,—that he alone is qualified to direct those powers to a great and specific result.

To the physiologist the forces of life stand in exactly the same relation as those of chemistry to the chemist, or gravitation to the astronomer,—or mind to the metaphysician; and he maintains that he knows just as much of the existence and nature of the vital forces as is known of any of the other powers of nature, and that they are as clearly pointed out by their phenomena, and that we may reason as philosophically about one as the other. A twinge of the toothache is not without its cause. This cause is seated in the nervous system, and he would be considered a dunce or a madman, who should attempt its explanation by any other than what are called vital properties, or who should not consider the property on which it depends, totally unlike anything that appertains to the inorganic world. This property, for the sake of distinction, is called sensibility. Just so is it with all the other properties of life,—irritability, contractility, &c. Like sensibility, they are known by their effects; and their effects are equally known to be peculiar to living organized matter. If the chemist, or natural philosopher, or the metaphysician, will show us any greater knowledge of the powers with which they are conversant, we will give up the argument.

This is all we know of the Almighty Being, and could we see Him, we should know Him no better, and probably should revere Him less,—at least it was so once. But we know His Existence and Nature, (in a natural sense,) just as we know the existence and nature of the vital forces, and in no other way. We also infer His Existence, because we know that matter and the laws which govern it, cannot have been self-existent. So, also, we irresistibly infer the existence of peculiar vital properties, because we know that there are no properties appertaining to dead matter that can produce the phenomena of life. The latter are just as much a direct emanation of the forces of life, as the work of creation is the work of God; and to deny the existence and peculiar nature of the former, because they are not

comprehensible, in the popular sense, affords an equal ground for denying the existence of mind and the Supreme Being, and their attributes. But in respect to the Deity, man is so apt to reject the evidences of nature, where the causes cannot be seen, that He was graciously pleased to announce His Independent Existence, when He pointed out the dependence and the duties of man.

Even admitting, that we could see, and feel the causes of gravitation, cohesion, affinity, &c. we should still know nothing more of their nature and their modes of action. Their existence might be a little more palpable to those who rely more upon sense, than upon the understanding. But even in this respect, the power of *sensibility* has the advantage, — since the consciousness of its existence is enforced through every sense.

It is said by Sir William Temple, that "when man has looked about him, as far as he can, he concludes there is no more to be seen; when he is at the end of his line, he is at the bottom of the ocean; when he has shot his best, he is sure none ever did, or ever can, shoot better, or beyond it. His own reason he holds to be the certain measure of truth; and his own knowledge of what is possible in nature."

We claim, indeed, for the forces of life, a clearer evidence of their existence and nature, than can be justly predicated of the forces of physics. (1) Irritability, contractility, vital affinity, sensibility, and sympathy, are all distinctly marked by peculiar phe-

(1) "The principle of life," says Mr. Hunter, "is perhaps conceived of with more difficulty than any other in nature, which arises from its being more complex in its effects than any other; and it is, therefore, no wonder that it is least understood." (a)

This appears to us to be only true as it respects an analysis of the vital phenomena. Every phenomenon of life does but illustrate, more and more, the nature of the principle and its independence of all other forces. It is the same in this respect as with the mind. No one would say that the complex operations of the latter render it "less understood," than if they were more simple, or there were but a single act of intellection. Should we understand electricity better, if it only manifested a luminous appearance? And so of everything else; the more multiplied its manifestations, the better do we comprehend its nature.

We cannot but think, too, that Mr. Hunter, who had such luminous conceptions upon this subject, in endeavouring to make it clear to others, has embarrassed its simplicity by attempts to illustrate it through the phenomena of magnetism, light, &c. between which and the vital principle there appears to be no analogy whatever. It would be an easy matter, also, to show the total want of any parallel between the mechanism and force of the watch and those of organized beings, — and that all illustrations of the latter, drawn from the former, are worse than useless.

(a) Lectures on the Principles of Surgery, p. 19.

nomena throughout the animated world. These phenomena are, also, vastly more multiplied, and more imposing, than those which appertain to dead matter,—whilst they hold no analogy or fellowship with each other. The proof is so various, so distinct, so irresistible in respect to the former, that consent would seem to follow unavoidably. Such is their character in their normal state. But, when external agents operate upon them, all their phenomena may be modified, and their distinguishing characteristics greatly multiplied,—till again we see them returning to their natural aspect;—or, as in the fatal termination of diseases, they totally disappear along with the manifestations of mind, or of instinct.

Who shall say, that the fall of a stone, the motions of the planetary system, &c., more clearly evince their dependence upon the power that is called gravitation? The ocean rises and falls; but this is only another coincident effect, and brings no variety to illustrate the force. The want of greater variety leaves the mind in doubt; and it is nothing but these simple effects, which imply a cause, that satisfy all that some unknown force prevails. But its nature, whether material or immaterial, an emanation or an oscillation, heat or cold, light or darkness, no one can divine. Still there is something,—a sort of spell, which holds all matter, in one uniform way, under its control,—save only, in its proper sense, the living organized being. And here we see the forces of life contradistinguished from the physical, as they are from the chemical,—since the forces of life may partially overcome the force of gravitation. The man walks,—the bird flies. It will not, of course, be said that so also does the bow send forth the arrow, and the powder, the ball,—for there is no analogy in the cases. In the latter, gravitation immediately overcomes the propelling force, whilst in the former case it exerts no control. The leg may be elevated for an indefinite time; and if it be at last brought to the earth, it is by the same power that had overcome the force of gravitation. In one respect, however, there is a specious analogy. The muscular power may become exhausted, as the force of the bow is spent when elasticity has done its work. In the former of these cases, gravitation may bring the leg, as the missile, to the ground. But it had no more to do with muscular exhaustion, than with the extinction of the elastic power. So in respect to all mechanical forces,—steam, &c. Their ope-

ration depends upon an artificial maintenance of the cause; and this depends, ultimately, upon the agency of the vital forces.

The forces of life are inherent in every part of living matter, and may forever operate *per se*, and resist all other forces. But in all the other instances, as elasticity, expansion, and all mechanical forces, they are not the sources of actual power. Self-action, then, is a distinguishing characteristic of animals and vegetables; for the principle reaches every part of their organization.

Is it said that magnetism will overcome the force of gravitation? Before this force can make any demonstration, the object which is attracted must be brought within the influence of the magnet,—and this by the agency of the vital forces. Those forces, also, may simultaneously overcome the force of gravitation and that of the magnet. This is shown by holding the object within the influence of both forces. Or, it would be more exactly stated, by saying, that in overcoming the magnetic attraction, the force of gravitation is also surmounted, since that of magnetism is now supposed to be the greater. But, what especially destroys the parallel with the organic force is the fact that there is nothing inherent in the magnet, as in the muscle, which can raise it from the earth. One has power of acting within itself, the other has none at all.

In respect to the forces of chemistry, is it known with any certainty, what particular causes are instrumental in effecting the chemical union of different species of matter? Are there not most remarkable revolutions in opinion as to the nature of magnetism, electricity, heat, and light, and the laws by which they are governed; and is not the doctrine of oscillations liable to be supplanted by another, to-morrow? Indeed, at this moment, there exist the theories of emanation and undulations of light. But whether one or the other be true, it affects, in no degree, our knowledge of the laws of reflection, refraction, &c., by which the specific existence of light is ascertained. And although the phenomena of light are thus diversified, and present, as it were, a variety of laws, we know less of light than we do of darkness. Finally, all that we know of matter, and of the sources by which this knowledge is acquired, arises from effects.

Dr. Billing goes a little farther for mechanics than many other

philosophers of his school, but mostly for chemistry. "Are the secretions and exhalations," he inquires, "modified by the *calibre* of the minute branches *admitting* only the vapoury parts to the surface of the serous membranes and of the skin, whilst they permit the transparent fluid parts of the blood to pass to the mucous surface, and keep back the red globules? This mechanical explanation might suffice, in part, in the instances adduced; (1) but when we come to the nutrition and renewal of muscle, and the formation of peculiar secretions, we must look for some still uncomprehended agency, which modifies the materials conveyed by the arteries, whilst they are depositing," &c. "This power can be no other than chemical."

Now, in the first place, this statement carries on the face of it its own refutation,—since it is affirmed that the "power" is an "uncomprehended agency." But it is always so with every chemical hypothesis we have yet seen,—their authors forever admitting, as we shall show in many instances, that there is no meaning in what they say. As to the foregoing clause which embraces the mechanical doctrine, we shall defer its consideration for another place. But we have yet another account with Dr. Billing. He affirms that the foregoing "processes, when examined, will be found to be chemical precipitations, by which new matter is deposited," &c. Now we utterly deny this, and call for the proof; whilst on our part, we shall hereafter assign many objections to the doctrine. If the organic formations, secretions, &c., be composed of the elements of inorganic matter, it is no proof that they were prepared and united through the chemical processes of decomposition and recombination; nor has any chemist ever been able to imitate a single animal or vegetable product.

Our author, however, is embarrassed by the inability of the chemist "to construct a man;" since, as we understand him, "we cannot discover how, in the minute embryo in the womb, the membranes were first determined in their shapes; we here arrive at the confines of our knowledge;" (2)—all beyond is plain chemistry.

Our author is also pleased to say, that Bichat's opinion that "the nerves are strangers to organic sensibility leads him to be-

(1) Our author says, that when cantharides fail of producing vesication, or a secretion of serum, "the cause here is a want of injecting force in the heart." (p. 23.)

(2) Principles of Medicine, pp. 5, 6. 1838.

come *visionary*, when he speaks of the lacteals exerting a choice as to what particles they will take up. This is mere *chemical* or *nervous* effect on their mouths," &c. But we think our author has mistaken the "visionary" man, as we have endeavoured to show in other places; although we go with our author in believing that the nervous influence has an agency; but not in the least in the sense in which he employs it. Our author is also pleased to say, that "I think I have shown, that what he (Bichat) calls *vital laws*, are but these *mechanical laws* modified by the Deity."(1) Now we have seen nothing to this effect in our author which is not embraced in the foregoing quotations. But suppose our author has shown it; what is left for the forces of chemistry with which our author originally started as the great agents of life, if the mechanical powers are identical with, and make up the vital?

Suppose, again, however, that a fair share of the work be given to both the chemical and mechanical powers,—how can our author say, that "such perfection in our organization leads us to believe with Job, that, however disarranged by death and decomposition, it may be again called into re-union, and that *in our flesh we shall see God?*" What is there in our mechanism compared with the astonishing forces with which it is brought into existence, and maintained against the invasion of those very forces by which our author would have us believe the whole work of life is begun and completed; but which, at last, as he admits, turn upon themselves, and lay waste their beautiful fabric? How can our author found an argument as to the reunion of the elements of our bodies, upon the premises that the living organization is carried on by the forces of physics, and that its dissolution is brought about by these forces? Whoever believes that God formed man out of the dust of the earth, can have no hesitation in admitting that He can form him again after he shall have returned to the dust. Our whole argument, therefore, upon this question, must rest entirely upon Revelation.

If we admit our author's doctrine, it would be an equally sound conclusion that the mechanism of a chronometer will resume its arrangement after it shall have been dashed into pieces. The principle is the same, however one machine may be more complex than the other. There is nothing, *per se*, in either,

(1) Principles of Medicine, pp. 19, 20. We are quite willing to take such forces for our vital ones, — so only they be sufficiently "modified."

that denotes the final cause indicated by our author. But there may be a ground for such an induction in the partial analogy which subsists between the forces of life and the immortal soul, —since there is impressed upon the latter a consciousness of its perpetuity.

Why then, in a disquisition upon the forces of life, this resort to the plan of organization? It proves nothing as to the ultimate designs of the Deity; but like a conclusion drawn by Dr. Prout, as we have already seen, it only serves as a decoy from the great question at issue. The derivation of the body from inorganic matter, its incessant mutations, its never-ending derangements, its more rapid progress than other matter to a dissolution of its elements, are all adverse to the Divine doctrine of its resurrection. Every thing but Revelation is opposed to the conclusion. Our author's expedient, therefore, only shows by inference, that if the chemical and physical forces were once capable of constructing such a fabric, they may again put the elements together. Indeed, if this be our author's argument, we agree with him that they would be all the better qualified; not only as journeymen of experience, but, having taken the machine to pieces, would better know how to put it together again. We believe with him in the resurrection of the body. To show him, however, that the mechanical laws will be again "modified by the Deity," we are told that the body shall be raised "a spiritual body;" which is also a farther proof to our mind, that these assumed modifications of the mechanical forces are, *de facto*, specifically distinct from the forces of physics,— a direct *fiat* of the Deity, consentaneous with the soul and with instinct, and forming a simple type in the vegetable kingdom. We shall resume this subject again, in examining the arguments of Dr. Hodgkin and others on spontaneous generation.

We may say here, that Dr. Hodgkin remarks: "I employ the term vitality, for want of a better, to designate a condition, which I think you will understand when I have stated some examples of the class to which I have alluded. I employ the term vitality here as well as on a former occasion, without any connection with the mystification which sometimes attends its use."(1) Very well. But our author employs the term, because he cannot get along without it. He would have been perfectly unintelligible,

(1) Lectures on the Morb. Anat. of Serous and Mucous Membranes, vol. i. p. 272.

had he spoken of gravitation, cohesion, chemical affinity, &c., nor would he have understood himself. Our author soon after speaks of "that *mysterious sympathy* which connects particular organs, both pathologically and physiologically:" (1) nor could our author's valuable work have had an existence, were it not concerned in most of its parts with a philosophical, however unintentional, interpretation of the vital forces?

We have already seen that this indisposition to admit the existence of the forces of life to be *something*, proceeds from its want of common analogies with sensible objects, and the difference in its laws. But we have seen, too, that upon the same ground we might deny the existence of the soul; and, indeed, this forms the ground of objection with the materialists.

For the same reasons, heat, light, electricity, and magnetism, are supposed to be non-entities. Thus, Dr. Elliotson: "I have adopted the common language in speaking of animal heat, *as though* the phenomena *depended upon a specific substance*. However, there may be every reason to believe that neither caloric nor light are fluids, but peculiar states only; and electricity may prove to be so likewise; and, perhaps, all these to be modifications of the same state." (2) But state of what? Certainly, either of something or of nothing. If the latter be most intelligible, why did our author "adopt the common language in speaking of animal heat?" Was it not for the same reason that Dr. Hodgkin employed the term vitality?

We are advocates for regarding nature in all possible simplicity, and can see no objection to the supposition that light and heat, and perhaps electricity, may be different modes of a common substance, whose constitution is so different from that of common matter that we cannot reason from one to the other.

(1) Lectures on the Morb. Anat. of Serous and Mucous Membranes, vol. i. p. 275.
(2) Human Physiology, part 1. p. 247.

We have the following view of this subject by Dr. Buckland. "It appears highly probable from recent discoveries, that light is not a *material* substance, but only an effect of undulations of ether; that this infinitely subtle and elastic ether pervades all space, and even the interior of all bodies. So long as it remains at rest, there is total darkness; when it is put into a peculiar state of vibration, the sensation of light is produced." (*a*)

Here, then, we have a tangible something, — not unlike our vital principle, — only, perhaps, a little more material; and the functions which are ascribed to it, — light, heat, and electricity, are quite as diversified as what have been supposed to appertain to the principle of life.

(*a*) Geology, Mineralogy, &c. vol. i. p. 35.

as we shall have frequent occasion to show, that is brought in favour of the chemical and physical doctrines of life.

A variety of hypotheses have been offered to expound the manner in which the forces of chemistry find their way into those minute vessels where the vital forces hold their empire, and how they exert their power upon the living blood, in breaking up and recombining its elements. The most favourite, because the most universal, mysterious, and undefined chemical agent, is that which refers all to galvanism. But it is the error of all, that they are founded upon premises either wholly assumed, or not considered in relation to accompanying facts with which they are entirely at variance. The latter is remarkably true of the experiments of Dr. Philip and others with galvanism; and as a specimen of assumption, we may offer the following from that able and indefatigable man, Dr. Edwards.

"If we seek among the facts known in chemistry," he says, "for an explanation of the difference between the constitution of the blood and that of the fluids secreted from it, we may soon be convinced, that the action of the voltaic pile is the only one which approaches it. Moreover, it appears possible, *artificially to imitate the principal conditions of the secretions*, and to separate from the blood, by means of the pile, a liquid resembling *milk*, and from the food itself, a material resembling chyme." (1) The whole of this statement is without any foundation; and yet the most important principles concerning life, disease and its remedies, have been founded upon it.

We have noticed, in the course of our work, other hypotheses more or less analogous. Each succeeding theorist, not finding the existing ones explanatory of any of the phenomena of life, tries his hand at something new. We ought not to omit the latest, by Misterlich, Gmelin, and Tiedemann. The lungs are made, by these eminent men, the great laboratory where all the chemical changes are carried on, and where "the organic products necessary to life are produced." The hypothesis is thus

" that some such general law as that of *gravitation*, which governs the material world, will be discovered to govern and regulate all the phenomena of organized beings." (a) The objection to this conclusion consists in the want of all analogy between the phenomena of life and those of inorganic matter. We cannot, therefore, reason from one to the other.

(1) On the Influence of the Physical Agents on Life, p. 305.

(a) On the Nature and Treatment of Diseases of the Heart, part I.

expressed by Müller. "They have lately proposed a perfectly original theory of respiration. The facts on which they ground their opinion are the following: that acetic and lactic acids exist in the free or combined state in most secretions, and also in the blood; and these acids, they say, must be generated in the animal body itself. They have farther ascertained, that *venous* blood contains more *alkaline sub-carbonates* than *arterial* blood. They suppose, now, that by the free contact of the blood with the air during respiration, *acetic acid is generated*, which decomposes the alkaline carbonates of the venous blood, and sets free the carbonic acid,—that the oxygen of the inspired air unites, in part, directly with carbon and hydrogen, and forms carbonic acid and water, and in part enters into combination with the organic compounds contained in the blood; *the result of which* is, that *organic products which are necessary to life are produced, as also lower organic products.*"

We have marked the points most important for consideration; and as they have come more or less under our review both here and in other places, we shall add no farther comments. We should cheerfully concede the ingenuity of the hypothesis, were it ascertained that the premises are just, and were it not so comprehensive as to leave little or nothing for the stomach, the liver, the general capillary system, &c., to perform. But it seems likely, already, to be superseded by some other,—since we are told by Müller, that "while the existence of gasses in the blood itself was doubtful, this was an ingenious mode of explaining the phenomena; but Gmelin himself has since recognised the existence of carbonic acid in the blood." (1)

(1) Elements of Physiology, vol. i. p. 335.

It is computed by Magnus, that a pound of venous blood contains five cubic inches of carbonic acid.

The foregoing hypothesis concerning respiration leads us to notice another which came "dead-born" from the author whom we have just quoted. We speak of it especially to show the hypothetical nature of all similar speculations.

"The cause of the first respiration," says Müller, "appears to me to be solely the stimulus afforded to the brain and medulla oblongata by the blood, which immediately becomes oxydized in the lungs. The former had been in a comparatively sluggish, torpid condition; but the arterialized blood, in a few minutes, reaches the brain, when the respiratory movements immediately commence." (*a*)

Here, the able translator remarks, that "before arterialized blood can reach the brain, respiration must have commenced." He inquires, "how is the air first drawn into the lungs?" We answer, that this appears to be one of the numerous secrets

(*a*) Ibid. p. 355.

One of the best things ever said by M. Andral, we find in a note to his ingenious work on the humoral pathology: "To admit the existence of a *vital fluid*," says this eminent man, "to explain the phenomena exhibited by living beings is imitating the natural philosophers, who have long distinguished by the name of *electric fluid* the unknown agent of a force that displays itself in nature by the appearance of a determinate series of phenomena. But, until it is proved that the forces which, in a living body, interrupt the play of the natural chemical affinities, maintain a proper temperature, and preside over, the various actions of organic and animal life, are analogous to those admitted by natural philosophy, we shall act consistently with the

of animated nature, which the ingenuity of man will never be able to explain. There are probably very complex causes involved in the process, the most essential of which may be totally hidden.

Müller gives us, in this place, many curious speculations of the moderns as to the cause of death when the nervi vagi are divided. He finally adds the opinions of each observer together, as forming the true philosophy. (*b*) But in this estimate of causes, the calculation does not embrace the interruption of the natural relation of sympathies, and the pernicious influences that are exerted upon various organs through the principle of sympathy, and the injury done to the organization and vital forces of the stomach and lungs, — which seem to us to be among the principal causes of death. The causes assigned are more the results of the foregoing impressions, and they rather form a consecutive part of the series of changes in the process of death, — every change of this nature, however, adding to the fundamental evil. This may be illustrated by dividing the nervi vagi of a new-born puppy, — when the animal dies in half an hour, — or, a full grown dog in one or two days. But tracheotomy prolongs life somewhat in young animals under these circumstances. Now it was found by Le Gallois, Edwards, and others, *that an animal lives longer without respiring, the younger it is*. But according to Dupuy, and others, older animals which die as soon as the young, after a division of the nervi vagi, live many days longer if tracheotomy be performed. This shows, at least in respect to young animals, that the opinion can hardly obtain, that "the incomplete paralysis of the muscles of the glottis, and the effusion of bloody serum into the lungs, produce the final suffocation and death which follow the division of the nervi vagi."

But what is true of any great principle in relation to young animals is likely to be so, in the main, in respect to older ones. The young live longer without air, than the older, in the natural state of their organs; but when the nervi vagi are divided, and tracheotomy is performed, the life of young animals is but a little prolonged. But it is otherwise in the adults. This shows that habit has established greater relations betwixt the blood and the vital forces of adults, by which respiration will, in a measure, counteract the pernicious influence of a division of the nervi vagi; but it does not appear to show that when death speedily follows a division of the nervi vagi, without tracheotomy, that death is not mainly produced by this direct impression upon the organic powers and functions, — especially since tracheotomy prolongs but little the life of new-born animals.

(*b*) Elements of Physiology, pp. 355, 358.

principles of that science, by giving distinct names to those two kinds of forces, and employing ourselves in calculating the different laws they obey." (1)

A very plausible hypothesis, and the best we have seen, has been projected by those who profess not to subscribe fully to the usurpations of chemistry, to account for the results of vital processes upon other principles than the forces of life. This is by what Berzelius calls the *catalytic forces* and *catalytic actions.* Still it is nothing but the chemical doctrine practically applied.

"When a jet of hydrogen falls upon spongy platina," it is said "the platina determines combustion and the formation of water." So does iridium and other substances, without entering into combination with the new-formed products." It is also said, that "at certain temperatures, iron disunites the elements of ammonia, but does not combine with them." Analogies are not uncommon. Now, this is carried up, by way of analogy, to the living vessels and pores of animals and vegetables, as if there was a complete resemblance betwixt the properties of spongy platina, iridium, and a heated gun-barrel, and those of arteries and excretory vessels. But even in "the recognition of analogies between organs, anatomists are now aware of the necessity of resting their comparison upon the elementary structure of organs, their connection with each other, and the changes they undergo during the progress of their development. Neither of these grounds of judgement can be safely trusted to alone." (2)

"Pore-action," say our friends, "is unquestionably a catalytic phenomena." But how does it resemble catalytic action in inorganic matter? A writer now quoted shall tell us. "The particles of which the walls of organic tubes consist bring about changes strongly resembling the results of chemical affinity, *and yet differing essentially from all kinds of chemical phenomena.*" (3) This is exactly what the vitalists assert, so far as new elementary combinations are concerned. They desire no greater admission. But many constituents of the secretions and excretions, and the materials of which every part of the body is composed, exist in the blood in a state more or less analogous to

(1) Patholog. Anat. vol. i. p. 421.
(2) Mr. Carpenter, in Edin. Philos. Journ. July, 1837.
(3) Prof. Draper, in American Journ. of Med. Science, vol. 21. p. 127.

the new formations. The principal office, therefore, of the vessels concerned in these processes is to eliminate the numerous substances,—not to form entirely new combinations; but to modify the existing combinations in a way wholly unknown to chemistry. But the constituents of the blood, of which we are now speaking, undergo a change during their elaboration; though this change is constantly such as to defy all chemical analysis, and cannot be explained by any of the known forces of chemistry. (1) We shall have occasion, also, in another place, to trace the conversion of food into blood; and although by the foregoing analogy, we should be warranted in the conclusion that this conversion is not of a chemical nature, we shall endeavour to show that chemistry is scarcely, if at all, concerned in the process.

If there be some exceptions to the general fact which we have just stated, they only embarrass still more the chemical doctrine, since the new combinations are formed in conjunction with the elimination or partial modification of the existing specific compounds. Both, it is obvious, are simultaneously produced by a common process; and the former, as well as the latter, are always precise and unique. To explain all the variety, and all

(1) Müller says, "If the new organic matters formed by the secretions,—such as picromel, casein, mucus, gluten, &c., are not taken into consideration, the blood will be found to contain the proximate elements of all the *solid* parts of the body." (*a*) And yet, farther on, he states that "the opinion that all the component elements of the organs exist in the blood in their perfect state cannot possibly be adopted. The components of most tissues, in fact, present, besides many modifications of fibrin, albumen, fat, and ozmazome, other perfectly peculiar matters,—nothing analogous to which is contained in the blood. The substance of the vascular tissue, the different glandular substances, also, cannot be referred to any of the simple components of the blood. Even the fibrin of muscle cannot be considered as exactly identical with the fibrin of the liquor sanguinis." (*b*)

In the latter instance, it had become indispensable to our author's microscopical theory that the capillaries have no open orifices, that this view of the subject should supersede the former; and it affords a rare instance of arriving at a great truth when we are constructing one hypothesis to serve the cause of another.

Now, if what Müller states be true, and we believe it cannot be controverted, we have not only new combinations differing wholly from any thing in the blood, but every elimination from the blood is so modified that it is no longer like any component part of that fluid. As to the former, no one has ever pretended that chemistry can practically account for their formation; and in respect to the latter, a greater obstacle is presented, since the modified eliminations of the component parts of the blood are perfectly appalling to the chemist.

(*a*) Elements of Physiology, vol. i. p. 132. (*b*) Ibid. p. 362.

the precision in the new or modified products, upon catalytic principles, it would be necessary to suppose that almost every vessel in the organized system has a peculiar chemical endowment; that in one, it is analogous to the property of "spongy platina," in another to a "heated gun-barrel," and so on, *ad infinitum*. Nay more, that in many parts, as in the liver, each capillary vessel should be endowed with a multitude of catalytic forces specifically different from each other, since the new formations are multitudinous. But this is manifestly absurd. By what properties, then, is it, that the liver uniformly separates and combines the materials that form bile, and in such proportions that they form nothing but bile; (1) the kidneys, urine; the ossific vessels, carbonate and phosphate of lime; the muscular, ligamentous, nervous vessels; muscle, ligament, brain, &c. Why are many of these remarkable substances so different from

(1) Bile is composed of about forty different substances, viz: in that of the ox, according to Gmelin, there is 1, cholesterine; 2, picromel; 3, biliary resin; 4, taurin; 5, cholic acid; 6, elaic acid; 7, a musk-like odorous substance; 8, the colouring matter of bile; 9, stearic acid; 10, albumen; 11, a substance of urinous odour; 12, a vegetable-like gluten; 13, casein; 14, salivary matter; 15, ozmazone; 16, mucus; 17, bicarbonate of soda; 18, carbonate of ammonia; 19, acetate of soda, and various salts of sulphuric, phosphoric, stearic, elaic, and cholic acids, with potash and soda, besides chloride of sodium and phosphate of lime.

Now, allowing only one half of these to be the product of the hepatic laboratory, we shall still have about twenty different constituents, of which the eight first are peculiar to the bile. They are always present, and in about the same proportions in the same species of animals; yet differ, but with the same exactness, in other species. All analogies in chemistry are opposed to this uniformity, and the science itself rejects all direct participation in the results. The catalytic, or some other chemical theory might bear the more strongly upon this abstract fact, if the bile were compounded of only two or three constituents. But when thirty or forty exist, compounded of a few simple elements, which are simultaneously combined in the same vessels into the foregoing numerous, distinct, and specific compounds, chemistry declares it is no work of hers. The several elements are brought into a state of intermixture during the decomposition of the blood; and were the chemical forces concerned, the influences would be so incalculably numerous, and forever varying, that the products would never be alike at any two moments of time. At one instant, there would be a non-descript, which would be followed at the next by a different combination, whilst each would probably form a simple compound, — all the elements going one way together. Of this, the chemist may readily satisfy himself by attempting the formation of bile.

It is also an important fact, that, according to M. Raspail, (a) the elements of organic products, solids and fluids, are not combined in definite proportions, like those of inorganic matter; and this we might infer, *a priori*, from the infinite varieties of the former. It appears to us an insuperable difficulty for the chemists.

(a) Chimie Organique, p. 78.

each other, formed when the organization is simple, and apparently nearly alike in the several parts? Why, on the other hand, are nearly similar products formed by organs of different, but very complex structure? According to Cuvier, in insects and zoophites, a great variety of products are formed from the homogeneous blood,—always the same in each species, but differing entirely from the common fluid, although "the process seems little else than mere transudation through a simple membrane." (1) And just so is it throughout the vegetable kingdom, whose organization is still more simple. What apparent difference is there betwixt some membranous bags in terrestrial animals and the gas-bladders of fishes? And will the chemist inform us, why the former never secrete anything but fluids, and those of different kinds, and why the latter never produce anything but gases, and those of different kinds? Why do the stomach and lacteals assimilate into one uniform fluid "all that infinite variety of substances which the voracity or caprice of mankind has appropriated for their food?" How do they reject the bile, and most other substances which they do not habitually absorb? Why is the nervous influence so much concerned in the *exact* formation of every secretion; or, perhaps, we should rather say, why will the sudden suspension of the nervous influence, or even the common changes in its influence, derange the secretions of complex organs? Do the forces of chemistry impart to the semen its procreating properties,—to the juices of the stomach those principles by which it resolves into a common substance all the varieties of food, whether in man or animals; or divest the milk of the stimulating quality which appertains to every other part of the body, to the blood itself, and by which an important final cause is answered in relation to other principles with which chemistry can have no connection? Why do various poisons, the matter of contagion, &c., produce unvarying and specific secretions,—establishing such as had no previous existence, and which no other agents can induce? Why is vitality of organized matter necessary to maintain the specific nature of the smallpox virus? (2) Why does any morbific agent alter the natural character of the secretions, and this, too, in a

(1) Leçon 23, sect. 2. art. 5.

(2) Mr. Travers says, "he has never known an instance in which the constitutional disease has been communicated after the death of the subject." (*a*)

(*a*) On Constitutional Irritation, vol. i. p. 236.

manner remarkably peculiar to the nature of the agent? Why is it, according to a distinguished humoralist, whose authority on this subject will not be questioned, that "a diseased mucous membrane presents as many varieties in the composition of its secretion as there are different degrees or modes of irritation in the membrane that furnishes it,—mucus and pus running alternately into each other?"(1) "There is reason to believe," says Dr. Cragie, "that the fluid secreted or discharged from the transparent membranes during inflammation, or as an effect of this process, is always specifically and chemically different from that which is found in a healthy state."(2) "The mucous fluids," says Bichat, "exhibit innumerable varieties in diseases, viscid or liquid, fœtid or without odour, gray, white, green, or even black in the morning, they have a thousand external appearances which evidently denote differences in their composition,—differences which chemists have not yet explained to us."(3) But we may be soon satisfied that there is nothing fortuitous in these results. We know that they depend upon specific conditions of the secreting organs; so much so, indeed, that certain vital signs being given, we may often, with great certainty, define the change that has taken place in the physical product.

Is there any intelligible hypothesis that explains how the catalytic forces, or other chemical powers, bring about the foregoing results? Is there the least analogy beyond the living organization that reflects light upon the phenomena?

What we have now stated may be said to be familiar. But does this alter the force of its application,—and is it not constantly kept out of view by the chemical philosophers? We shall therefore go on with another series of unanswerable difficulties.

If we apply ourselves to other phenomena that are connected with the physical products, we shall find many of a vital nature coinciding with them. These we must irresistibly refer to forces that are peculiar to living matter. When the vital phenomena are modified, it implies a change in the condition of the powers upon which they depend; and since the material results correspond with the vital changes, reason must assent to a dependence

(1) Andral's Patholog. Anat., vol. i. p. 297. Also, William's Lectures on Diseases of the Chest, Lect. 10; and others.

(2) Practice of Physic, p. 393.

(3) General Anatomy, vol. 3, p. 115, also p. 89.

of the whole upon a common cause. In a thousand ways we may artificially modify the vital forces of a part, and calculate, with near approach, the vital and the physical results. The art of medicine is founded upon this principle. Just as we may happen to affect the vital forces and determine a particular series of vital phenomena, so shall we obtain particular consecutive products. These are always associated. When the former appear, the latter follow. Nor can it be contended, that chemical forces must have a special agency, upon the ground that the products are of a chemical nature,—since daily observation is showing us that there is something appertaining to all the physical products of living organized matter that distinguishes them from true chemical compounds. We shall see, also, hereafter, that wherever a simple compound of this nature is presented, it is highly probable that the combination is not a natural one, but produced by the chemist himself.

Again, is it by chemical influences that the presence of food in the mouth, or its prospective enjoyment, produces a redundant flow of saliva,—or that the same phenomenon occurs when we "foam with rage?" Is it this which increases the secretion of urine, when a sound is introduced into the bladder? Is it this which determines a copious flow of the bile, when food passes the duct of the liver,—or a flow of tears when the conjunctiva, or nose, is irritated, or the mind is moved with passion,—or, the profusion of milk when the child sucks? Aye, we repeat,—why does the saliva flow in such abundance when the cook approaches,—and why do the tears run in torrents at the moment we are cast down by grief? Have you galvanism enough to explain these phenomena, or how is it generated? Will your gun-barrel help the difficulty? What has the cook to do with the galvanism,—or why should the brain send forth such a profusion of this potent fluid to the lachrymal glands, and to these alone, when it is so sparing of it in other emotions that may agitate the brain far more powerfully? Or is it, that grief is more analogous to acidulated water, or to a solution of blue vitriol, than joy, or love, or anger? Why, in syncope, or under the influence of terror, is the skin bathed in sweat, or do the secretions run off profusely from the bowels? Why is one secretion lessened by the increase of another,—or why, under other circumstances, as in the latter stages of phthisis, is this relation of antagonism overcome, and diarrhœa, sweating, dropsy,

expectoration, &c. all augmented? Why is there a remarkable relation between the secretory functions of the skin and of the kidneys, as evinced by the increase or diminution of one, when certain impressions are made upon one or the other organ? Why is blood secreted from a distant part, when the menses or hemorrhoids are suppressed? Why is the mucus increased at the points of the membrane upon which the sound rests in the urethra,—or by the compressing instrument applied to polypus of the vagina, or of the nasal fossa,—or by the tube in fistula lachrymalis, in strictures of the æsophagus, and in various analogous instances? In all these cases, too, where there are associated glands, they send forth redundant secretions. (1) Why do cantharides exclusively augment the flow of urine,—mercury, especially, the saliva,—changing also its specific character; or why does the former substance change the perspirable matter into a copious secretion of serum, of lymph, and finally of pus; or, why does one cathartic produce serous evacuations, and another, a profusion of mucus, or bile, &c.? Why are some secretions at one moment abundant, whilst others are sparingly produced? Why does iodine promote the wasting of particular parts? Why, in diseases, whilst all parts are freely supplied with blood, does one gland increase its secretions at the expense of another? Why do the skin and the kidneys interchange their functions at particular seasons of the year? Why is the whole glandular system dormant in the fœtus, and why do the secretions start forth in such abundance at birth? Why do the testes yield nothing, nor the womb its menses, till adult age? And why do the mammary glands pour out the milk of life at the only moment when life demands it? Why does the lachrymal gland express the passions in childhood, more than the eye or the face?

Will chemistry answer? And then there are all the diversified vital phenomena which coincide with the various physical re-

(1) We have stated, in another place, a fact which has often fallen under our observation, that smoking tobacco has induced an attack of the piles,—and this, frequently, without apparently disturbing the intermediate organs. A medical gentleman of distinction in this city has lately informed us that snuff-taking has brought upon himself a very troublesome itching of the anus, and that this subsides as soon as the snuff is abandoned. Now what have any chemical forces to do with all this? And if not with this, what connection can they have with all the other phenomena and results of life,—since all of them are more or less connected by analogies with the foregoing?

sults of which we have spoken,—always more or less appearing in conjunction, yet all differing according to the differences of texture with which they are connected.

Finally, as we have said, we find in the secretions many substances to which there is nothing analogous in the blood,—such, for instance, as urea, (1) or picromel, resin, cholesterin, cholic, and elaic acids, taurin, semen, casein, horn, hair, sugar, animal poisons, gelatin, &c. unless in the last instance we submit to the chemists, and yield its affinity to albumen. All these substances, with the exception of sugar, are habitually formed from the blood, and many of them in vast quantities, by particular parts of the system only. They have no existence in the inorganic world; and whilst the chemist admits that he is unable to convert even albumen into gelatin, he says it is the easiest thing for the organized system to do it in every part of its nutritive apparatus. And although albumen is found in the products of glandular organs, it appears that gelatin never is. Thus we have the remarkable fact, that particular parts of the living organization have the faculty of always generating compounds that are wholly unlike any constituent of the blood, whilst no other parts can assume the same offices, nor can the former interchange in respect to their particular products;—that this is true through all time, of all men, and of all animals that are organized like men,—though the parallel fails the moment organization changes.

If we ascribe to the vital forces, and the varied instruments through which they operate, the power not only of electing certain constituents of the blood, but of decomposing and recombining their elements according to the existing peculiarities of the forces and their instruments of action, there is no difficulty in accounting for the foregoing, as well as for all other vital phenomena. It might be admitted, that after the vital actions had elected from the blood the elements for any new compound, chemical affinities may determine the new combinations. But then the difficulty arises as to the first step in the process after the vital actions shall have elected the materials, which consists in their decomposition. In certain parts, as in the liver, a variety of new combinations are simultaneously formed, and were the constituents of the blood decompounded by the forces

(1) It is said to have been found in some cases of diseased kidneys, &c. Probably an absorption. See Guy's Hospital Reports, vol. i. pp. 354, 358.

of chemistry, (1) it is inconsistent with all that is known of these forces to suppose that the decomposition would be always uniform, or that so many precise combinations would be formed from four or five elementary principles, existing, as it were, in a state of intermixture with each other. On the contrary, we should expect the most heterogeneous results. And this is precisely what happens in the laboratory of the chemist, whenever he attempts the recombination of a single constituent of the bile, after separating its elements. Besides, it is now admitted on all hands, that the blood is endowed with the peculiar condition that appertains to the living solids; and since, in the latter instance, it is well ascertained that this condition resists the forces of chemistry, so also may we safely argue that the blood opposes a like resistance.

The hypothesis as to galvanism is only a display of ingenuity, or a cloak for ignorance. There is not one good fact to sustain it. Try it upon any part of the blood, or its various parts in any state of combination,—in tubes of the most cunning contrivance, and it will always turn out like the philosopher's stone. Nor is there a shadow of reason to suppose that galvanism is generated in any part of the higher order of animals. No analogy can be derived from the torpedo, &c., for reasons which we have stated in another place. Upon what plausible ground can Dr. Prout, (2) and others, defend the assumption that the liver, pancreas, stomach, &c., form together a galvanic battery? Is there any greater similarity in their physical relations to each other, than between the mill-stone and the hopper; or do they present a single phenomenon which is remotely analogous to electric operations? There is nothing offered but general assumption. The electric current has never been detected in the nervous or in any other part. (3) Whenever specifications are attempted, as we shall variously show, it is conceded that even analogy wholly fails. If digestion be promoted by galvanism, it is evidently in virtue of its properties as a stimulant to the vital forces. Besides, has it been considered what an enormous

(1) Dr. Elliotson remarks, that "chemical affinities are not destroyed by life, but only so brought to play that decomposition is not their result." (a) This assumption might pass, were it not that some forces or other are "so brought to play," that "decomposition" is quite an important part of the animal economy.

(2) Bridgewater Treatise, &c. (3) See Müller's Physiology, vol. i. p. 72.

(a) Human Physiology, p. 27.

quantity of the galvanic fluid would be necessary for the purposes of digestion, and the formation of the various secreted products,—especially when rapidly augmented, as in weeping, &c.? Should we not try this question by the trifling results, in the way of decomposition, which are obtained from a galvanic battery, whose power is immeasurably beyond any thing which can, with reason, be imputed to the animal body? Moreover, it appears to be absurd to suppose that galvanism or any other chemical agent would operate with undeviating uniformity in particular parts, and never bring about the same results in other parts where the same fluid is the substance acted upon.

"In chemical processes," says an able defender of the chemical hypothesis of life, "re-agents give rise only to products, combinations, and decompositions,—applied to organic bodies, and especially to the nerves, their effects, however various they themselves may be, are never other than manifestations of the proper forces of the bodies acted on, or modifications of their forces." (1)

This is an obvious truth, and we make the quotation because the truth is well spoken. If we apply galvanism to a nerve, we get nothing but spasms of muscles, and some manifestations of an increase of vital actions; but no change in their products. If we now substitute another chemical or mechanical irritant, precisely the same results follow,—even greater, sometimes, from alkalies. A blow on the nerve of the elbow gives the same sensation as electricity. The galvanism pervades the whole part. It must be exactly the same as if generated by the *part itself;* but it does not augment the quantity of the secretion, unless the actions of the part be also increased,—and even then its effect may be that of diminishing their quantity. Just so is it with muscular action. It is at first excited by galvanism, but completely extinguished, for a while, by the long continued application of the fluid in the same quantities. And so with the sensations. This shows that the agent is unnatural, and that it impairs or destroys the forces that are natural to the living tissues. Carry it farther, as in a stroke of lightning, the same effect is more perfectly and suddenly produced. What is depressed, suspended, or destroyed, in these experiments? The chemical forces? Or has artificial galvanism affected the elaboration of

(1) Müller's Elements of Physiology, vol. i. p. 612.

galvanism in the animal batteries? Or if galvanism and the "nervous principle" be identical, has it paralyzed or extinguished itself? Or why is not a dead animal as good a galvanic apparatus as a living one?

In Dr. Philip's experiments of restoring the digestive process by galvanism after dividing the par vagum, the stimulant only rouses the vital actions,—only "produces manifestations of the proper forces" of the stomach. (1) Were it otherwise, then should galvanism artificially applied, especially in long continued and heavy doses, very sensibly affect, at least, the quantity of the secretions. But we have seen that this will not happen without an increase, or a change of the vital actions; and this does not ensue till it can be fully shown that galvanism, *per se*, has no agency in the results. We transmitted this fluid through the salivary glands of a boy for half an hour, but saw not the least evidence of a greater flow of saliva. But in all fairness, we should mention the fact, that at last, to reward him for his patience, we transmitted a shock of some severity; when he became angry, and the tears flowed in torrents, and his mouth foamed. Whether this phenomenon was owing to the charge of our battery, or whether thereby we had started the animal batteries, we could never satisfactorily make out. But however this might be, we could not help reflecting, that such a deluge from the blood could never have found its rapid escape without other outlets in the capillary vessels than the "invisible pores."

Again, if a galvanic current be transmitted through a nerve transversely, it excites muscular contractions; but if the nerve be tied, this effect ceases, although the nerve is as good a conductor as before. This is a most prolific experiment. It appears to destroy the supposed identity of galvanism and the "nervous principle." It shows, too, that in the living economy more important principles are in operation than that of galvanism. It shows that there is something peculiar to the nerve itself which has a certain agency in the vital actions. It divests this mysterious agent of its universal attributes, and is one important step in assigning to it its proper rank in the world of dead matter. (2) Humboldt took up this experiment, and showed that by transmitting a current through a compressed nerve and its associate

(1) Will not alkaline solutions applied to the nerve do the same?

(2) Much serious, as well as laughable use has been made of the "violent shocks" which Cotugno suffered, when his hand was struck by the tail of a mouse.

muscle, contractions could not be excited without insulating the nerve between the muscle and ligature.

As to the agency of electricity in the formation of the secretions, we could suppose it probable, could we as readily comprehend how the substances are so precisely decompounded as to be always united in such a way as to form some thirty or forty distinct and exact compounds that make up the bile,—eight of which, at least, are wholly new. Having got the precise elements, and all kept apart from each other, excepting such as are required to form each distinct combination, electricity might step in to aid the chemical forces in consummating the combination. (1)

If we concede that the decompositions and recombinations,

(1) The excellent editor of Mr. Hunter's Lectures is disposed, upon this point, to go with the chemists. He thinks that "Mr. Hunter, in his zeal for the vital principle, has probably gone too far in excluding the agency of chemical causes." Very well — but show us your reasons, gentlemen; — we always give you a hearing, and a fair one, by quoting your facts and arguments.

"If it were possible," says the editor, "to present the elementary particles to each other out of the body under precisely the same circumstances that they are presented in the body, and in the requisite states and proportions, the same combinations would take place, and the same results would be produced." But what does all this prove? It is nothing else than conjecture, — a mere begging of the question, and leaves the subject exactly where it was found. It moreover has the singular merit of admitting that a living, organized apparatus is indispensable to the results, — since nothing else can supply the conditions which our author requires. We have made many citations of this nature, in other places, because, in common fairness, we should show the nature of the proof upon which the chemical doctrine reposes. For this reason, we should have added another note by the editor, which may be thought to have a special bearing upon the question. "The refinements," he says, "even of modern chemistry have not yet enabled us to form any *animal* or *vegetable* principle, *ab origine*, from any combination of the ultimate elements." (*a*)

Mr. Hunter says, "the only substance produced in an animal, which cannot be called *animal matter*, is the earthy matter of bone;" and his editor subjoins, that "it is a question with some geologists whether *lime* is not essentially an animal product." Thus we again see, for the fiftieth time, that the chemist at last abandons his own ground. Turning over another leaf, our editor goes on with the same admissions, which are only a recapitulation of Mr. Hunter's arguments. Thus, we read in a note, that "the transformation or passage of common matter, *under the empire of physical laws*, to living matter which exists in defiance of those laws, is a mystery which will, perhaps, never be completely understood." (*b*) But is it not evident, if matter become animated "in *defiance* of the physical laws," that those laws can have no connection with the process, — but, on the contrary, are perfectly excluded? And, according to our editor, "there are grounds for believing that the first rudiments of life, or the revivification of dead animal matter, actually commence in the stomach."

(*a*) Hunter's Lectures on the Principles of Surgery, pp. 16, 17, *notes*.

(*b*) Ibid. pp. 17, 18.

which are going on in the living organized body, are partly the result of chemical forces, especially the latter, it is here that electricity plays its part. So far, no agency of this substance is implied in the direct phenomena of life. It would, then, merely promote the union of elements according to their affinities. And if this limited use be assigned to the electric fluid, it is wholly more compatible with facts, to suppose that it enters the body from without, than to surmise the generation of galvanism by the organized tissues.

Again, it may be thought that the presence of this fluid in organic, as well as in inorganic matter, implies some agency in the operations of the organized mechanism. We see not, however, that such an inference should follow. For inscrutable purposes, it is a law of this fluid that it shall pervade all matter; but the final cause may be wholly foreign to many species of matter. It is rational to suppose that there may be some great end in relation to the inorganic world, which could not be answered, unless this universal law existed,—and it may have a common reference to the physical relations of organized matter to the rest of the universe, — such for instance, as gravitation. Besides, we can see a special reason why it should pervade the living organism,—and this is, that it should be ready to aid the chemical forces as soon as the vital become extinct. And since it contributes nothing to that destructive work in the living body, in which it is so lavish of its influence after death, we argue, also, from this fact, that it is permitted to abide only as an inert substance in the living system. Where such an agent is perfectly indispensable to the actions of the chemical forces which take place after death, it would have been an unnecessary provision to have excluded it from the living system where powers are supplied that render it inert and harmless during their existence.

But another objection, not less formidable, grows out of the foregoing considerations. There are many organs and tissues, if, indeed, we may not say so of the whole, which either produce combinations that have no existence in the blood, or so modify the constituents of that fluid that they are no longer the same. The chemical hypothesis is based upon this admission. But these various parts are, also, made up of compounds wholly different from their secretions, and are constantly employed in renewing the variety of matter which composes their structure.

It is, therefore, evident, if the results be of a chemical nature, there must be the most diverse processes going on in immediate contiguity with each other,—whilst each is forever distinguished by the most precise characteristics. (1)

But we will not tire the reader by pursuing the subject any farther. We leave it, however, with reluctance, and cannot yet dismiss it without saying, that although many, who expound the phenomena of life upon chemical principles, admit that there are such things as vital forces, they have never told us what is their business. We have enough about galvanism, and other solutions of an analogous nature; but nothing appears to be assigned to the vital forces; whilst it is impossible to offer a probable reason why these last do not originate and perfect every change that takes place in the living system. The difficulty is, the vital forces are placed beyond the reach of such experiments as have made us familiar with galvanism; but it should be recollected that it is but a little time since we knew anything of the chemical capabilities of this fluid. (2)

There is yet another aspect in which we could wish, for a moment, to contemplate the forces of chemistry in their supposed connection with the actions and results of life. If we look at the vegetable kingdom, we find in all its phenomena, a repulsive attitude towards the agencies of chemistry. There is an ever-during exactness in all the characteristics of every species; yet so intimately allied are the nearest affinities, that it is only by great minuteness of observation, that we may determine the characteristic distinctions. What connection can chemistry, or any of the forces of physics, have with all this unvarying, and closely allied organization? And yet, however near the apparent approximation in this respect, and although flourishing side by side, one plant shall be an aliment, and the other a deadly poison. On the other hand, everything is confused, and forever

(1) Müller has a mathematical subtlety, which we submit for consideration. "It might be imagined," he says, "that each molecule of blood which comes under the decomposing influence of a secreting organ, is completely decomposed and disposed of by it. If the molecule of blood be expressed by the sign a, and a molecule of the component matter of the secreting organ be x, the secretion would, according to the above supposition, be $a - x$." "It is," he adds, "a simple, and, therefore, attractive hypothesis." (*a*) Q. E. D. But how would this answer for such complex secretions as the bile?

(2) See APPENDIX.

(*a*) Physiology, vol. i. p. 467.

changing, in the inorganic world. And yet such is the law of nature in respect to analogies, the vegetable kingdom appears to merge, at last, in the inorganic. Still the lowest plant, in its structure, endowments, and processes, is as distinct from inorganic, as light is from darkness.

Passing from the great outline of inorganic matter, where the progression of affinities appears to have no existence, we come to the great chain of sensible analogies which connect organized matter with its Creator. But this begins with species of vegetables that are so allied, in sensible appearances, to the inorganic, that we may be only able to say where the beginning commences, by observing the interstitial growth of the former,—its power of reproducing its species,—its absolute dependence on gaseous or other aliment, &c. At length, we attain a more distinct point of departure; but as we pass along from species to species which are most intimately allied, we have an almost equal difficulty in fixing the distinction. This distinction, however, is ultimately obtained, and remains a never failing guide to the recognition of the species. But the second remove gives us another point, from which we advance onward and upward, in the same hesitating manner, till we finally run, by imperceptible degrees, into the highest degree of vegetable organization.

Pursuing a retrograde analysis, we ultimately arrive again at inorganic matter. But here, also, we come in contact with the lowest species of the animal kingdom; — the three so apparently blended, that we know not, by physical signs, the ending of one or the beginning of the other. The sponge supplies an example of a sucessive arrangement, by naturalists, in the three great kingdoms of nature. But, however illusory the physical distinctions, the internal characteristics, which depend on structure and specific powers, are always definite and precise. It is by the peculiar processes that are carried on by the forces of life, that the distinction is indicated. Nor is it a little remarkable, that the vital powers, which have been held so chimerical, should be the foundation of the test by which we completely separate the lowest of the animal and vegetable kingdoms from the inorganic, and those, again, from each other. It is one of the triumphs of modern science, that it has established an impassable limit between vegetable and animal organization, in the discovery of that great law which has ordained that animals shall subsist upon organic matter, whilst the inorganic is the nutriment of plants,

—thus making the vegetable kingdom the great laboratory of food for the higher department. The inorganic is destined to subserve the uses of the world of life; and thus utterly subordinate in its objects, and so totally separated from living matter in all its phenomena, reason would seem irresistibly to suggest a perfect distinction betwixt the forces of each.

Thus, then, we have the vegetable kingdom isolated from the mineral and the animal; whilst the distinction is only known by laws that have been deduced from a long series of observations. By this method we pass the transition from the lowest of vegetable to the lowest of animal organization, and we mount along the animal kingdom in exactly the same embarrassed manner in which we had studied the alliances in the inferior department.

But here the variety in organization is immeasurably greater, and is marked in its outline with greater distinctions; yet all parts successively terminating by imperceptible gradations in each ascending link of the chain. Amongst the aquatic zoophites, insects, fishes, &c. everything runs into each other as in the vegetable kingdom; yet every species is precise in its organization, its functions, and its products. These, again, pass by close affinities, into land-animals; — the insects of the water into the terrestrial, — the saurian fish into saurian reptiles, — the breathing fish, along gradations, into seals, — and these, at last, into terrestrial mammalia; and by another branch, we are gradually conducted to the feathered race, which passes on to land animals, as seen in the bat, the ornithorynchus, &c. The intimate affinities begin with the lowest aquatic zoophites; and thence mounting upward, and branching outward, in multiplying directions where other branching links are constituted, yet all blended, one by one, as each series takes its departure and stretches forward, till the highest of the whole terminates in man, who is mainly distinguished from the last, by reason, voice, and the upright posture. And yet, again, from almost every link in the several ascending series, a new chain branches off, — all variously terminating in each other, as they approximate the common link which is formed by man. Nor is this the end of our chains. The last link, only "a little lower than the angels," stretches upward into the world of spirits, — where the whole merges into God Himself. "Usque adeo natura," says Scarpa, "una eadem semper atque multiplex, disparibus

etiam formis effectus pares, admirabili quadem varietatem simplicitate conciliat."

Looking back upon this almost endless series of gradations, branching off in various directions from the first link in the chain, which is the common beginning of a thousand chains, through the whole of which the fundamental principles of the common link are completely distributed, and the organization of the whole founded upon common laws, — and although the extremeties of each chain, at their departure from the common link to their termination in the highest common link, which is formed by man, be widely separate, yet in no two successive parts of the complex, but harmonious whole, shall we find a break, and scarcely a mark of separation; thus looking back, let us ask the chemist what possible connection the laws of physics can have with such a system of exactness and similitudes?

So entire is all this harmony of animated beings, even of the vegetable kingdom, — so perfectly and immutably blended together, yet always so distinct and exact, that when a part is lost, the lover of nature wanders about in restless, but confiding inquiry, for the absent medium of connection. "It is there! or it is there!" and onward he bounds over mountain and vale, over sea and lake; nor does he cease, till success rejoices him more than the possession of all the rest.

Again might we go on, and ask for the causes which suspend the process of vegetation, as soon as the seed has reached its maturity, — why the leaves fall, and the sap ceases to circulate, — or why, again, at certain uniform periods, the process of organic action is again resumed, and what was but yesterday a leafless, and apparently lifeless forest, is decorated to-day with an enchanting foliage, and with gorgeous and fragrant blossoms?

Since, therefore, neither the catalytic forces, galvanism, nor any chemical agent or force will explain the results of life, its processes must be perfecctly *sui generis*. It is admitted by the chemical theorists, that all "the changes differ essentially from *all kinds* of *chemical* phenomena." Therefore, it is an irresistible inference that the forces which determine the changes "differ essentially from all kinds of chemical" forces. The vague resemblances which exist, and the disposition to explain all phenomena by some universal rule, have led to this invasion

upon the institutions of nature. The whole material world is connected together by analogies; but the rule which would refer the products of organized beings to the agency of chemical or physical forces, would make man a plant, or a vegetable a stone. If we infer the dependence of organic products upon chemical forces on account of the nature of their composition, it might be equally argued, that the whole phenomena of life depend upon physical laws, because the instruments of action are constituted upon mechanical principles.

We have seen, that in endeavouring to account for the formation of the secretions, the chemist ingeniously resorts to the agency of what he denominates the catalytic forces. If ammonia be heated in a gun-barrel, the latter disunites the elements of the former, without combining with them. This theory, therefore, considering the multifarious secretions that are generated, supposes that every vessel, and every pore, that is concerned in forming different elementary combinations, is endowed with a distinct chemical influence, although every vessel may be as much composed of a common substance as all gun-barrels are composed of iron. The very gist of the doctrine, however, requires that the composition of different vessels, which determine very different secretions, should be as unlike to each as iron, platina, iridium, &c. are to each other. The theory is baseless without this assumption, which will scarcely be admitted. As to the supposed differences of chemical influences according to "the shape and magnitude of the pores, the length of the pipes and tubes," we see not by what right the catalytic can depart from his own rule of analogy, till he can show us that gun-barrels of different lengths and diameter exercise different influences upon the elements of ammonia. We rather regard this as "cutting the knot, than as untying it." We have but just got rid of Boerhaave's *error loci*, to accommodate which, his imagination gave every possible shape to the vessels and pores. Let us inquire, also, what analogy there is between this gun-barrel process (which is the best illustration that chemistry has yet afforded,) and those changes that are induced in the various secretions by emotions of the mind? (1) Can you bring any analogous causes to act on the

(1) Chemistry neither explains the cause of the changes, nor can it detect the nature of the alterations. We know them, in part, by their effects upon the forces on which they depend. The child suffers from the milk, when the mind of the mother is agitated; the bile is altered in colour, increased in quantity, and purges under the influence of passion.

gun-barrel, platina, &c.? But leaving the influences of spirit upon the physical results of life, will the catalytic or any chemist tell us how the various actions, secretions, &c. are more or less suspended by cold in the hibernating animals? Perhaps nothing more easy, you say. Here, just as in the gun-barrel, cold approximates the molecules of which the tubes are composed,—their diameters are lessened, and therefore the catalytic, or other chemical, influences, must be diminished or destroyed,—and, moreover, the very important chemical agency of heat is alike withdrawn. Thus, you conclude, it is exactly the same in both cases; and doubtless you feel obliged to us for our benevolent help. But this is only an instance of that common philosophy which is founded upon one half the circumstances of a case,—the remaining half being exactly the other way. For, if we now increase the degree of cold which surrounds the torpid hibernating animal,—carry it down to a point which threatens the extinction of life,—you will see animation rousing itself in vigorous defence,—the circulation, respiration, heat, and all the secretions starting forth, and acquiring their full perfection; whilst you may still go on reducing the atmospheric temperature. At this moment, too, under the circumstances of change so utterly opposed to your own theory, and so repugnant to chemical philosophy, the temperature, and various functions of the animal, will be the same as in a summer's day.

Still, there can be no doubt that animal and vegetable products depend, in part, upon the arrangement, size, &c. of the vessels and pores. But of the extent of this principle we know but little. There are numerous phenomena, of which we shall speak again, that imply a greater dependence upon modifications of the vital forces in different tissues, and different orders and series of vessels. We differ especially from the catalytic chemist upon this point, in supposing that the influences which depend upon size, length, &c., of the vessels, are the result of vital, not of chemical forces. In any individual part, however, the organization is always the same. The variations of its phenomena and products, therefore, depend upon other causes than such as are assigned by the catalytic theorist. Scott is undoubtedly right, when he says, "the vulgar idea of a peculiar organization (beyond fineness of ear and of utterance) is nonsense."(1) And

(1) Diary, January 12, 1826.

pores, "for, percolation depends less on the *form* of the organ than on its *vital powers*." "So that the *requisites* for a given production are two,—materials and organic powers." (1) This last is our doctrine.

We have constant admissions, on all hands, that the secretions are variously affected by the nervous influence; whilst the admissions are as ample that "we can form no idea of the mode in which the mere chemical action of two bodies can be effected by that influence." (2)

Thus it is with all who defend the chemical doctrine to the last extremity. Ingenuity is exhausted both in argument and illustrations drawn from mechanical contrivances. But with a candour that becomes them more as men, than their former efforts do as philosophers, they admit that life is nothing like chemistry; still, in the very face of this admission, the chemical doctrine, which would serve as well to expound the revolution of the heavenly bodies, is sent forth for "the history of organic existence."

As to the catalytic theory of the chemists, it is nothing more than the revival of the speculations of Asclepiades, which, by the force of their novelty and simplicity, had threatened to overturn the Grecian philosophy at Rome. The Hippocratic had prevailed in the imperial city for nearly two centuries, and was now denounced by the reformer as "a meditation upon death." This innovation was transplanted upon the corpuscularean philosophy of Epicurus; and merely by a change of terms, Asclepiades pretended to explain the causes of all secretions, and all diseases,—imputing them to the difference in number, figure, and size of the pores and vessels. (3) He taught what we have seen affirmed at the present day, that "there is but little difference between man and a sieve."

Another very ingenious American experimenter upon the blood, thinks that "instead of placing the actions of the living system under the dominion of certain *occult* powers," "ought we not rather to consider them, so far as they are within the reach of human investigation, as differing from the *phenomena of inorganic matter* only in the peculiar complexity of the

(1) Human Physiology, part i. pp. 260, 266, 276.
(2) Bostock's Physiology, vol. ii. ch. 10. § 4.
(3) Cœl. Aurel. de Morb. Acut. l. i. ch. 14.

structures in which they originate, and the multiplicity of the agencies to which this complexity must give rise."

Here, again, is the same begging of the question with which the chemist always starts off,—the same "cutting of the knot," which he charges upon the vitalists. The *occult* powers of life, and the *revelation* which distinguishes the forces of chemistry, are always the points of departure; when, in truth, one is just as *occult* as the other. Both forces are known by their phenomena, and by nothing else. There is a vastly greater variety, however, resulting from the powers of life, and in this respect we are better enlightened as to their nature, than as to the chemical forces.

But our object was, to show how an intelligent chemist will always slide off, at once, from his own ground. Our author immediately concedes, that "it is true, the chemist, in his researches into life, *is not permitted to reach the sanctuary of the presiding genius;* yet this is no reason why he should not be allowed to investigate the *structure* of her habitation." (1)

Now, here is every concession we could wish; and it is what every enlightened chemist irresistibly, though perhaps unconsciously makes. The "sanctuary, they cannot reach;" and as to the "presiding genius," that is our vital force; and we think that our author himself, on farther consideration, will admit, that it is the business of the anatomist, not of the chemist, "to investigate the *structure* of her habitation." We therefore see not what remains for the chemist, according to his own interpretation. The ground being thus voluntarily abandoned, we shall go on to occupy it. But at present we shall conclude with a remark of a chemical physiologist, by way of showing, once more, the nature of inductions of the most general and important import.

"The conclusion which would follow from this view of the subject," says Dr. Bostock, "is, that an attraction exists between the mouths of the lacteals and the chyle, which seems to be *analogous* to, or *identical* with, the elective attraction which *unites different chemical substances;* and that the lacteals, as well at their extremities, as through their whole extent, are *possessed of contractility*," *&c.* (2)

This is to us, an unintelligible combination of chemistry and

(1) Dr. Rogers, in American Journ. of Med. Sciences, vol. xviii. p. 278.
(2) Physiology, vol. 2. p. 460.

vitalism. Whatever the attraction be which "exists between the mouths of the lacteals and the chyle," we suppose it will be conceded to exist, also, in relation to all other parts where analogous phenomena take place,—which, indeed, will embrace every variety of capillary action. Again, this attraction is considered so "analogous to the elective attraction which unites different chemical substances," as to be identical with it. On the contrary, however, it is so far from being identical with the elective attraction of chemistry, that there appears to be no sort of affinity; and this is, at least, conceded by all who defend the chemical theory of life. Indeed, in the foregoing quotation, we see our author coming to this opinion before he has finished the sentence in which the doctrine is announced,—as is manifest from the sense in which he uses the word "contractility."

Still, there exists in the vital processes the same exact elective attraction that distinguishes the chemical. But there is this important difference, which separates them entirely, and refers them to totally different laws: In one case, the attraction takes place betwixt "different chemical substances,"—betwixt a particle of matter of one kind and a particle of another, by which a new substance is formed,—differing entirely in its various properties from either of the elements. In the other case, however, the action is betwixt a compound fluid which undergoes no change appreciable to the chemist, and a vessel which remains perfectly unchanged, and yields no part of itself to the substance attracted. The idea, even, of capillary attraction would be wholly indefensible, since nothing is admitted to the lacteals but chyle.

Perhaps the greatest difficulty which chemistry encounters in its attempted analysis of life, is the variety of the simple modifications of the natural constituents of the blood, as manifested in health and disease. Here, it is generally impossible to detect any difference in the elementary constituents which form the component part of the healthy blood, and that which is modified by morbid actions. Still there are other ample proofs of the change,—from which alone we fairly argue the existence of specific forces.

We might cite a hundred instances to show the difficulties into which chemistry is forever leading us, when we apply its laws to the actions and phenomena of life. But we will sum them all up in the late theory of Dr. Leymerie, who "supposes that the phenomena of vitality, the *punctum saliens* of Haller, and the red

colour of the blood, are the products of a chemical combination resulting from the union of ammonia (volatile alkali) with a substance of the nature of gelatin." (1)

SECTION III.

Those who admit the existence of a vital principle, as contradistinguished from physical forces, generally regard it as a simple material substance. This is well enough stated by Dr. Stevens. "Like the cause of heat, or electricity," he says, "it exists in the shape of an invisible, imponderable fluid," (2) or as Müller has it, "imponderable matter." (3) The "spiritus archæus," the "vis insita," the "organic soul," &c. having been laughed out of fashion, many physiologists have taken up the appellation of "organic force,"—avoiding *vitality* as far as possible. But, although we entirely object to the term "soul," we do not see that the matter is much improved by rejecting the "vis insita," —unless it be that "organic force" is less obnoxious to the chemical hypothesis than the more significant term of "vis insita." Others, again, like Abernethy, Sir H. Davy, &c. (who, however, are oftener right than wrong upon this subject,) appear, frequently, to make no radical distinction betwixt the life of a man, and the attributes of a stone. "It is not meant to be affirmed," says the former, "that electricity is life. I only mean to argue in favour of Mr. Hunter's theory, by showing that a subtle substance of a quickly and powerfully mobile nature seems to pervade everything, — and is the life of the world; and that, therefore, it is probable, that a similar substance pervades organized bodies, *and is the life of these bodies.*" (4)

Now there is nothing doubtful or ambiguous about this, — since our author intended it as a distinct exposition of his views,

(1) New-York Med. Repository, vol. viii. p. 442.
(2) On the Blood, &c. p. 124.
(3) Elements of Physiology, vol. i. p. 287, &c.
(4) Introductory Lectures.

in reply to a misapprehension that had existed. "I am concerned," he adds, "because my meaning has been misunderstood, or misrepresented." And yet, what is remarkable, at other times when he has not directly before him the glaring opposition betwixt the phenomena of organic and inorganic matter, he makes the most absolute contradistinctions between the forces of one and the other. Indeed, this may be affirmed of most others who hold similar opinions, — the unaccountable fact existing, that the more luminous the subject, the more the light is turned into darkness.

But our author also says, that "every hypothesis should rationally account for all the known phenomena of the subjects they pretend to explain." Now no one will deny that living is contradistinguished from dead matter by every phenomenon appertaining to each, or that the forces which prevail in dead matter will no more explain the phenomena of life, than they will the direct acts of Providence. Electricity has been assumed either as the essence of life, or its unmeaning type, for no other reason than that it is imponderable, mysterious in its ways, and pervades all matter; whilst, as we have seen, it leads to conclusions that are opposed by the whole history of organic life. It is assumed, therefore, only as a cloak by those who will not comprehend the plain phenomena of life, or to aid, by its subtleties, in putting down better theories. All attempted illustrations, however, drawn from sources where no analogies exist, appear to us only to darken the subject.

All we know of the absolute nature of the "organic force," or "vital principle," resolves it into several distinct properties or forces, some of which may exist independently of others. Thus, we have irritability, mobility, vital affinity, sensibility, sympathy. We infer their distinctions from certain well known phenomena. They appear to be inherent in the several parts of the system, and not floating about like caloric or the electric fluid. They do not even all belong to the different tissues, since some of them are wholly peculiar to the nervous system; — at least this is true of sensibility, if not of sympathy.

Still, we think those philosophers right, who regard the essential principle of life as a simple substance; and it will be one of our objects to show that this opinion is well founded. But in all our investigations on this subject, it will remain our principal purpose to contradistinguish, entirely, the forces, and

actions, and phenomena of life, from those of physics. Nor is this a mere speculative inquiry, without its practical uses. Its import is of the highest and most dignified nature, not only in relation to science, but to the art of medicine. There can be no correct or philosophical treatment of disease, so long as our remedial agents are supposed to be concerned about the forces of physics or chemistry, — and there can be no sound principles in medicine, whilst they are imbued with illusions from the physical sciences. The records of medicine abound with the grossest errors and superstitions, that have had their origin in fallacious views of the properties of life; and when we speak of the humoral pathology, it will be seen that their practical results have been worse than the philosophy.

We shall, also, have occasion to show, that the physical doctrines of life have led to the theory of spontaneous generation, and have invaded the sanctuary of the Mosaic history of creation.

That we may be fully understood in this place, we will again state the creed which we wish to make out. We believe that the vital principle, vital power, organic force, organic power, are one substance, — and we use these terms synonymously. They refer, with us, to a universal cause of animal and vegetable life. We believe, also, that this vital principle has various attributes, distinct or generic, and partial or specific; or perhaps we should call the former distinct properties. Thus, of the generic or distinct, — we have irritability, mobility or contractility, &c. and the modifications of each of these in the same or different tissues form our specific (1) or partial variations. These properties are also constantly varied in disease, and these variations we call *changes in kind.* The partial modifications, in their natural state, we also call *variations in kind.* This is much in conformity with the views of Bichat, and it appears to us to simplify the subject as far as it can be done at present, and to be in strict conformity with the phenomena of life. Our conceptions of this method are, also, greatly promoted by many analogies that are supplied by the mind, to which the phrenologists assign, without hesitation, some thirty-six perfectly distinct properties.

(1) We do not use the word *specific* in its common acceptation. Like Mr. Hunter, we "only mean by it peculiarities, or distinctions." (*a*) This will be its general import, wherever it may occur.

(*a*) Hunter on the Blood, &c. Introduction, p. i. *Note.*

We now enter upon an inquiry which we concede to be, in part, hypothetical, but is still sufficiently justified by its importance, and by the numerous facts which prompt the investigation. We are aware, that it continues to be the discouraging opinion, that we are not sufficiently prepared for generalizations as to the nature of life. But if the day of preparation has not yet arrived, it will never come. We have been familiar, for ages, with all the phenomena of living matter, and it is upon these phenomena that our generalizations must be established. The indifference, or the timidity, that would resist inductions from a series of facts that have been accumulating from the time of Hippocrates, and to which no objections can apply, would be content to regard effects without reference to their causes, and to build up a science of facts alone. Such empirical philosophers scarcely ever advance beyond the precincts of instinct.

It seems to be admitted, on all hands, that the "principle of life," whatever it be, is a simple substance,—that is to say, the presiding forces are not constituted by distinct species of substances. A contrary supposition would be adverse to the analogies of nature, and to the astonishing harmony amongst the actions of organized matter. Nor is there any material agent with which we are acquainted, whose phenomena are at all analogous to those of the living organization. In the former instance, they are few, simple, and not liable to modifications from the influence of other causes. This is especially true of the simple forces of nature, — as of gravitation, — and it is even so of electricity. Their attributes are very limited, — for whatever be the variety of effects, they clearly refer themselves to a simple principle, or to an agent possessing very circumscribed and unchanging properties. On the contrary, however, whilst there exists a simple principle of life, its phenomena are so incalculably diversified, that they denote their dependence on several distinct and variable forces; and of these, sensibility is at least peculiar to one tissue. The evidences of their distinct nature are too constantly before us to require exemplification. But we may say of the two properties which are most frequently confounded, — sensibility and irritability,—that their differences are every where manifested in affections of the nervous and organic systems. In paralysis, boiling water may vesicate the skin without producing a sensation; whilst, on the other hand, in the

cholera asphyixia, water of the same temperature may scarcely excite a blush on the surface, though it produce the same anguish as in health. We refer back to our first section, especially, for a variety of other facts which entirely contradistinguish the forces of life from every other power or agent, and which appear to disconnect them almost as fully from the material world, as the phenomena of mind abstract the soul from like affinities.

If we admit a nervous fluid, and regard it as identical with galvanism or some other analogous matter, it only gives obscurity to a subject otherwise as plainly illustrated by its phenomena, as is galvanism itself; for we have no facts in relation to such substances that at all explain the phenomena of the nervous system, or any of the results of life. Others, like Müller, (1) speak of the "oscillations of the nervous principle;" but this is equally unsatisfactory. How shall we conceive of motions being propagated, in the way implied, and with the rapidity of thought, through a series of nervous fibres to the brain, and thence to some other remote part; and this, either by touching a nervous filament with the point of a needle, or with a solution of potass, or again, with electricity? The same spasms, too, which the needle excites, after subsiding, may again return long after the original cause has been withdrawn. What is there in *light* to set in motion a fluid, when it impinges upon the retina, —and why is the motion so restricted to the optic nerve that no other is engaged in seeing, — or why, to the third pair, that no other muscle contracts than the iris, — and why the sympathetic contraction, at the same moment, of the other iris? The whole system is full of such examples, which the motion of fluids, vibrations, &c. in no respect explain. (2) The only analogies we have, appertain to the mind itself, and this is completely associated with all the phenomena of sensation, — whilst it may exert, more or less, an influence upon all the actions of life. "No language," says Fordyce, "has ever yet become so copious as to express the varieties in the senses." (3)

(1) Physiology, vol. i. p. 796.

(2) Mr. Hunter remarks that, "as we find the nerves of the same materials as the brain, it is a presumptive proof that nothing material is conveyed from the brain by the nerves, nor *vice versa*; but that they only continue the same action which they received at their end." (*a*)

(3) Dissertations on Fever, dis. i. p. 86.

(*a*) On the Blood, &c. p. 89.

We can see no objection, therefore, but much reason, for considering the essence of the vital force distinct from matter, — something analogous to the soul itself; (1) or rather, in conformity with the analogies of nature, as well as with the phenomena of life, — something intermediate betwixt spirit and matter, or, in animals, the principle of instinct and matter, which forms a connecting link. Material causes can as well operate upon such a principle as upon the mind; for come we must to the latter conclusion before we can reach the soul. As to the united action of body and mind, we might as well inquire "whether the fiddle or fiddle-stick makes the tune." (2) And as to our conclusion, which is founded, in part, upon analogy, (of which we shall say more hereafter,) we may add, that no objections can be reasonably made, — whilst, on the other hand, we have in the inorganic world, certain modifications that greatly separate the essence of one species of matter from another, — as light, heat, &c., if they be species of matter, from the ponderable substances. The imponderable penetrate, also, the densest, as readily, and even more so, than porous, substances.

The nature of every thing, so far as it can be comprehended, is only known by its qualities and phenomena; and in investigating the forces of life, they must be considered as well in their relations to mind as to matter, and all in their united operations. There are many phenomena, appertaining to the properties of life, which appear to place the principle upon which they depend in an intermediate rank between mind and matter. These we shall endeavour to indicate, from time to time, as we go along. The nature of the principle must be tried by its phenomena, just as that of all other forces, or as we contemplate the mind itself; — and we may reason and form opinions just as philosophically about it. But we have a higher object in view than that of rendering the foregoing distinction probable.

(1) This opinion is sufficiently ridiculed by Mr. Lawrence, (*a*) and Dr. Brown, (*b*) as well as that regarding the material properties of life. But the former is not good authority in spiritual matters, and the latter deals too much in spirit. Denials of this nature are altogether gratuitous. They are made without offering a shadow of reason, — and to give them their only point, they are sent forth in the dress of satire.

(2) Scott's Diary, March 14, 1826.

(*a*) Lectures on Physiology, &c. p. 78.

(*b*) Lectures on the Philosophy of the Human Mind, vol. i. lec. 7 & 9. Also, his Inquiry into Relation of Cause and Effect.

Physiologists, who ascribe the phenomena of life to chemical forces, get entangled in a labyrinth of difficulties. This we have abundantly shown in different places. We have seen the learned Müller in this dilemma; generally mounted upon the chemical hobby, but now and then springing upon our Pegasus; and finally going off upon the soul itself. "We do not assert," he says, "that the essence of the mind has its seat in the brain alone. It is possible for the mind to act, and receive impressions, by means of one organ of a determinate nature, and yet be present generally throughout the body." Indeed, he thinks that "facts prove conclusively, that the mind, although its only seat of action is the brain, is itself, nevertheless, not confined to it." But our author's facts show, also, that he considers the *mind* the *vital principle* in all parts; — and thus provided, it certainly becomes less remarkable that chemical forces should play their otherwise inexplicable part in the animal economy, when subjected to the direction and restraint of such an intelligent agent. It will of course, therefore, be supposed that our author goes farther than ourselves, in considering the vital principle perfectly immaterial and immortal. But it is not so, — at least in respect to its immateriality. This is shown by the "two facts," which our author considers "sufficient" "to prove conclusively," that the mind or vital principle is not confined to the brain. "The one is, that animals low in the scale, as planaria, &c. are divisible, and that some propagate their species by spontaneous division. It is evident from this fact, that the vital principle is divisible; but since each portion of the divided animal evinces a separate will and special desire, we have a distinct proof that the mental principle of these lower animals, whether it be or be not identical with their vital principle, is also divisible. The second fact is, that the *mental principle* in the higher animals, also, *in man even*, is, like the vital principle, in a certain limited sense, *divisible*." (1)

Our author's argument, as it respects man and the upper ranks, is founded mainly upon the phenomena of generation; and although it may be that our author would not advocate materialism, in its proper acceptation, yet when he comes to the divisibility of the soul of man "in a certain limited sense," and associates it, as a parallel case, with the complete diversi-

(1) Elements of Physiology, vol. i. p. 819.

bility of instinct, or, as our author calls it, "the mental principle,"—in the lowest animals, it appears to us but little better than materialism. This grows, in part, out of our confounding together mind and instinct; in part from identifying both with the vital principle; and, in part, from speculating upon a subject so far beyond all human comprehension as the endowment of the fœtus with a rational, immaterial, immortal soul. (1)

Addressing ourselves to those who strictly believe with us in the Mosaic history, we think we may find in Revelation some proof that even the forces of life are unique and have no types in any other department of nature. We premise, however, that we have no belief that any knowledge has been imparted by Scripture in relation to special matters of science; but that much may be inferred from the account of creation as to the nature of the forces by which living matter is governed. (2) On looking, then, at the account given by Moses, we find a very extraordinary and specific description of the manner in which man was brought into existence, and which distinguishes his creation from that of inorganic matter. "And the Lord God formed man out

(1) Our author farther says, "in favour of the view, that the *mind is merely a particular manifestation of animal life*, may be urged the fact, that it is not confined to one class of animals, or to man, but is possessed even by the *lowest creatures*." Again, "on the other hand, for the opinion that the mental is independent of the vital principle, it may be urged that one whole division of organized living beings, namely, plants, are destitute of all mental manifestations. This objection may, however, be met by the *supposition*, that the *vital principle is in a latent state in relation to* its *mental actions*," &c. And again, "if there be no body which does not possess an energy, a force, or action,—may not the *mind* itself be the *property resulting from a certain condition or combination of matter in living beings?* What is the cause of absence of all signs of *mental* acting in the body after death? Is it that the *matter* has lost its peculiar condition or composition,—the united action and alteration of its vivified atoms; and that these, their condition being altered, henceforth manifest other properties? Or is it that the mental principle has really separated from the body?" (*a*)

In another place we have adverted to other late speculations of a similar nature, and endeavoured to show that these are the fruitful sources of error in other branches of philosophy, and of that imputation of skepticism which rests heavily upon natural philosophers; who, of all others, should most avoid these quicksands, which never fail to make shipwreck of character, of truth, and morality. A mind thus biassed loses, also, its influence in propagating useful information,—since all its inductions are distrusted, if its direct affirmations be not justly subject to doubt.

(2) All species of learning should be made subservient to Scripture; but Scripture can rarely be brought to the aid of science.

(*a*) Ibid. pp. 822, 824.

of the dust of the ground, and breathed into his nostrils the breath of life, and man became a living soul."

Here was no agency of the chemical or physical powers. The whole plan, also, was perfectly distinct from that of inorganic matter. The fabric of the new being had no analogies with the former, and his phenomena were all distinct and without a semblance to anything that existed before the beginning of vegetable life. This, in itself, supplies an irresistible proof that new forces (or the same as designed for animals and vegetables,) were created for the government of his organization, and to constitute the essence of his life. But, as if to convey a full and distinct impression that man is not the creature of the physical forces, nor amenable to their operation, the inspired writer, after informing us that all the varieties of organization were direct and specific acts of God, and thus contradistinguishing organic from inorganic matter, proceeds to state the manner in which life was imparted to the miraculous fabric of man:—"He breathed into his nostrils the breath of life, and man became a living soul." Now, it cannot be philosophically contended that this act relates alone to the *soul* of man; for, in the first place, the annunciation refers as well to *life*, in its ordinary acceptation, as to the soul. The original language imports a distinction which clearly substantiates the foregoing construction, and leaves no room for cavil. Commentators agree that the Hebrew words relate even more to the animal powers or animal life, than to the soul. (1)

But, however this may be, in assuming Scripture as a ground

(1) "Hebrew, (plural) *breath of lives*; intimating, as some have supposed, that man possesses the vegetative life of plants, the sensitive life of animals, and that higher rational life which distinguishes humanity. The action is evidently spoken of after the manner of men." Again, "Hebrew, *became to a living soul*. The phrase *living* soul, is, in the foregoing narrative (Genesis i.) repeatedly applied to the inferior orders of animals. It would seem to mean the same, therefore, when spoken of man that it does when spoken of beasts, viz. an animated being, a creature possessed of life and sensation," &c. "As to the intellectual faculties which raise man so far above the brute creation, we find no term that expressly designates them in any part of the sacred narrative. This seems rather to be *implied* in what is said of his being made in the image of God," &c. "Indeed, it may be remarked that the Scriptures generally afford much less *explicit* evidence of the existence of a sentient immaterial principle in man, capable of living and acting separate from the body, than is usually supposed." (*a*)

(a) Prof. Bush's Notes, Critical and Practical, on the Book of Genesis, pp. 53, 54. 1839.

of argument, it is manifest that man was completed in his structure without life before he became endowed with a soul, (1)—and that the act which created his soul, bestowed also, the vital forces. One appears to be as much a new creation, distinct from the forces of dead matter, as the other. When man was already perfected in his structure, he was without life. But by the act of breathing into his nostrils, his peculiar physical life and his soul were simultaneously created; and such is their companionship whilst life continues, that some philosophers have considered them identical. And how perfectly in harmony is all this with the exit of man. His soul and the vital forces leave the corporeal frame simultaneously; nor will either be restored but by another act of creative energy.

But again, it cannot be said that the *soul* itself constitutes the *life* of man,—leaving out all physiological facts,—since brute animals and plants have as much the specific forces of life as man,—and since, also, reason and Revelation enforce the belief that animals and plants have no soul.

But it is objected, perhaps, that plants and animals were created antecedently to man, and that we have no such account of the creation of the same vital forces which equally appertain to animals and plants; though we think, in a modified state as it respects the latter. We grant the objection is apparently reasonable; and could it not be obviated, it would affect the validity of our argument. But let us try. In the first place, all will admit that the expression of "breathing into the nostrils of man" is probably figurative, and intended only to imply a perfectly specific act of creation,—that nothing analogous to the creation of the vital forces and the soul had been performed till animated matter was created. The inspired writer, however, chose the most intelligible and emphatic mode of conveying the information,—since nothing is so familiar to man as that he lives especially by breathing. (2) And this, also, is another proof to our

(1) This and its analogies are the only instances in which it can be said that the act of creating did not involve the simultaneous production of the forces by which matter is governed. The whole work of creation was miraculous, and therefore is not connected by any analogies with the subsequent processes of nature.

(2) Hence, also, Hippocrates calls the air, the *pabulum vitæ*. He distinguishes aliment into three kinds,—victuals, drink, and air,—the last of which he calls *vital*. (*a*)

The manner in which the creation of man is announced is clearly intended to con-

(*a*) De Flatibus.

mind, that the inspired writer intended to be fully understood, and in that most obvious sense which reason dictates. He was speaking to the ignorant, as well as to the man of science. For a like reason, also, we are told that the sun stood still at the command of Joshua; and even in these astronomical days, we continue to speak of his rising and setting. And yet such is the consistency of man, the miracle has been denied, because science has discovered that the sun is always at a stand. Even now, should the earth's rotation cease, the expression of Joshua would still prevail.

Lucid brevity, also, is a sublime characteristic of the account of creation. It was enough that the account of man's creation should be fully stated, to enable the greatest skeptic to understand that the same life which appertains to animals and vegetables was created in the same way. Man was taken as an example of information on this subject, being the most perfect of created organization. The analogies amongst all their vital phenomena, and the equal disappearance of those phenomena after death, are so perfectly plain, that none can doubt the identity of the forces upon which they depend, (especially amongst animals,) or that they came into existence by analogous acts of their Creator. But we have, however, in relation to brute animals, exactly the same account of their formation out of the earth as in the case of man, the inspired writer seeming little disposed to leave any ground to the unbeliever. It is true, there is nothing said, as in the case of man, as to the successive steps observed in their creation. But it is just so in respect to woman, of whose creation there is nothing said in the way of repetition; the general plan having been indicated in the account of man. It is said, however, that she was made out of a rib of man, as this was a distinct circumstance, and illustrates very emphatically the relation which the sexes bear to each other. Man is also connected with animals by instinct itself, as well as by other analogies not less striking.

Even in respect to the vegetable kingdom, a remarkable analo-

tradistinguish the materials which compose his structure from the principles which animate it. His lifeless body was made of the earth; his living powers proceeded, as it were, in a more direct manner from the Almighty himself; and hence, again, the most intelligible mode of conveying the latter knowledge, was after the manner of man. Solomon understood the subject in this way; thus: — "then shall the dust return to the earth as it was; and the spirit shall return unto God who gave it."— Eccl. xii. 7. But we have already seen that the *spirit* and the *vital powers* were the united result of a single act.

gy prevails. Moses affirms the creation of "every plant of the field, *before* it was in the earth, and every herb of the field, *before* it grew; for the Lord God had not caused it to rain upon the earth, and there was not a man to till the ground."

We have said that man (and animals also,) was at first an *inanimate* apparatus. But had the forces of inorganic matter been adequate to carry on the operations of organized matter, man would have been a living body before the act of "breathing into his nostrils," or, in language divested of a highly expressive metaphor, before the act of creating his living essence. The physical forces, already existing, would not have been created anew for the special use of organized matter. This reasoning is only in conformity with the admitted fact, that the Almighty does nothing superfluous, — nothing that is useless. The vital forces of man, then, came into existence with his soul, as did those of animals along with instinct. And pursuing the descending analogy, we come to simple organic life, as manifested in the vegetable world. But here it is plainly modified, and the living forces are evidently of an inferior order; and as we descend along the inferior vegetables, we come at last to discover some resemblances betwixt the lowest forces of life and those which rule in the inorganic world. Still the disparity between the forces, as indicated by their effects, is at least as great as the difference between the lowest species of organic matter and inorganic; and the very differences are best denoted by the differences in the results of their respective laws.

The analogy, however, is very remarkable betwixt the forces of vegetable and of animal organization, — as we infer from the striking coincidences in their phenomena. But the modifications which exist have even their type in the highest species of organized matter; for there is the clearest proof, that the same forces are modified in different tissues of the same animal, and more remarkably so in different species of animals.

It appears, therefore, that there prevails amongst the organic forces of different species and varieties of organized matter, a series of exact variations, which constitute a chain of analogies adapted to the nature of the organized matter, more imposing in its designs and sublimity than those affinities which connect organization itself, or the materials of the inorganic world.

From the foregoing observations, it appears that we have noth-

ing analogous to the vital forces, animal or organic, in the simply material world. But it is otherwise when compared with the world of mind and instinct. Here are properties which bear some resemblance in their phenomena, (particularly through their associations with the vital forces,) with sensibility, sympathy, irritability, mobility,—all of which are distinct forces, and yet, as with the mind and its properties, it has never been imagined there are as many distinct species of matter. Nor are they, as we have said, exactly the same in any two distinct parts; having, indeed, a great variety of modifications. Sensibility is remarkably varied, as denoted by its adaptation to distinct and very different agents, in its relation to the different organs of sense,—to say nothing of the complex nature of the principles on which the reflex processes depend, and the involutions of sensibility and sympathy in those remarkable operations. (1) It is the same, more or less, with irritability, and organic contractility.

But, besides these specific modifications, the organic forces are liable to change from the action of external causes, just as the properties of the mind are liable to analogous influences when the organic forces of the brain become modified in the foregoing manner. These modifications, too, like the natural varieties in the organic powers, appertain to the particular parts upon which the foreign agents have made their impression, and do not shift from one to another; though, from the connection of the organic forces with the principle of sympathy, analogous modifications may be communicated from one part to another through the medium of this power; or, as we think it probable also, through sympathetic relations amongst the organic forces that exist independently of the nervous principle of sympathy. And without intending, in the least, to identify the immortal soul with the perishable vital forces, we may trace an analogy

(1) Müller goes into an elaborate inquiry, to ascertain whether "the nervous principle or force of the motor fibres is different in its quality from that of the sensitive fibres." It would appear that the difference is highly probable. As to the organic forces, he mainly inclines to the chemical theories. But he fully recognises the vital force of the nervous system, and objects to the opinion of a nervous fluid. "All these considerations," he says, "teach us that the existence of a regular circulation of the nervous fluid, from the brain and spinal chord through the nerves back to the central organs, cannot be demonstrated, and in the present state of our knowledge appears very improbable." (*a*)

(*a*) Elements of Physiology, vol. i. pp. 723, 727, 728.

betwixt them in the entire subversion of one property of the mind, and abolition of sensibility, or organic irritability, in one or more parts.

It is manifest, therefore, that we are not defending the doctrine of a "spiritus archæus," or an "intelligent soul;" but of various forces which are temporarily inherent in organized matter. Nor does this view exclude their dependence upon a common substance, such as we have endeavoured to describe.

The vital forces cannot be generated by matter, since upon them organization depends; nor by the forces of physics, since these are perfectly incapable of restoring the structure, or even its elementary composition, after the organized matter is decomposed; or, of reanimating the machine after decomposition has begun; whilst, on the other hand, these are the forces which lay waste the structure, and only so, after the signs of the vital forces shall have totally disappeared.

Again, if the vital forces be strictly material, like electricity, which abounds as much in the dead, as in the living body, it is impossible to conceive of their extinction, or, perhaps, even of death itself. It is not the soul which first leaves the body. There is a failure, and sometimes a near extinction of other forces, before the flight of the spirit. The mind may continue remarkably unimpaired in the midst of the ruins of the organic powers; and this is another proof that Müller, and his school, are wholly wrong in regarding the soul as the vital principle, so far as they acknowledge the existence of such a principle.

We have said there is nothing, perhaps, analogous to the vital forces in the inorganic world. We reach this conclusion through the only medium of knowledge, the effects of causes. We should have excepted a certain character appertaining to light, caloric, electricity, magnetism, gravitation, &c. These are so subtle, that it is undetermined whether they be really matter, or something different from matter; and this is one reason why they are called "imponderable substances." Imponderable *matter* would have been theoretical. Electricity, heat, and light, are capable, also, of penetrating the densest substances. Some consider them identical, whilst others suppose them to be little better than imaginary existences. Those who identify electricity with the nervous power, or with other forces of life, constantly avoid every expression that shall imply a materiality of the latter. In the very comparisons which they make, it is "nervous influence" which they

use; rarely, if ever now, nervous *fluid*,—whilst in the same paragraph stands in opposition the term electric, or galvanic *fluid*. There is everywhere manifested a suppressed consciousness, as it were, that the forces of life cannot be measured by those of inorganic matter, or by inorganic matter itself.

If the forces of life be essentially material, we must suppose, in the propagation of individuals, that they are divided betwixt parent and offspring; and that they must, in this way, either sustain a great diminution or extinction after many generations, or, that they become multiplied in every individual. The last of these propositions may be true, though very improbable; but if there be an alliance to immateriality, the fact which spirit supplies upon this question, will, at least, considerably simplify the subject. The soul is transmitted undivided and entire,—and we have reason to believe that such, also, is the case with the forces of life. The latter can only be true by admitting their analogy with spirit.

But in whatever sense we regard the vital, the physical, the chemical forces, and spirit, each has as much a positive and distinct existence as the matter which we see and feel.

We are fully sensible, however, that we cannot reach the absolute nature of the forces of life, any more than the forces of physics; and it is far less for the purpose of ascertaining their intrinsic nature, which, in an abstract sense would probably be useless, that we have made the foregoing remarks, than to show that the living system is governed by forces entirely peculiar to itself. Having no other object than the latter, we shall go on with our suggestions; and although we do not exactly hold, with Abernethy, that "our theories, hypotheses, or opinions, refer to one and the same act of the mind," we yet agree with him, that "they should be verifiable or probable, and should rationally account for all the known phenomena of the subjects they pretend to explain; and that, under these circumstances, it is allowable to maintain them as good, until others more satisfactory be discovered." We feel, too, that "no man, who thus theorizes, need feel shame in this employment of his intellectual powers; no man need feel arrogance, for it is acknowledged that his theory is but a probable and rational conjecture." (1)

If the theory of a nervous fluid, vibrations, &c., will best account for the phenomena of sensation, and other actions con-

(1) Lectures, &c.

nected with the nervous system, there are others equally remarkable, which are independent of that system, and therefore implying the existence of other causes. A principle, however, of the nature which we are endeavouring to illustrate may be common to the whole organism, and be associated with every part, as the soul is with every part of the brain. And if we rely upon the phrenological modifications of the mind in the different parts of the brain, which we only admit as true in a limited extent, we may have some conception how the vital principle is modified by the nature of the parts in which it is implanted; or rather, how it may manifest distinct powers in different parts.

We go, in the main, with Haller, Bichat, and Hunter; though we think the latter has exposed the doctrine of life to the misapprehension of many by figuratively imputing to the vital forces something like intelligence. Thus, he says, "the body, or parts of the body, have a *recollection* of former impressions, when impressed anew," &c. (1) though he evidently means no such thing in a literal sense. This is shown by the extraordinary, and almost absurd use he makes of the word *soul*, in speaking of mere physical powers. Thus, he says, "what is simply mechanical, that is, made of inert matter, must have, as it were, a soul, to put and continue it in motion."(2) These are unhappy figures of speech, since they are liable to reproach, or to introduce corruptions into philosophy. Mr. Hunter has, also, given offence by speaking of the powers of life in the aggregate, and calling them collectively, as we do, the *vital principle;* when it is obvious from the manner in which he treats of irritability, the principle of motion, sympathy, &c. and the modifications of inflammation, that his conceptions of the properties of the organic force were the same as understood by his eminent objectors. "I shall carry my ideas of life," says Mr. Hunter, "farther than has commonly been done. How far every part has an equal quantity of life, or the *powers of life*, is not easily ascertained." (3) It is remarkable, however, that they, who most deride Mr. Hunter's opinion of the vital powers, constantly employ the same expres-

(1) On the Blood, &c. p. 90.

(2) Lectures on the Principles of Surgery, p. 12.

(3) On the Blood, &c. Introduction, p. 3.

Bichat has been misapprehended in the same manner, as to his use of "organic sensibility." He gave to the term the same distinct import which others intend by irritability and excitability.

sion, or something like it, when speaking, in a general sense, of the phenomena of life.

The whole of our subject rests entirely upon the observation of phenomena; and these, for wise purposes, afford but little information as to the absolute nature of the causes, a knowledge of which would be of no practical advantage to man. "These are the mysteries of our God, which in his Divine Providence, farre do excell whatsoever things else, shewing man's wisdome meere foolishnesse." (1) Who shall resolve the problem, and tell us the nature of that principle through which an intense degree of cold is not only resisted for a time; but, by which, in the hibernating animals, the natural temperature may be re-established by the progressive operation of cold after the heat of the body shall have reached that minimum which threatens a destruction of life?

We are as much in the dark as to the essence of all forces, as Fontenelle's opera spectators, who only enjoyed the effect of the scenes and machinery, without seeing the weights and pulleys on which the different movements depended. (2) But we know the latter to be necessary to the effects which were observed, and we know also, that there must be different powers, when different effects will not result from a common power. Those effects, according to their extent, enlighten us as much in respect to the existence and nature of one power as another. Just so, "by the colour, figure, taste, smell, and other sensible qualities, we have as clear and distinct ideas of sage and hemlock, as we have of a circle and triangle." (3) In respect to the vital and other forces, the difficulty consists in our being conversant

(1) Woodall's Surgeon's Mate, p. 196. 1617.

"That the intelligence of any being," says D'Alembert, "should be able to reason, till he loses himself, on the existence and nature of objects, though condemned to be eternally ignorant of them; that he should have too little sagacity to resolve an infinity of questions, which he has yet sagacity enough to make; that the principle within us, which thinks, should ask itself in vain what it is that constitutes its thought, and that this thought, which sees so many things, so *distant*, should yet not be able to see itself, which is so *near*; that self, which it is, notwithstanding, always striving to see and to know; these are contradictions, which, even in the very pride of our reasoning, cannot fail to surprise and confound us." And yet, they who thus reason in relation to the soul, and "the existence and nature of objects," deride the existence of vital powers, because they are only known, like the soul and all things else, by their phenomena.

(2) Pluralité des Mondes, Conv. 1.

(3) Locke on the Human Understanding, b. 4. c. 3. sect. 26.

with matter, and our inability to appreciate the nature of force abstracted from matter. This difficulty would vanish, however, if we would regard the effects of force in the same way that we do the sensible qualities of matter, and we should have as clear an idea of the former as of the latter. We have, in the various degrees of spirit, an illustration of this principle; and in the forces of life, (to draw on other analogies,) we think that we perceive a connecting link betwixt the forces of inorganic matter and spirit.

There are no violent transitions in nature. The material existences pass gradually, as it were, into each other; (1) and so we have reason to think it is with the spiritual. Its gradations progressively increase from their lowest state in the inferior animals up to the angels, where it bears some affinity with God himself. The most exalted have been sometimes embodied with matter; and in man the association of spirit and matter is perfectly intimate, as, also, is instinct in animals. The forces of life are particularly concerned about sensation; and here, again, there would be a violent interruption of the law of analogy, were there not something intermediate betwixt spirit and matter, a bond of union as it were, through which impressions upon the senses should reach the spiritual existence. (2) We may fancy

(1) It is well said by the philosophical editor of Mr. Hunter's Lectures on the Principles of Surgery, that "it is highly probable that the different proximate elements of vegetable and animal substances hold different ranks in the scale of organized substances, in the same manner that one animal ranks higher in the scale of organized beings than another. Thus, starch, sugar, and woody fibre among vegetable substances; and gelatin, albumen and fibrin among animal substances, are *raised*, as it were, from one to the other by successive processes in the animal and vegetable economy." (*a*) Mr. Hunter has the same idea; and we quote it especially as the fact shows the great improbability of chemical agencies in the functions of life. In the process of vivification, "vegetable matter," says Mr. Hunter, "goes through *intermediate* stages, in which it is neither vegetable nor common matter; the same with animal matter." So we should arrange in the foregoing order, blood and gastric juice. The latter becomes more highly vitalized by the organic force of the capillaries; and by its action on dead organic matter, it performs the first step in the process of bestowing the organic force, or life.

(2) Something analogous to this appears to attend the moral discipline of the Governor of the Universe. It may be illustrated by an example. "In God," says Haller, "all is order, and that in the greatest perfection." "The inseparable connection which there is betwixt bad actions and the natural consequences of the disapprobation of God would be broken, were an act of his grace to enlighten and sanctify immediately a depraved and uninformed mind." (*b*)

(*a*) p. 14, *note*.

(*b*) Haller's Letters to his Daughter.

it to be electricity; but this no more aids the understanding as to the communications from matter to the immaterial existence, than if we regard the nerves, *per se*, as the instruments.

There is a property, however, appertaining to the principle of life, which appears to us to be perfectly intelligible in its function, if we allow its phenomena to carry their proper weight. This is the power of sympathy, forever in operation in all organized matter. It forms a bond of union betwixt the other forces of the vital principle, betwixt those forces throughout the system, and possibly betwixt them and the soul. It connects matter with spirit. It is an intercommunication which should not be overlooked in considering our suggestions on the subject of progressive analogies, from organization up to the mind; these last constituting the extremes.

It may be said, that it is the tendency of the foregoing doctrine to assign an immortal spirit to brute animals. We think not; although we cannot doubt that the substance on which instinctive actions depend, is immaterial. It is the first link in the great chain of spiritual existences; and agreeably to the law of ascending analogies, the manifestations of a spiritual essence in animals should be very low; and this, too, progressively decreasing as we descend along the great chain of animation. But the extinction of any thing will not effect the principle of analogy in its relation to existences. (1) We know nothing of the order of Providence, (excepting what is revealed,) after the system of ascending analogies amongst existences is broken up; and all reasoning from analogy is then at an end.

The analogy betwixt mind and instinct is inferred from the coincidences in many of their phenomena, and even from the subservience of the brain to their respective functions. Other-

The supposition of an *immateriality* of the vital forces which has been made by some, but which we do not defend, has been often ridiculed, but never opposed by a fact or a reason. Mr. Lawrence is amongst the most hostile; but he takes the same occasion to insist upon the materiality of the soul, and to misrepresent Mr. Hunter's views upon the subject of the vital powers. (*a*)

(1) Perhaps we ought to say, without any ambiguity, that we have no faith in the strictly reasoning powers of animals. They have no association of ideas; and one generation never lays up the gains of the past, or makes any improvement upon them; and as to the duration of their spiritual existences, we believe they totally end, when animals have answered their final cause in subserving the uses of man,—just as the vital forces become extinct when their intention is fulfilled.

(*a*) Lectures on Physiology, &c. p. 73, *et passim*.

wise, the higher animals having a brain similar to that of man,(1) did we not suppose some spiritual essence connected with it, analogy would be so strong in this case as to establish exclusive materialism in respect to the spiritual functions of man, which are so manifestly connected with the cerebral system. If the brain be necessary to the operations of the soul in man, and evidently designed for this special purpose, it would be an absurdity to suppose that precisely the same organ has been given to animals for a different purpose, or that it should be capable of any independent functions which do not appertain to the same organ in the human species. Again we see the same passions, and other instinctive operations, in animals and in man, are alike affected by affections of the brain. We see, also, that the instinctive powers of animals may be improved, to a certain extent, like the intellectual powers of man, and by the same methods. But we have only connected this inquiry with our subject, to illustrate the universal law of ascending analogies in every department of created existence, — running, too, from one department into another, — and to obviate objections to our conclusion as to the intermediate nature of the vital forces betwixt spirit and matter, — since it is manifest that animals, as well as man, are governed by the same vital powers.

This part of our subject naturally leads us to speak, again, of the complete extinction of the forces of life, when the spiritual leaves the material part, — and of the impossibility of supposing the extinction of purely material forces. But we have, in the extinction of the instinct of brute animals, a complete exemplification of the annihilation of spiritual forces; — and this, to our mind, is an index to the nature of the vital powers.

We are, however, so much the creatures of sense, that we shall probably still go on explaining everything appertaining to life by some tangible or visible cause, or by some laws with which we fancy ourselves to be better acquainted. We have already cited several examples; and if we take up any writer, indifferently, it is more than an equal chance that our authorities will be increased. Thus, here is Sir G. Blane's Medical *Logic:* "The changes," he says, "accomplished by the actions of

(1) It is true, there are certain anatomical differences; but they are no greater than might be inferred, *a priori*, from the differences betwixt reason and instinct. Sœmmering discovered fifteen betwixt the brain of the tailless ape and that of man. But the general character is the same.

life may be conceived to be effected through the agency of some imponderable fluid; such as electricity, light, or magnetism. We may conceive, for instance, that each gland may be furnished with a sort of voltaic apparatus for effecting its specific change." (1) But did any of the foregoing substances ever reproduce the phenomena of life in a dead subject, although the organized structure, as we have shown, may remain unimpaired? It is mere assumption to answer a speculative and unintelligible hypothesis, to affirm that structural derangement is necessary to death. If galvanism be the immediate cause of the deposition which constitutes the interstitial growth, what bestows vitality on the new formed matter? Or, if this vitality be imparted by specific powers of the formative instruments, why should not those powers be adequate to the entire transmutation? And if equal to such results, why not to all the purposes of life? Is there any philosophy in the unmeaning multiplication of causes, — especially when the superadded ones will not explain a single phenomenon, or a single result appertaining to living organized matter? "What," says the same elegant writer, "can be more incomprehensible in the whole compass of nature, than the art by which that conversion and secretion of elementary particles is effected, by virtue of which, bone, cartilage, muscle, membrane, and every other form of organized animal substance is created, at the very point of time and space, in which this conversion and secretion is called for, and then and there moulded into the form of the respective organs which they constitute; that these organs should be precisely adapted to the ends of nature, — the growth of each keeping pace with all the others; and that these unvarying processes of growth and repair, should go on with such harmony on both sides of the body, as to produce that correspondence and symmetry which we behold!" (2) And yet, electricity, or magnetism, or light, are called in to explain this mysterious process; whilst their operation in the inorganic world does not supply a single analogy; and all that is known of living matter exhibits its independence of the laws of chemistry and physics.

We have thus extended our remarks upon this important subject, as we cannot believe that there can be any correct philosophy in physiology, any sound inductions in medicine, or any rational practice of the healing art, without a correct understanding of the powers of life. All our inquiries upon these subjects,

(1) Medical Logic, p. 59. (2) Blane, ut Supra.

turn wholly upon the vital forces; — and he, who shall regard them as coincident with the powers that rule in the inorganic world, must, as it appears to us, travel a route upon which he will be forever losing his way.

There can be no practical adherence to the phenomena of nature, where inductions are wrong as to her laws. The mind will be diverted by its false philosophy, and the shadow will be preferred to the substance. We could mention many examples in distinguished quarters; such, for instance, as the application of galvanism in the treatment of disease, upon the ground, either of its "identity with the nervous influence," or of its constituting the *materia vitæ*. These fundamental errors lead to a thousand others, which, though more partial, swell the amount of corrupt philosophy and practical mistakes. It is the main cause of those speculations which have led to the treatment of local inflammations and inflammatory fevers by tonics and stimulants. It is the modern foundation of the humoral pathology, and the consequent treatment of "the worst forms of fever by table salt." (1) This doctrine, however, as we shall see, was comparatively innocent, when nature was regarded according to her phenomena. It had its origin in other considerations than such as relate to fundamental laws. It was a mere pastime with the ancient masters; or, if it received any practical indulgence, it was generally kept in subordination to the dictates of nature. In another place, we shall see that the physical doctrines of life have led to the hypothesis of spontaneous generation, and have laid a foundation of infidelity.

It is only within a few years, that the doctrine of the united agency of spirit and matter, in the production of thought, has begun to prevail, because philosophers were unable to conceive, and still more unable to demonstrate, the connection. The difficulty was, the spirituality of mind divests its phenomena of all sensible appearances, and is itself the agent by which its own properties are investigated.

If, then, matter and spirit may thus mutually act on each other, there will be little difficulty in admitting that sensibility and sympathy may exist in the nervous system in a state analogous to that of spirit, and as readily explain all their phenomena, as that the reciprocal action of mind and of matter should

(1) See Essay on the Humoral Pathology.

determine the phenomena of thought and of muscular motion. That the primary impression in the function of sensation depends on such a principle seems probable, also, from the manner in which the great spiritual part is connected with the process, and from its consummating the function. The analogy may be thus extended to sympathy, and to all the vital powers; or, we may suppose that the same Wisdom, Which could contrive such an intimate connection between mind and matter, may have created in the properties of life an intermediate substance partaking of both, by which their relations may be more analogously combined. Nor would this arrangement be the less admirable from the circumstance of the coexistence of its several parts. And there appears to be less difficulty in supposing that the agents may produce their direct impressions on the intermediate principle which we have indicated, than that the first impression is wholly on some material substance, like the electric fluid, which is then sent to the brain, and there exerts an effect on an immaterial principle. The latter hypothesis, indeed, appears to be perfectly unintelligible, — since it is neither illustrated by fact nor analogies. Our intermediate substance, on the other hand, is recommended by the relations of mind to matter, and the great chain of analogies, and is peculiarly adapted to establish a connection betwixt those extremes.

If there be any ambiguities attending the subject, are there not equally others in relation to all the principles of matter? But the difficulty is always recurring, that some sensible agent is wanting for sensible phenomena. In all the functions of a physical nature, the operation of causes, — the consecutive motions, — the final product, — and their relations to each other, are more or less subjects of demonstration by the senses. The heart pulsates, and the effect is always a movement of the blood. This process is so apparently mechanical, that it is comprehended by the most common intellect; however it may overlook the essential springs upon which it depends. But, as we follow the courses of the blood to its ultimate destinations, we become as much entangled in the labyrinth of physical changes, as we can possibly be in those of the vital phenomena. The mind passes the interval of unknown operations, and connects the physical results with primary motions, as the seeming relations betwixt cause and effect. And yet, the intermediate processes were those on which all the consecutive phenomena depended, — which involved the operation of properties of whose nature

and modes of action it is impossible for imagination to conceive, but through the vital signs. These are every where numerous, diversified, and imposing; and they supply an internal proof, if we may say so, of the specific nature of their forces, and the peculiarities of those forces, which appears to us irresistible. There are often certain facts which alone address themselves to the understanding, that carry a conviction which no sensible demonstration can produce. This is often exhibited in our courts of justice,—and a striking exemplification may be found in those analogies amongst the languages of the human race, which carry all nations back towards a common family.

Will it, then, be said that there is no intelligible connection betwixt our vital properties, and the motions, and the physical results of organized matter? What intelligible connection is there betwixt the properties of the soul and the motions of the brain? What intelligible connection betwixt the stimulus of the blood and the motions by which bile is generated, unless you admit the peculiar forces for which we contend? What intelligible relation subsists betwixt the action of the liver and the production of bile; or, what betwixt an excitant of the senses and the sensation that follows,—unless the same admission be made? A common answer must, obviously, be given. They exist towards each other as cause and effect,—but wholly unintelligible without a primary agency, which is an antecedent cause; and however impossible it was for the mind of a Newton to grasp the nature of that cause, he endeavoured to expound the affections of the nervous system, which he had no doubt were essential to all thought and sensation. But the oscillations of Newton, the contractions of Darwin, the vibrations of Hartley, the secretions of Galen, the galvanism of Galvani, the indefinite forces of the chemist, and the caloric and the magnetism of wilder imaginations, serve only to show, by their great incongruity, that the hypotheses are founded on imaginary data, and that each has neglected the phenomena of life.

With all the advantages, then, of subjecting our organs, and many of their motions, to the examination of our senses, what more do we know of their nature, their causes, their modes of operation, and their products, than we have learnt even respecting the mind and the brain, and their separate or united phenomena? It is chiefly by their effects, in either case, that we endeavour to ascend to their probable causes. Who will demonstrate the nature of those physical properties by which foreign

agents produce their impression on the powers of vitality? And yet so nice is our discrimination amongst them, as prompted by the vital signs which they produce, that it is one of the most important objects of the physician to select from the great multitude of cathartics, of emetics, &c., a certain species whose properties shall best correspond with the modified signs of vital action; and it is no unusual phenomenon, that of the whole range before him, he decides with accuracy, that there is one only which is well suited to the case. And his conceptions of the specific properties of the agent, and those of the organization, even in their modified state, are so comprehensive, that he may foretel their united result. He knows as much of the properties of one as of the other; and that he admits their existence and specific nature is manifest from his deliberate action. Whoever prescribes for disease upon any other ground is a mere charlatan.

Finally, the phenomena of life are as easily comprehended as those of inorganic matter, and denote as clearly, or even more so, the nature of their causes. Who, indeed, will define the nature of cohesion, gravitation, chemical affinities, &c.? Like the properties of life and of spirit, and their relations to matter, their existence is only inferred from certain uniform phenomena, and from such, alone, we deduce their relations to objects of more sensible demonstration. We reach the connection betwixt common matter and its properties, — betwixt the vital powers and organized structure, — betwixt the intellectual and moral faculties, and the nervous system, — the concurrence betwixt them in the production of certain effects, — and the differences in the nature of the several powers, by a common process of observation. There are mysteries attending the same conditions of the whole which we must leave to the sole comprehension of the Great Author Who intended the whole to subserve the purposes in which we are alone interested; Who has wisely secured to Himself the nature and control of first causes; — and Who has thereby restricted our inquiries to the only useful end of knowledge, their various phenomena. These may be so employed, as to answer the wants, the conveniences, and the various purposes of intelligent beings. Those are the springs of action which it might be unsafe for man to understand.

In mechanical philosophy and in chemistry, we are contented with the simple observation of phenomena, and leave unattempted the nature of the properties which they imply, or the modes

by which they operate. When we see two aerial substances result by their admixture in a solid form, we ascribe the phenomenon to the property of affinity, and are rather contented with our ignorance of its nature, than impelled to its investigation. We see that electricity is concerned, but we know not how. We see, however, that there is another force, which we distinguish from electricity, gravitation, the forces of life, &c., by the effects of each. If we extend an elastic substance and then allow it to contract, we infer that the contraction depends on another distinct property; but whose nature it would be folly to explore. When we regard the "music of the spheres," we know that there is a principle which forever operates with uniformity in sustaining their relations; and by multiplying and comparing analogous phenomena, we find its influence extended to all matter; but however adapted to the ambition of man, he has never ventured to explore its mysteries. As we ascend to organized matter, and arrive at a being to whom belongs not only physical properties of admitted obscurity, but others on which especially depend the phenomena of life, the subject has acquired a grandeur too imposing for the submission of curiosity. Defeated, however, in his attempt to unfold the nature of the vital forces, the chemical physiologist, with the most unaccountable inconsistency, rejects all the clear indications that are supplied by the vital signs, and gives up to the feeble analogies of his own science. Others, again, not contented with establishing the distinctions between the vital properties and the common forces or matter, have endeavoured to expound what the mechanical philosophers have despaired of attempting in a less complex and more tangible subject; or, where imagination has preponderated, it has laid hold on the prerogatives of the poet, and struck into existence the machinery of intelligent agents. They have thus exceeded the just limit of inquiry, which only ends with the results of causes. There is nothing in the organic forces to justify the inductions as to their intellectual nature,— as little, we think, to show their absolute materiality, — whilst we believe we have offered some good ground for our conclusion that they hold an intermediate rank betwixt spirit and matter.

Again, as we rise still higher among abstract principles, and reach the great labyrinth of intellectual existence, the mind is employed on its own constitution and endowments, and finds a subject replete with irresistible sublimities. It is now concerned

with its own properties, its modes of investigating other subjects, and is better contented with the restraints that may limit its extraneous knowledge, than with any ignorance of itself. The great principle which stimulates inquiry is now in full operation, and the mind pushes on with the hope of reaching the consummation of its desires in discovering the nature and the cause of its own being. Hence has arisen the philosophical science of metaphysics, and its various modifications, till they disappear in the doctrine of materialism. But it has been the fault of metaphysics to carry abstraction so far as to insulate the mind from the body, — to consider the intellectual properties, and their operations, too independently of organized matter; and the science is thus encumbered with mystery and discouraging paradoxes. But how strong the temptation to step beyond the legitimate pale of science, — how constantly analogies fail of their influence, and we grasp at the most latent conditions of spirit. One speculator contends that it exists as an independent agent, because matter can only be compatible with matter; another, that such is its separate state, because he can see no necessity for its physical union, — that it resides in the brain, or is disseminated in the body, roves through the nerves, or is shut up in the pineal gland, and presides over the organic movements, as over extraneous objects; another descants on the elements of which it is composed and resolves them into separate existences, because he does not see an affinity among them, — no necessity for their co-operation, — or, this distribution may be better suited to some plan he may have made as to the organization of the brain; — whilst another, like the rest, satisfied of their existence, yet acknowledging his ignorance of matter, whether dead or living, and admitting the distinctive forces by which they are governed, declares that he cannot comprehend the nature and the properties of spirit, and therefore pronounces it all a chimera, — that the phenomena of mind are nothing more than the results of that matter of which he also professes an absolute ignorance.

The facts, then, being conceded, that we know not how one species of matter unites chemically with another, or how the needle traverses the compass, or how the electric fluid lights up the north or lies concealed in the clouds, or how the component parts of matter are held in juxtaposition, or its separate masses retained in their places, — all which, however, we know are determined by special causes, from constantly observing the rela-

tion of phenomena, — we are thus prepared to understand that the objections to the united action of spirit and matter are untenable, and that thought, every act of intellection, our sensations, our passions, are the result of their co-operation. The one is as manifest as the other; and the same method of proof which demonstrates the existence of the foregoing facts, conducts us to a like conclusion as to the forces of life.

Mr. Hunter has some philosophical reasoning from affections of the mind to those of organization. "The actions of the body, and the cause of these actions, with their effects, are exactly similar to those of the mind; and as we are sensible of the actions of the mind themselves, abstracted from their causes or effects, we reason about them as much as we can reason about their causes and effects. This is not the case with the actions of the body, — for in them we are only acquainted with the causes and effects, not the action itself; therefore our minds are only reasoning about them from analogy."(1) This mode of reasoning we are constantly and successfully employing in the science of medicine; which, more than any other, is made up of inductions from analogy. What has been said by Mr. Hunter, in relation to analogies supplied by the mind, is a solid ground of inference in regard to the character of the vital forces; and whether we have contributed any light, is a matter to be determined by others. It may be safely affirmed, however, that whilst no undoubted analogies exist betwixt the phenomena of inorganic and those of living organized matter, they are not a little imposing as they respect organized matter and the mind.

We conclude the foregoing essay in the language of Bichat: "Examine all the physiological and all the pathological phenomena, and you will see that there is no one which cannot be ultimately referred to some one of the properties of which we have spoken. The undeniable truth of this assertion brings us to a conclusion not less certain in the treatment of diseases, viz. that every curative method should have for its object the restoration of the altered vital powers to their natural type."(2)

(1) Lectures on the Principles of Surgery, lec. 9.
(2) General Anatomy, applied to Physiology and Medicine, vol. i. p. 17.

APPENDIX.

(See page 68.)

It being our purpose, in our several essays, to examine their subjects as critically as we may, this plan necessarily involves considerable detail, and a statement of many facts with which our readers may be familiar. We have particularly felt the importance of protecting our conclusions as to the vital powers by all the facts and argument we might command, since the odds, as it respects the opinions of philosophers, are so greatly against us. Nor would it have at all comported with the deep respect which we bear for their learning and genius, to have set up conflicting views, without a full exhibition of our reasons. We think that we have not even yet said all that may bear us out in our doctrines of life,—especially in relation to galvanism and electricity. The identity of this fluid with the nervous power, or the "nervous fluid," as it has been often called, and its special agency in the vital and physical results of the living system, have been ably maintained by numerous philosophers; among the most eminent of whom are Galvani, Volta, Aldíní, Michælis, Ritter, Saucerotte, Barther, Rossi, Bellingeri, Humboldt, Valli, Monro, Fowler, Wollaston, Home, Rolando, De la Metherie, Ampère, Schweigger, Pfaff, Kellie, Becquerel, Ærsted, Breschet, Brodie, H. Davy, Prévost, Dumas, Philip, &c. Others, including some of the foregoing, it is well known, have advanced the doctrine that the galvanic or electric fluid is generated by all parts of the body, even by the globules of blood, and that it is the great agent in forming the products of vital actions. We shall, therefore, consider some of the remaining objections in this appendix.

Drs. Philip, Billing, (1) and most others, who defend the identity of the nervous influence and galvanism, carefully avoid speaking of a nervous *fluid*,—always calling it the "nervous influence," whilst they speak of the electric *fluid*. Indeed the latter term is employed by Dr. Billing in announcing their identity;

(1) Principles of Medicine, p. 17.

but *fluid*, as applied to the "nervous influence," is evidently obnoxious,—grating to the ears,—at least of the critics. And yet Dr. B. does not hesitate to speak of the "nervous influence being discharged into the muscles from the nerves; and this discharge may be produced in a variety of ways, as by the blood in the heart or capillaries, the presence of food in the intestines," &c. (1)

A most important fundamental inquiry has been wholly overlooked in the experiments and conclusions, which relate to the foregoing subject. There has been no attempt to show the existence of that imaginary substance the *nervous fluid*,—for fluid it must be, if identical with galvanism.

It must be also borne in mind that experiments are not always to be trusted, where they are intended for the support of some favourite hypothesis. Thus, Dr. Philip states that in twenty-two cases of dyspnœa, he had given decided relief from the paroxysm by transmitting galvanism from the nape of the neck to the pit of the stomach. (2) On the other hand, Dr. Ramadge states that he has repeated these experiments in numerous cases, at two infirmaries for diseases of the chest, and that "there were only two of them in which any benefit was obtained from its use." (3) "It may be fairly said," Dr. Forbes observes, "to have failed in asthma." (4) Electricity and galvanism have been long renowned as remedial agents in the hands of those who regard them as essential in the processes of life; and with another class on account of their marvellous effects upon inorganic matter. But they have been most thoroughly tried in extensive hospitals, and in private practice; and it may be affirmed that they have been found almost wholly wanting, even in those affections, (as in palsy, defective secretions, &c.) where, if in any, they should manifest their *vital* energies.

Numerous experiments might be quoted to show that galvanism acts only as a stimulant. Koestlin, (5) and others after him, have shown that the electric fluid hastens the hatching of eggs, and promotes the germination of seeds. Is it not analogous, in

(1) Bichat says, "physicians generally attach no precise idea to the term *nervous influence;* the habit of experimenting shows how much they have abused it." (*a*)

(2) On the Laws of the Vital Functions.

(3) On Asthma, p. 147.

(4) Cyclopedia of Prac. Med. Art. Asthma, p. 205.

(5) Diss. de Effectibus Electricitatis, &c.

(*a*) General Anatomy, vol. 3, p. 223.

these instances, to the operation of heat? It is a vital stimulus, and therefore can be no more allied to the principle of vitality than any other foreign agent. "The heart," says Dr. Phillip, "may be seen performing its functions in the chick, in ovo, when no vestige of brain or spinal marrow can be traced." This was one of the numerous instances in the way of the hypothesis, and which he attempts to explain by another, suggested by an equal difficulty attending the development of the human fœtus born without a nervous system. "If the nervous *influence*," he says, "be galvanism, there *may be* some apparatus in the uterine system, for collecting and supplying this agent, till the brain and spinal marrow can perform their functions, and which may continue to supply their place where they never exist." The demerit of this explanation consists in its want of all foundation, and its application to another hypothesis which is equally deficient. But is there probably, also, the same galvanic apparatus in the egg of a fowl, to supply the temporary want of brain? Or rather, if galvanism be the nervous power, and will hatch an egg, should it not follow that the principle by which the hen hatches is equally the nervous power?

There has been some emphasis upon the supposed agency of electricity in developing the phenomenon of animal heat. It is true, like many other irritants, it will produce an increase of temperature; but this is done, in an unknown manner, by their action on the vital properties of the solids. Still, it no more proves the identity of electricity and the nervous power, or any other vital power, than it would appear, in the other case, that electricity, heat, and every irritant capable of exciting vascular action, are identical; or that heat, alkalies, &c. are identical with the nervous power.

But it is more probable that the effect of electricity in raising the temperature of the blood, when removed from the vessels, (1) is only indirectly connected with its vital properties, and that it mainly depends upon chemical influences. The blood, being removed from the body, can acquire no real accession of caloric. What is developed was in a latent state. By some chemical influence, the capacity of the blood for heat is diminished, and its extrication follows. This, it is true, is said to have taken place

(1) See Philips's Experimental Inquiry, &c., p. 175, and Scudamore on the Blood, p. 54.

only in arterial blood. But it only shows the probability of Dr. Davy's conclusion that arterial has a less capacity for caloric than venous blood, (being in the ratio of ·903 to ·913,) and parts with it more readily. (1) Besides, since we think it must be that animal heat is a secretion, and, therefore, requiring the agency of the vascular system, there can be no analogy betwixt its generation in the body and out of the body.

Again, the induction from the foregoing experiment would wholly deprive the solids of any participation in the production of heat; and would afford us the unique phenomenon of an important vital function going on in a fluid whose vitality is questioned, and this, too, after its entire separation from every part that has contributed to its formation. But Dr. Philip remarks, that galvanism "ought to occasion an evolution of caloric, as it effects the formation of secreted fluids,"—by which the dependence of animal heat on the solids is admitted; and, of course, if true, the experiment must fail from the nature of the premises.

It is also said by Raspail, (2) and Davy, (3) that heat is either not evolved during the coagulation of blood, or that it actually falls. Still electricity may promote such mechanical or even chemical changes in the blood out of the body, as to elicit a development of caloric.

But one affirmative experiment is as good as another, when both are simple, and conducted with care. Girtanner, (4) and others, have raised the temperature of the *venous* blood "several degrees," by combining with it ogygen gas. (5) Which of the theories shall we adopt; or, are oxygen, galvanism, and the nervous power identical?

"Before we can expect," says Dr. Philip, "that the nervous *influence* can be made to pass through any other conductor than that to which it belongs in the animal body, there must exist a powerful cause soliciting it to some particular point. In a muscle of voluntary motion, there can be no such cause. The nervous influence is not attracted to the muscle; it is sent to it

(1) Philosoph. Trans. 1814.
(2) Nov. Sys. de Chim. Organique, p. 361.
(3) Journ. of Science and Arts, No. 4.
Gordon says heat is evolved. Annals of Philosophy, vol. 4.
(4) On the Principle of Irritability.
(5) See our Essay on Animal Heat.

by an act of the sensorium, carried into effect by the powers of the nervous organs, which are subjected to its influence; those organs, which, on the one hand, prepare that influence, and those which, on the other, convey it when duly prepared. The muscle is altogether passive till the influence is applied to it. But the case is wholly different with respect to theseat of many of the functions of the ganglionic system. We know from direct observation, that in many of them there is a cause constantly operating which solicits the nervous influence to their organs." (1)

However true may be the foregoing facts, they show no analogy betwixt the laws of electricity and the nervous influence; nor will the former at all explain the remarkable modifications of sensibility, muscular motion, and the phenomena of the ganglionic viscera, that take place in the normal state of the body, and under circumstances of disease. How will this theory answer to the reflex function of the nerves? How will it explain the various phenomena obtained by Dr. Reid, (2) and others, in their experiments upon the par vagum, the glosso-pharyngeal, and the pharyngeal nerves; or those which depend upon the more simple arrangement of the spinal nerves of sensation and motion?

All that is said, however, in the foregoing quotation about galvanism being attracted or sent from the brain to particular parts,—that "there *must exist a powerful cause* soliciting it to some particular point,"—that "in a muscle of voluntary motion there *can be no such cause*," *&c.* is, certainly, the merest conjecture,—a perfect begging of the question. But since there is nothing apparent in the arrangement of the cerebral apparatus, more than in other organs, by which galvanism may be generated, and nothing in either especially analogous to a galvanic battery, we see not how such a function can be philosophically ascribed to the brain, without some better proof. And then, as to the supposed conductors, are they any better than muscle or any other tissue? Can the galvanic fluid be transmitted along a nervous cord in a dead limb, without being equally distributed to other tissues? That it appears to follow the nerve exclusively in a living part, (not involved in a circuit,) is owing to an il-

(1) London Med. Gazette, March 1837, p. 930.
(2) Eding. Med. & Surg. Journ. Jan. 1838.

lusion that grows out of its manifestations as a stimulant to the vital forces. If we will to move a finger, — admitting the mind to have the power of discharging the cerebral battery, — why should not the whole arm be moved, and other muscles which are supplied by nerves connected with those of the arm; — or, why, indeed, should not the galvanism discharged from the brain pervade, equally, every part of the body? By what assumption shall the mind limit its direction to an individual part? The nerves have no insulating power; and what especially proves that galvanism is not the nervous agent, is the interruption of the nervous power by a ligature around any nervous cord. So when galvanism is passed transversely through a nerve, the muscle contracts, although the galvanism do not pervade the nerve.

Dr. Edwards, for the purpose of showing that the galvanic or electric fluid is made to traverse the nervous system, produces the discovery of Vauquelin of the fatty matter, which not only surrounds each of the fibres, but invests the entire trunk of the nerve, — "exhibiting itself in its most minute ramifications." "The insulation of the nervous fibres is produced by this abundant fatty matter." He then reaches the conclusion, that "it will be obvious, that by means of these provisions, the electric fluid, which has arrived in the nerve, cannot deviate to take a different route." (1)

This may all be well in theory; but like most other writers whose theories do not repose on facts, our author soon exhibits the fallacy of his own. He wishes to show that the muscular contractions, which are produced by bringing a solid body in contact with a nerve, depend on the development of galvanism. This fact he sufficiently establishes; but in doing so, he at the same time shows, that the fatty substance surrounding the nerve is no obstacle to the escape of the galvanic fluid to the adjacent muscular substance. To succeed with the experiment, he was obliged to insulate the nerve; for "when it reposed on the subjacent flesh, he was unable to produce contractions," "on account of muscle being an excellent conductor of electricity." (2)

This coincides with our author's affirmation, that "it appears possible artificially to imitate the principal conditions of the se-

(1) On the Influence of Physical Agents on Life, p. 304.
(2) Ibid. pp. 311, 312.

cretions, and to separate from the blood, by means of the pile, a liquid resembling milk, and from the food itself a material resembling chyme." (1) In the exercise, therefore, of a natural right to protect the cause of truth and of science against such invasions, we ask whether we are not entitled to the conclusion, that chemistry has made its inroads upon the science of life by expedients that are unworthy of its lofty, and otherwise noble bearing; — whether we shall not suspect that all its inductions in relation to life are imbued either with the artifices of imagination, the zeal of ambition, or the more reckless disregard of facts?

The foregoing phenomenon of muscular contraction, as stated by Dr. Philip, (2) and as universally known, takes place when mechanical or chemical irritants are applied to a nerve.

The hypothesis of Prévost and Dumas, that all mechanical and chemical stimulants, and heat, operate by developing electricity, is without a fact to sustain it.

We may also say, that according to Bichat, and many other physiologists, as we shall have occasion to see more particularly in another place, galvanism generally fails to excite contractions in the muscular tissue of organic life. An important objection to the theory of galvanism, as applied to the nerves of animal life, is stated by the older Monro, (3) who remarks, that a muscle is never brought into action by the nervous power, except when transmitted from the brain to the circumference; whilst muscles are convulsed when the electric fluid passes from the smaller branches into the trunks of the nerves.

The incompatibility of the galvanic theory with the phenomena of sensation and muscular motion, especially when variously modified by affections of the brain, is too obvious for any comment. The history of neuralgic affections (4) presents, also, a series of phenomena, especially when regarded in connection with their remote causes of malaria, cicatrices, injuries, local irritations, &c., which must discourage the greatest adept at galvanic philosophy.

As to anything like positive demonstration, the hypothesis, whether it relate to the identity of galvanism and the nervous

(1) Influence of Physical Agents on Life, p. 305.
(2) Gulstonian Lectures, lec. i. 1335.
(3) Experiments on the Nervous System, &c,
(4) See Rowland's Summary "Treatise on Neuralgia," in Dunglison's American Med. Library. 1839.

power, or any other powers of living matter, is opposed by the entire failure of Person, Pouillet, Müller, (1) and others, to detect electric currents in the nerves, or in any other part. Müller also states, that "with the galvanometer, no electric current can be discovered in the blood. I perceived no variation of the magnetic needle of the multiplicator, even when I inserted one wire into an artery of a living animal, the other into a vein." (2)

The argument drawn from the electrical eel, &c., is no more in favour of the foregoing doctrine, than if derived from the characteristic secretion of the cuttle fish. The electrical apparatus of the former has no more relation to the general economy of the animal, than the ink-bag of the latter. The electricity is not supplied by the brain, but is the product of a peculiar apparatus. (3)

(1) Elements of Physiology, vol. i. p. 72. (2) Ibid. p. 135.

Müller does not believe that the globules of blood generate galvanism. But he says, "should any physiologist be so fortunate as to prove beyond doubt the electric property of the blood, I could only congratulate science on the great advance which it would thus have made." (p. 134.) It may be expected, therefore, that it will soon be "proved." But we do not see how science will have gained — since the galvanic theory will always remain contradicted by the phenomena of life.

(3) We cannot forbear noticing here a very extraordinary case of animal electricity, reported by Drs. Mussey (*a*) and Hasford, (*b*) — the latter having been the attending physician.

The subject of this phenomenon was a lady of thirty years of age. On the evening of one of our brilliant auroras, she "discovered that an electric spark passed from her fingers, elbows, or any other part of the limbs or body, when brought nearly in contact with conducting substances." "In a minute, four strong sparks, each *an inch and a half* in length, were elicited." "On the return of her husband, who had been absent, she met him *at the door*, and playfully presenting her finger to his face, astonished him with her newly acquired power." This faculty lasted about twelve weeks, "when it was entirely lost."

Dr. Mussey did not witness the case; but it appears to be well authenticated, — though not more so than the cases of animal magnetism which abounded in the eastern part of our country about the same time, and to which respectable physicians bore their undoubting testimony.

There are several points worthy of consideration in the foregoing case.

1*st*, The obvious one of its entire want of analogy in the human subject, — since in all the cases supposed to be allied, the generation of electricity obviously depended upon the clothing of the subjects, and the temperature of the air. Where sparks have been seen to pass from the extremities of divided nerves, the experiments were made with the expectation of this result; and "sparks in the eye" are amongst the most common optical illusions; and we have it on the authority of Zimmerman, that "the desire of seeing a thing occasions us to see it every where."

2*d*, The impossibility of accounting for the accumulation of the fluid without supposing an entire change in the nature of the body.

(*a*) American Journ. of Med. Science, vol. xxi. p. 377.
(*b*) American Journ. of Science and Arts, January, 1838.

Rudolphi (1) has supplied a very elaborate account of this apparatus as it exists in the gymnotus, and silurus, — and there is nothing analogous to it in any animal excepting those which are universally considered electrical. It is as much a specific apparatus for the generation of galvanism, as the liver is for that of bile. And since this peculiar contrivance is, *prima facie*, indispensable to the production of the galvanic shock, and provided

3*d*, The marvellous fact, that this change coincided with the occurrence of an aurora of unusual splendour, and apparently the consequence of it.

4*th*, The remarkable singularity of the case, in presenting the phenomenon of emitting sparks in rapid succession from every part of the body, whilst no part was isolated, — the feet supplying a constant medium through which, according to the known laws of electricity in a case of this nature, there should have been nearly an equilibrium produced, — the Brussels carpet "to the contrary notwithstanding." But this was only occasionally interposed; whereas the phenomenon of emitting sparks appears to have existed under all circumstances.

5*th*, But admitting the body to have been at all times isolated, the phenomenon would be still adverse to all experience and to all philosophy. There is nothing in the human organization which, in common language, could have incessantly generated and accumulated such vast quantities of the electric fluid. We have, moreover, already seen, that the most delicate instruments cannot detect a current of electric fluid in any part of the body.

This is more a case for the understanding than for the senses. The former we will never sacrifice while it may call to its aid what may be considered the immutable laws of nature. We call them immutable, when only a solitary exception can be adduced to show them otherwise. We make these exceptions bend to the general rule; and we believe we are safe in affirming, that all such apparent deviations have sooner or later been divested of their marvellous aspect. We are not disposed to question the facts as stated in this case; but we do believe, that the generation of the electric fluid depended upon the dress of the subject, or upon some contrivance that has not yet been revealed. Such a stratagem would be less remarkable than the imposture of Miss M'Avoy, whose medical attendant, Dr. Renwick, published a quarto volume to prove that she could see from all parts of her body.

Will the respectable gentlemen certify that, in the case which we have been considering, there was not an under-dress composed of cat-skins? But the lady was "respectable" — and so were they, at the East, who thought it a good joke to try the credulity of the world with animal magnetism. Shall we surrender universal experience, the principles founded upon that experience, because a "respectable," but "hysterical," female, "plays her pranks before High Heaven?" Before it be permitted that such a one shall throw the learned world into a paroxysm of astonishment, or, at most, shall upset their settled opinions, we think her ladyship should be "searched to the very skin."

The chemists, however, must at least place this case amongst Cotton Mather's "unaccountables," and not attempt to bolster up a theory by a dubious circumstance which is contradicted by the whole history of man.

Such as may not be familiar with electrical phenomena in man or woman, may consult with advantage the accounts given by Pfaff and Ahrens.

(1) Inden Abhandbungen der Acad. von Berlin, 1820, 1821, 1824.

for the express purpose, and on sound galvanic principles, it appears to us that the nervous system is no more necessary to the production of the galvanic fluid, than it is to any other animal product. But it is said, perhaps, that if the communication with the brain be interrupted, the fluid is no longer generated, and therefore it must be the "nervous influence." Might we not affirm, upon the same ground, that bile, &c., is the "nervous influence?" But we maintain that the nervous system is not as necessary to the generation of galvanism as of the strictly animal secretions; nor does the experiment alluded to prove the contrary. It only proves that the animal has lost his voluntary power of inflicting the shock. The galvanism may be generated as usual; for we are incapable of detecting it but through the will of the animal. Sir H. Davy, Bonpland, Humboldt, Gay-Lussac, and others, found it had no effect on the electrometer; and others have represented it as being only slightly appreciable. From the foregoing considerations, also, it follows that all experiments in the way of irritating and wounding the brain prove nothing in favour of the nervous hypothesis, — since these irritations only provoke the will of the animal. How the will exercises the foregoing influence we do not pretend to explain. It is enough that such is the fact; and a fact, too, that might have been inferred from the analogies that relate to the various means of defence with which animals are provided. It is certainly remarkable that the manifestations of galvanism should depend so exclusively upon the will; but this only shows us that there are powers presiding in the apparatus wholly different from those of our laboratories.

But should it still be maintained that the experiment of dividing the communicating nerves, establishes the influx of galvanism from the brain to the galvanic apparatus, we will then proceed to show, by the same reasoning, that it is the skin of the animal which supplies the galvanic fluid, — since Spallanzani found that the torpedo loses its electrical power, when its skin is wholly removed.

To carry out the analogy in relation to the means of defence, we may even extend it as far as the urine of the skunk. And by the same rule, if galvanism be the "nervous influence" in the gymnotus, urine is the same "influence" in the former animal, — since both are more or less designed as means of defence, and

both are equally under the control of the will. The relation of all the secretions to the brain, including that of galvanism and heat, seems to be nearly the same in all the cases. (1) One apparatus may be more supplied with nerves than another; but this is not more remarkable, and perhaps not more inscrutable, than that the heart should be so imperfectly supplied with nerves as to have led eminent anatomists to deny their existence, whilst they abound in some other organs of far inferior use. Animal electricity depends, also, upon the general state of the vital functions. Its production, like other secretions, is more or less arrested by disease; and the presence of the skin, as we have seen, and that of other organs, is indispensable to its formation. Sir H. Davy, having no hypothesis in view, regarded it as distinct from common electricity or galvanism.

But the peculiar apparatus of the torpedo, &c., is a special contrivance for the generation of galvanism; and the very fact, that certain animals are thus provided for this specific purpose, is a strong negative proof that the common organization of the system is incapable of this office. (2)

(1) The late researches of Matteuci upon the physiology of the torpedo, appears to establish a special influence of the fourth lobe of the brain over the electrical apparatus. (*a*)

(2) These considerations, it appears to us, impair Dr. Billing's argument drawn from the electrical animals in favour of animal magnetism, — though we do not regret to see the two subjects associated by an eminent man. This estimable writer, in occasionally stepping aside from the stream of nature to refresh his fancy upon the margin of its pure waters, sends forth the inspiration, that "the physiology of the nerves shows, not merely in the torpedo, gymnotus electricus, &c., but in simple muscular action, that a change in the electric state, plus and minus, takes place on every act of volition; in other words, that there is an electric communication between the brain and the tips of the fingers, whether in playing the piano-forte, or making the passes or actions of animal magnetism." It is proper, however, that we should say, that our author "is unwilling, at present, to introduce a subject *so much* under dispute as animal magnetism." (*b*) Although we think no apology was necessary in the case, we may hope for another vision, and that another revelation will disclose more as to the potent agency of galvanism or magnetism, (whichever it may be,) in the processes of life. Especially do we wish to be informed, how "the physiology of the nerves shows" the right of our author to put forth the foregoing assumptions.

Billing is a man after our own heart, and we have, therefore, avoided no good chance for a collision with him, where important laws come in our way. He is fearless of every thing but facts; and knowing his secret, we have endeavoured to take

(*a*) Le Gazette Médicale, Oct. 1837, and Séances de l'Academie des Sciences, Oct. 1836.

(*b*) First Principles of Medicine, *Introduction*, p. ix. 1838.

Dr. Hunter Lane (1) has a hypothesis, by which he endeavours to show how galvanism may accomplish all the varied products of the living system. An appeal is made to the torpedo, and the galvanism that is generated by piling up dead animal tissues. Of the former we have already spoken, and the latter goes no farther than the specific fact. Almost any three other wet substances, arranged in the same way, will evolve galvanism. It proves nothing as to the effect that may arise from organization, under the influence of the vital forces. (2) These, indeed, are wholly opposed to the forces which rule in dead matter; and we have seen that all contrivances have failed to detect the electric current in any animal body, except the truly electrical. Dr. Lane's fundamental assumption, therefore, fails, and we cannot be "justified by our knowledge of the anatomical structure of secreting organs, as well as by analogy, in *assuming* the capability of these organs to manifest powers corresponding to galvanism;" and we respectfully think that the subsequent reasoning is more hypothetical.

As it respects digestion, Müller tried galvanism along with the artificial digester, but it made no difference. We may mention, by the way, that he found oxalic acid, of all others, the best digester out of the stomach. (3) But he has much confidence in Schwann's "pepsine."

But all hypotheses of the nature of Dr. Lane's must submit when we come to the analogous processes of vegetable life;

advantage of it. We also hold with him, as will be seen by the foregoing remarks, that "it is of great consequence to correct erroneous theories, and thereby enable students to arrive sooner at well founded notions of practice." By the way, also, our author says, that "he has shown how every medical man has his hobby to carry him to the same point; which, though he thinks it very different from his neighbours, is as like it as one four-legged jade is to another; how one man thinks he has made a discovery that he can cure cholera with sugar of lead, and that there is nothing equal to it; whilst tartar-emetic, calomel, Epsom salts, mustard, lemonade, or vinegar and water, &c. &c., will do the same thing." Now we have quoted this, not only for its merit as a palpable hit, but to remind our author that he left out his two hobbies of galvanism and animal magnetism.

(1) Liv. Med. Gazette, vol. i. p. 305, and Tiedemann's Physiology, p. 227.

(2) Serious objections have been made to Volta's, Galvani's, and Humboldt's experiments on frogs, and to their inductions from them. (See Müller's Physiology, vol. i. p. 618.)

(3) Physiology, vol. i. p. 543. Matteuci says, he effected digestion of meat with chloride of sodium and the simultaneous influence of galvanism; Müller says he did not. *Bladders* were used in the experiments, p. 550.

especially where the nervous system, as in Lane's theory, is made an indispensable part of the mechanism. In the experiments with the animal substances, tendon is as good as nerve.

We might go on to multiply objections to the whole theory of galvanism as applied to the phenomena of life; but these we shall have occasion to present, from time to time, in our inquiry into various physiological processes.

PHILOSOPHY OF BLOODLETTING.(1)

SECTION I.

Medicus "naturæ Minister et Interpres."

It was the original design of this paper to illustrate more fully our theory of venous congestion, by endeavouring to show, through the philosophy of the operation of bloodletting, that it is probable this remedy removes congestion of the veins upon our principles; and that, therefore, we obtain from this source a strong presumption in favour of our doctrine of the proximate or pathological cause of that disease. We have designed it, also, to reflect some light upon the nature of the vital forces, which we have just considered, — since we have endeavoured to show that the effects of bloodletting are wholly incapable of explanation upon any principles in physics.

We would premise, however, that in nothing which we are about to say, do we in the least intend to supersede those facts which are the true basis of medical science, nor to make the indications for bloodletting to depend upon the philosophy of its operation. We fully recognise the spirit of the principle, — "ars medica tota observationibus."(2) We do but endeavour to account for the influences and the changes which result directly from the loss of blood, and thus to render the philosophy of its operation subservient to the suggestions of experience. Its application, as a remedy in particular cases, must be especially determined by observation.

How, then, does bloodletting operate? How are diseased vessels unloaded, in some instances, by the abstraction of small quantities of blood; when, in other cases, under apparently the same circumstances, a great extent only of the remedy will effect the same result?

(1) The substance of this article was communicated, in the form of an Essay, to Dr. James Johnson, as announced in the Medico-Chir. Rev. July, 1837, p. 296.

(2) F. Hoffmann.

The phenomena are evidently less determined by the quantity taken, than by some other cause. "Difficult as it is," says Dr. Bright, "theoretically to explain the efficacy of abstracting blood from the small superficial vessels between the shoulders, as a means of unloading the circulation in the brain, experience certainly leads us to think favourably of the practice." (1)

In those cases of apoplexy which are attended by congestion of the cerebral veins and extravasation of blood, and in which the countenance becomes pale, "if they admit of relief," says Dr. Philip, "the smallest loss of blood from the head is immediately attended with a diminution of the insensibility." (2)

"In the treatment of gastritis," says another enlightened observer, "there is nothing more useful, nothing more decidedly efficacious, than the free and repeated application of leeches." "It is the grand agent in effecting a cure." (3)

"The action of the heart," says another, "may be weakened by small quantities of blood abstracted by leeches, when moderate or larger quantities would be required to produce a similar effect by venesection from a large vessel." (4)

"I ordered a gentleman," says the same eminent man, "to be bled for a pulsating pain in the head. He was bled repeatedly for it without relief. I then ordered twelve leeches to be applied to the temple, when the pulse fell twenty beats; though it had not fallen before under repeated large bleedings from the arm. The leeches completely relieved the pain. I could not account for this, but I have observed it repeatedly since." (5)

"Whatever explanation be given," says Dr. Wardrop, "the good effects of using leeches at a distance from the affected organ in certain states of disease is indisputable, and this practice is particularly applicable in all congestions of blood." (6)

"On applying leeches to the chest," says Dr. Beddoes, "I have noticed the disappearance of suffusion of the eyes. We know that the effect goes deep." (7)

Stahl says, "mirum est quantum venæ sectio topica prope locum affectuum instituta possit." (8)

(1) Medical Reports, vol. ii. p. 336.
(2) Essay on Indigestion, p. 148.
(3) Stokes's Theory and Practice of Medicine, p. 36.
(4) Armstrong on Typhus Fever, p. 95.
(5) Armstrong's Lectures, vol. i. p. 342.
(6) On Bloodletting, p. 48.
(7) On Fever and Inflammation, p. 186.
(8) De Sanguisug. Utilitate.

"Authors have stated, that a few drops of blood from the nose were often followed with more relief than copious bleedings." (1)

Aretæus thus describes this effect of local bleeding: "Cum morbus autem diu trahitur, et caput in causa est, cucurbitula occipito affigenda, et sanguis largiter hauriendus,—*plus enim quam venæ sectio profuit.*" (2)

Oribasius had great confidence in cupping, and states that he owed his life, in an attack of the plague, to the abstraction of two pounds of blood by scarifications upon the thigh. (3) Alpinus says it was universally practised by the Egyptians, and describes their method of operating." (4) Fracastor, who recovered from an apoplectic affection, complained that he could not obtain the benefit of his favourite remedy, although he had pointed to his head for the purpose of being cupped. (5) The only good thing done by Themison was that of introducing leeching into the practice of medicine, B. C. 50. (6) Cupping is recommended in preference to venesction, in many cases, by Hippocrates, Aretæus, Celsus, Galen, Avicenna, and other ancients. Avicenna devotes a chapter to the mode of applying, and preserving leeches. (7)

It appears that, through all ages, a peculiarity of effect has been observed to attend the local abstraction of blood.

As to general bloodletting, it is observed by Dr. Hall, that "it is one of the most remarkable facts in physic, that if several patients of similar strength and constitution, but affected by dissimilar diseases, be respectively placed in the erect position, and bled to deliquium, they will be found to have lost very various quantities of blood. I have known a patient, not apparently very feeble, faint on losing four ounces of blood; and I have known patients to lose fifty, sixty, and even seventy ounces of blood without syncope." (8)

Galen makes the same remark, which has also the advantage of great practical importance. "Memini enim quibusdam ad

(1) Louis on Bloodletting, p. 78.
(2) De Morb. Acut. Cur. l. i. c. 4.
(3) Opera Coll. 7. 20.
(4) De Med. Ægypt, l. 3. c. 8.
(5) De Affect. Capit. p. 270.
(6) Cœl. Aurel. de Morb. Acut. l. 3. c. 3. de Ceph.

Rhazis describes the operations of cupping and leeching, very minutely. (a) Local bleeding came into high repute after the revival of letters. It is strongly recommended by Walther, Aponens, Crato, Nymmann, Schencke, Dodonæus, Lusitanus, Haly, &c.

(7) L. 1. Fen. 4. c. 22.
(8) Researches on Loss of Blood, p. 105.

(a) L. 7. c. 21.

sex usque libras sanguinem detractum fuisse ita ut febris protinus extingueretur, nec ulla sequeretur virium afflictio; quibusdam vero sesquilibram haud citra leve saltem virium detrimentum, quibus si quis duas vacuasset, extreme læcessit." (1)

Dr. Wardrop, in stating the same fact, suggests the reason for the difference in the results. "That there is a great difference in the quantity of blood in different people would appear probable from the circumstance, that if a succession of individuals be observed, afflicted with a *similar* disease, requiring the abstraction of blood for its cure, the quantity necessary to produce the same effect, varies very much in every different instance, which may probably depend on differences in the quantity of the blood in each person." (2)

So, also, Dr. R. Jackson: "Deliquium," he says, in the treatment of yellow fever, "occurs sometimes from the loss of a few ounces of blood, sometimes scarcely from the loss of ninety-six ounces." (3)

A practical as well as theoretical error, as it appears to us, prevails more or less in regard to the manner in which the loss of blood produces its effect. We may state it as briefly avowed by M. Andral. "By the employment of bloodletting," he says, "the organ congested is relieved of a part of its superabundant fluid, the general mass of blood in circulation is diminished, and a powerful cause of excitation is thus withdrawn from the system; but neither by local nor by general bleeding can we remove the unknown cause under the influence of which the hyperæmia was originally developed." (4) Something like this appears to be implied by Dr. Wardrop in the foregoing extract.

It will be our object to show, that the essential effect of bloodletting is scarcely at all owing to "the diminution of the general mass of blood;" but that it is purely in virtue of the direct influence of the remedy upon the vital forces; and that this impression, in modifying the action, is equivalent to a removal of the "unknown cause," always excepting mechanical irritants, and especially the

(1) De Curandi Rat. per sang. miss. c. 14.

This is the first instance on record in which the quantity of blood extracted is specified. Galen bled more extensively than is commonly supposed.

(2) Op. Cit.

(3) Hist. & Cure of Febrile Diseases, vol. i. p. 226.

(4) Patholog. Anat. vol. i. p. 20. So, also, Dr. Cragie, Elements of the Practice of Physic. *Introduction.* p. xxvii.

"thorn of Van Helmont." We concede to M. Andral, who illustrates his principle by adducing the "thorn," that it would be necessary, in such a case, to conjoin with bloodletting some mechanical means before the "unknown cause" could be removed, or await the suppurative process.

But whether the thorn of Van Helmont be regarded in a literal sense, or it be the blood, or any other exciting cause that may be assumed, we see not the analogy betwixt this and the pathological condition upon which bloodletting and other remedies are designed to operate. Herein, also, we may see an error of the humoral pathology. If a morbid state of blood be assumed as the "thorn," or an exciting cause of disease, how does it happen that the removal of a few ounces will sometimes overthrow the choicest cases of the humoralists? The foregoing example denotes, also, a want of due appreciation of the forces of life.

Equally objectionable, too, are the doctrines of revulsion, derivation, and the hydraulic, — at least, it appears so to us.

SECTION II.

We shall first consider the philosophy of the effects of loss of blood as they are manifested in the healthy subject. The principles will be, in all important respects, the same in disease, although some of the direct manifestations may be different. When, for instance, the action of the heart is strong, and the pulse bounding, as in many inflammations, the energy of the circulation may be directly lessened. On the other hand, in venous congestions, opposite phenomena may exist, and we may feel the action of the heart strengthening and the pulse rising, ere the blood has ceased flowing.

We shall, also, extend our inquiry through the gradations of its influence to its consummation in syncope. It is not our purpose, however, to inquire into those ultimate influences which

depend upon the diminution of fibrin, &c. except as they may serve to illustrate the essential philosophy of bloodletting.

We shall defer, for our next section, a consideration of the influences of the cerebral system in the various modes of abstracting blood, — it being understood that a tacit reference is had, more or less, to those influences, in our present inquiry.

In the first place, we suppose the earliest effect of bloodletting consists in a contraction of the blood-vessels. That in leeching, this contraction begins in the extreme vessels of the part, which soon involves, by sympathy, those in the vicinity; and what is especially important, such as may be the seat of disease, when leeches are applied to the part inflamed, or in its immediate neighbourhood. This contiguity, however, as will appear, is not always necessary to the immediate impression. It may happen, as we shall endeavour to explain, upon a similar principle somewhat modified, when the affected part is remote from the place of leeching. By the same principle, also, the capillary series, which possesses in all parts an organization and function more or less alike, becomes involved in like manner throughout the system. The larger vessels, sooner or later, participate in this contraction, and by reacting sympathy, increase the artificial affection of the extreme vessels. But such is the sympathetic relation betwixt the extreme vessels and the heart, that in many susceptible constitutions or from peculiar conditions generated by disease, the impression induced upon the *vires vitæ* of the extreme vessels that are under the primary influence of leeching may be propagated directly to the heart, as well as through the more direct chain of sympathy, of which we have just spoken. It is obvious, therefore, as will be farther shown when we speak of the operation of general bloodletting, that the whole system, under these circumstances, will be more immediately placed under the full influence of the remedy, and less blood will be required, than when the effect is propagated mainly along the extreme vessels, the capillaries, (1) and the larger series.

The foregoing effect is originally exerted on the larger vessels and the heart through the united laws of continuous and remote

(1) We call that series of vessels from which the extreme, organic vessels are propagated, the *capillaries*. We often use, however, *capillaries* for *extreme vessels*.

sympathy, — but chiefly by the latter as it regards the action of the heart.

There is no difficulty in comprehending how the foregoing laws are concerned in the phenomena of leeching, if we consider, for a moment, how very circumscribed inflammations of the skin, or of any other part, may derange the whole system through the same laws.

In general bloodletting, or when a large vein is opened, the effects are varied, and in a way of practical importance. The first impression is simultaneously on the large and small vessels. A contraction takes place directly, through the impression exerted upon the vital forces, from the diminution of the volume of blood, — that of the smaller vessels increasing in a greater ratio from their greater sympathy with the larger, and their greater endowment with the vital powers. The contraction of the extreme vessels, therefore, grows out of a double influence, — that which is exerted by the direct impression of the remedy, and that which proceeds from the change in the larger vessels, and which is propagated through the principle of sympathy. But the impression thus made upon the extreme vessels reacts, in its turn, upon the larger vessels, till a circle of complex influences becomes established. For the foregoing reason, however, the alarm, from the beginning, is felt most sensibly by the extreme vessels and capillaries, and their contraction, from the beginning, is wholly the greatest. This contraction increases, also, in an increasing ratio as the blood flows from the vein. The phenomena, of course, are more or less modified by constitution, &c. but the same laws prevail in all cases. In leeching, on the other hand, the contraction of the large vessels is wholly at first, and mainly afterwards, consequent on the impression upon the extreme vessels and capillary arteries, and is purely the result of vital action. In general bloodletting, this contraction is only in part, though mainly of that nature.

Simultaneously, therefore, with this beginning contraction is a modification (or, if it be preferred, an excitation) of the vital properties of the capillary arteries, of which their greater contraction afterwards, till syncope begins, is chiefly the consequence. Since, also, these changes may commence at the very onset of the remedy, the amount and rapidity of the primary change, when clearly the *result of the loss of blood*, will depend on the suddenness with which the blood is abstracted.

The next link in the series of changes is the beginning failure of the heart's action. This does not appear to arise, as generally supposed, from the mechanical diminution of the circulating mass, (1) since the abstraction of one or two ounces in robust subjects may be sufficient to produce syncope. The phenomena, too, are generally determined by a much smaller quantity of blood, when taken from a large, than from a small orifice. The heart, also, may continue to receive the same or a greater quantity of its natural stimulus, whilst there will be a diminished volume to move in the vessels. "If bloodletting," says Mr. Hunter, "be considered in a mechanical light, as simply lessening the quantity of blood, I cannot account for its effects; because the removal of any natural mechanical power can never remove a cause which neither took its rise from, nor is supported by it." (2) It is also proved by experience, that the permanent effect of bloodletting, as a remedy, often depends much upon the manner in which the blood is abstracted.

So far as it relates to the mechanical diminution of blood in the capillaries as set forth in the following extract, we partially agree with the learned author; but even this is only a consecutive result of an important victory already obtained over the essential cause of inflammation. "Though it is impracticable," says Dr. Cragie, "to restore their tone and energy to the capillary vessels of an inflamed part, it is possible, by diminishing the mass of their contents, to give them less to do, and place their exhausted powers more upon a level with the duty which they have to perform." Bloodletting is then recommended for this purpose. (3)

Here we have a combination of the doctrine of debility and that which imputes action to the inflamed capillaries. The author supposes "their powers to be *exhausted*," and yet they

(1) We have cited Andral and others to this effect. So, also, one of our latest and best authorities. "In inflammatory diseases, though we cannot by any means operate directly on the process, so as to subdue it in the affected organ or organs, experience teaches us, that, if we diminish the materials whence the process derives its intensity, — if we empty the overloaded and distended vessels, &c., the vessels of the affected texture are, in general, adequate to complete the cure." "Treatment suggested by this method is denominated *rational*." (a)

(2) On the Blood, &c., p. 345.

(3) Practice of Physic, vol. i. p. 406.

(a) Cragie's Elements of the Practice of Physic. *Introduction*, p. xxvii. *Edin.* 1836.

have such a "*duty to perform*," that we can only restore the powers by "*giving them less to do.*" We shall get rid of the whole of this embarrassment, by supposing that the pathological cause of inflammation is constituted not only by an augmented state of the vital forces of the instruments of disease, but that the forces are also modified in *kind*. Bloodletting so impresses these forces, that they are brought into a new state of action, by which their contraction is produced, and their contents more or less expelled.

We have said, that in cases like the foregoing, "an important victory" is obtained over disease, before the blood is expelled from the capillary vessels; or, in other words, the vital forces are so modified by the impression from the loss of blood, and disease so far subverted, that the contraction of the capillaries will often continue long after the application of the remedy. But the extent and durability of the change will depend upon a variety of causes, such as relate to constitution, the nature of the remote causes, and whether, also, the impression have resulted purely from the loss of blood, or, in part, from moral emotions, and will be even influenced by the manner in which the blood may be abstracted. These considerations will come under review, more or less, hereafter.

We have seen that the author last quoted predicates his view of the operation of bloodletting, in conformity with the doctrine of passive relaxation of the immediate instruments of morbid action, and of capillary stagnation of blood in inflammation; which he has, also, extended to the hot stage of idiopathic fever. The philosophy, therefore, is vitiated by the hypothesis upon which it is founded. Moreover, is it not manifest, if the doctrine of debility, passive relaxation of the capillaries, and stagnation of blood were correct, that those capillaries would immediately become again injected, especially if it were also true that they had been mechanically emptied by the abstraction of blood; and this the more so from the debilitating nature of the remedy?

However, then, we may regard the operation of bloodletting in suddenly diminishing and maintaining a diminished volume of blood in the immediate instruments of inflammation, it appears to be manifest, that there can be no loss of power, and no relaxation in the vessels. And this is especially obvious in those cases, (as in many instances of pneumonia,) where the action of the heart is prostrated antecedently to the loss of blood, but becomes

exalted after the loss is sustained; the extreme vessels and capillaries of the part, however, acquiring and maintaining a diminished volume of blood.

The contraction, therefore, of the small vessels is scarcely in any degree the result of the supposed mechanical effect of bloodletting, whilst the diminished volume of blood in the capillaries is owing to the vital contraction of these vessels. Did the mechanical principle operate, and were the distension of the vessels owing to their relaxation, bloodletting could not be a cure for inflammation, but would infallibly increase the evil which it so certainly removes.

But the vital forces being impressed, as we have stated, by the loss of a moderate proportion of blood, so that a great and universal contraction is established in the capillary system, the attendant expulsion of blood is certainly an important step in the process of cure. It is still, however, a secondary step, or rather the removal of a morbid symptom which has been effected by a partial restoration of the *vires vitæ*, upon whose lesion the morbid symptom depended. True, this accumulation of blood constantly tended to maintain and to increase the cause in which it originated; and should it recur from an imperfect impression of the loss of blood upon the vital forces of the circulatory organs, it will still tend to re-establish the former morbid state of the capillaries. In the latter instance, especially, the return of inflammation must be ascribed, in a measure, to the blood as an exciting cause; and, so far, we must consent to be considered humoralists. But much more is due to the imperfect nature of the impression upon the vital forces, which, retaining their morbid state, do but go through with the same series of phenomena which they originally instituted. We may say, also, that the loss of blood, which establishes the change in the extreme and capillary vessels, (for it is universally divided amongst all the capillaries of the system,) is almost inappreciable when compared with the amount of the circulating mass, whilst it is powerfully felt by the *vires vitæ;* and the diminution of the volume of blood in all the capillaries may be twenty times greater than the quantity abstracted.

Allied to the foregoing hypothesis of Dr. Cragie and others, is that by Dr. Arnott, when applied to bloodletting. "Fainting," he says, "is produced by several causes, and among others, by any occurrence which renders the blood vessels about the

heart suddenly less full or tense than usual. It would appear that the heart being accustomed, when it contracts, to a certain degree of resistance, has its action disturbed when the resistance is much diminished." (1)

This construction, we shall endeavour to show, is in no way applicable to the effects of bloodletting; whilst it is also as erroneously supposed by many, that the primary failure of the heart's action depends upon the removal of a proportion of its natural stimulus. We mean, that this is our opinion. Indeed, we think it more improbable than the hypothesis which we have just examined, since the general contraction of the capillary arteries and veins actually increases the volume of blood at the centre of the circulation. This sudden accumulation of blood about the heart is, probably, one cause of its embarrassed action. But we must look for a greater in that principle of sympathy which brings into a state of corresponding contraction a vast extent, if not all, of the extreme vessels of the body. This is most distinctly manifested in leeching, though it is equally true of general bloodletting. The power exerted upon the heart is that of sympathy, reflected from the impression upon the vital forces of the capillary system; not the diminution of the volume of blood in the cavities of the heart, either in a mechanical or a vital sense,—since the volume at the heart is actually increased when syncope approaches.

If, however, the action of the heart in leeching, be influenced by an impression upon a *small* extent of the capillary vessels, as is manifest in some cases, how much more likely is it to suffer when a similar impression is universally and more suddenly produced. Hence, in general bloodletting, when the contraction of the capillaries is rapid and simultaneous throughout the body, the amount of influence is immense, and the action of the heart may be immediately prostrated.

The contraction of the capillary system we suppose to depend chiefly, as we have stated, upon the action of its vital forces, and but little on the elasticity of the vessels. (2) The latter, however, co-operates more or less, particularly when a sensible failure has taken place in the action of the heart; when the ca-

(1) Elements of Physics, &c., vol. i. p. 470.

(2) With Bichat (*a*) we suppose the property of elasticity, in the living body, to be modified by the forces of life; and that it is, therefore, scarcely a subject for experi-

(*a*) On Life and Death, p. 43. Gen. Anat., vol. i. p. 35.

pillaries, in consequence, suffer a greater diminution of their supply. But, in leeching, the contraction of the vessels, throughout the operation, is more perfectly the result of the action of the vital forces.

Syncope, therefore, when it is induced by the *loss of blood*, is a test that the vital powers of the extreme and capillary vessels have been strongly affected. Like the contraction of those vessels, it is one, though a less simple, consequence of that affection.

We infer, also, that the failure of the heart's action from loss of blood depends, primarily, (with the exception of a concurring cause to be hereafter stated,) upon its sympathy with the change in the extreme and capillary vessels, and the blood which has accumulated about the centre of the circulation.

The ratio in which the various phenomena succeed each other, in disease, will, of course, depend on the condition of the vital powers, especially such as belong to the heart and blood vessels. We have often the clearest evidence, that a corresponding susceptibility pervades the whole sanguiferous system; and when this exists under a certain modification, the abstraction of a very small quantity of blood may almost instantly determine a paroxysm of syncope. At another time, under a different modification, depletion may be carried to a surprising extent, even to eighty, ninety, or one hundred and twelve ounces, without inducing the paroxysm. (1) Although a certain contraction of the vessels necessarily follows this great loss of blood, it may even be in these cases mostly from the mechanical cause, and the elastic

ment after death. The same doctrine is advocated by Reid, (*a*) Tiedemann, (*b*) and many other physiologists.

(1) In some cases of acute inflammation of the brain, Dr. R. Jackson has suddenly abstracted one hundred and twelve ounces of blood, and thinks "the loss may be even carried farther without compromising the patient's safety. (*c*) Again, a patient may faint from the loss of a few ounces at the first bleeding, but may bear the abstraction of several pounds in two or three hours afterwards. The cause of this phenomenon will be considered in the sequel.

Dr. Armstrong mentions two cases, one of laryngitis, the other of enteritis, where eighty ounces of blood were drawn before syncope took place. (*d*)

(*a*) Archiv für di Physiolog. T. 7, p. 438.
(*b*) Comparative Physiology, vol. i. p. 29, &c.
(*c*) History and Cure of Febrile Diseases, vol. ii. p. 138
(*d*) Lectures on Acute and Chronic Diseases, vol. i. p. 395.
See, also, Dr. Forbes's Laennec on the Chest, p. 246, *note*.
Rush's Med. Inquiries and Obs., vol. iv. p. 337.

property of the vessels, and that the contraction of the small vessels is mainly in the ratio of the diminution of the volume of blood. There has been no strong impression made upon the *vires vitæ* of the extreme vessels, and, therefore, they sustain a diminution of their contents in a proportion more corresponding with that of the large vessels, and the amount of blood actually removed from the circulating mass. Thence, disease either remains more or less unsubdued, or the vital powers, ultimately feeling the loss, take on a new action, disease abates, and as one of the consequences, in inflammations, the instruments of morbid action contract and expel a part of their contents. Thence, also, in such cases, no great sympathetic influence will be exerted upon the heart, during the application of the remedy, and no preternatural quantity of blood determined upon that organ; but having less blood to move, the heart may act even with greater force, till the more vital contraction commences. Thence, also, is it in part, if local inflammation exist in these cases, and depletion be not carried to the point of syncope, that the increased force of the circulation often restores the plethoric state of the vessels concerned in the morbid process. (1) But a greater cause exists in the failure of the remedy to impress the vital forces in such a way as to establish their alteration. (2)

The diminution, therefore, of the volume of blood in the in-

(1) This subject has no connection with another which has been lately agitated, respecting the quantity of blood expelled by the heart, and the force of its contractions, when its action is much accelerated. (*a*)

(2) These principles are constantly illustrated in practice. Thus, "the abstraction of a few ounces of blood in puerperal peritonitis," says Baudeloque, "will, at most, only produce a momentary diminution in the symptoms, and retard, somewhat, the progress of the disease, which will soon recommence with *increased* violence." (*b*) Here we see illustrated the mechanical principle in respect to the increased force of the heart, and the vital one as it regards the unaltered state of the instruments of disease. Gordon gives us the same exemplification in a stronger light. "When," he says, "I took away only ten or twelve ounces of blood, in puerperal peritonitis, my patient always died, but when I had the courage to abstract twenty or twenty four ounces at one bleeding, in the beginning of the disease, the patient never failed to recover. (*c*)

"Nam sicuti inflammationibus sanandis nimia sanguinis quantitas noceat, ita etiam exigua ejus portis iis adversa." (*d*) We shall see, in the sequel, numerous illustrations of this principle.

What we have just said in our text is, also, more or less applicable to the healthy

(*a*) See Edin. Med. and Surg. Journ., July, 1835.

(*b*) On Puerperal Peritonitis, p. 326.

(*c*) On Epidemic Puerp. Fever, p. 78.

(*d*) F. Hoffmann, de Magno venæ sectionis, t. 3, p. 269.

flamed vessels is a very remote effect of lessening the quantity of the circulating mass, or even of its abstraction from the vessels themselves. The quantity of blood, we have said, which is sometimes sufficient to overcome severe inflammations, bears but little proportion to the amount in the vessels when syncope takes place.(1) According to computations which have been made of the dilatation of the *radial* artery by the quantity of blood injected by the heart, (the entire quantity being two ounces,) the diameter of the vessel, which is about two lines, would not be increased beyond the twenty-sixth part of a line. But we witness very great contractions of the arteries and veins from the loss of two ounces from the circulating mass. This arises, also, during the application of the remedy, when, even inflamed vessels that are enlarged to many times their natural diameter, may be also nearly reduced to their original volume. How little, therefore, of that contraction can depend upon the diminution of the volume of blood, when this amount, only, is taken from the general mass.

That, in the former instance which we have just supposed, where syncope is induced by the loss of a small quantity of blood, the phenomena are owing to some sudden alteration of the vital forces of the extreme vessels, which is thence propagated by sympathy to the heart, and not to the diminished volume of blood, seems to be obvious, also, from the fact, that the abstraction of blood may be often carried, soon afterwards, to a large extent without a recurrence of syncope. The first impression has so modified the condition of the vital powers, that they are comparatively insusceptible to the loss of blood at its second abstraction. This is especially true when syncope arises rather from some emotion of mind, than from the blood abstracted. Or, if depending chiefly upon gastric influence, an effort at vomiting so reduces the irritability, and more or less modifies the vital forces at large, that we may often proceed, without delay, to accomplish a full bloodletting.

state of the system; though, generally, in a far inferior degree. Analogous influences may arise from those natural modifications of the vital powers which constitute the differences in constitution, and which also grow out of age, the prolonged influence of climate, &c.

(1) Dr. Philip says, "the vessels in the parts most inflamed were enlarged to several times their original diameters." (a) This is universally admitted.

(a) Inquiry into Laws, &c., p. 260.

If these principles be correct, they will "explain the efficacy of cupping betwixt the shoulders in unloading the circulation in the brain," and how "the heart may be more weakened by small quantities of blood taken by leeches, than by venesection;" as well, also, all the remarkable phenomena appertaining to the different modes of abstracting blood, which we stated in our first section.

In respect to leeching, (1) the contraction of the extreme vessels being produced by a direct impression upon them, and parts of the same structure and function being most disposed to sympathize with each other, especially when existing in continuity, (2) the influence of this change in their vital powers, *cæteris paribus*, will be more powerfully extended to the greater series of capillary vessels, and otherwise propagated in the complex manner already explained, and more completely maintained, than when the impression is produced by venesection; admitting our philosophy in this respect to be also true. A deeper impression, under certain circumstances of the system, is made upon the vital properties of the capillaries in leeching, than in general bloodletting.

The sympathy of which we have spoken may be propagated not only by continuity, but by what is called remote consent. The consecutive developments in disease, as is well understood, often depend upon this principle. Parts that are primarily affected may induce a derangement of function in some neigh-

(1) Very little attention appears to have been paid to the modus operandi of leeching. Baudeloque says, that "the employment of leeches possesses over bleeding from the arm the advantage of a revulsion, effected by the wounds or bites of the animals." (*a*) Dr. Gooch supposes the advantage of leeching consists in "emptying the distended capillaries of the part." (*b*) But then, if the important effect did not consist in altering the morbid state of the vital forces of the capillaries, the blood would flow in, as fast as it flowed out, and maintain the capillaries in a plethoric state.

(2) This principle is clearly seen in rheumatism, when it affects the fibrous tissue. We have known it to attack this tissue almost universally, including the dura mater.

Dr. Warren tied the right carotid, for the removal of an erectile tumour, situated at the internal angle of the eye. There had been a violent throbbing of both carotids and their branches, and that of the left was removed by tying the vessel on the right side. "The perfect success of the operation showed that the affection of the left side was altogether sympathetic." (*c*)

This is, also, a case for the consideration of such as suppose that the blood vessels have no independent action.

(*a*) On Puerperal Peritonitis, p. 342.

(*b*) On some of the Most Important Diseases of Females, p. 54.

(*c*) Surgical Observations on Tumours, p. 403.

bouring part, or the whole system of capillaries may become involved in a morbid process, from the same cause, according to the susceptibilities of parts or of the whole. So, also, in leeching or venesection, but especially the former, the change which is instituted in the primary instruments of disease may exert an effect, analogous to the foregoing, on the sympathetic developments particularly, and independently of that influence which may be produced upon them by the loss of blood.

In other cases, the influence of the action induced by leeching is propagated to some remote organ that may be the seat of disease, partly through the medium of an intervening organ, betwixt which mutual sympathies may especially prevail. In venous congestion of the liver, the remarkable effect of leeches applied to the anus is well known; whilst, if applied to the region of the organ, little or no change of action may follow, and general bloodletting may be equally inefficient. Here the specific influence exerted by the leeches upon the terminal part of the mucous membrane is propagated to the liver through the instrumentality of the entire tract of the former organ, as well as through the principle of remote sympathy. The impression exerted upon the liver appears not to result so much as is generally supposed, through the communication of the blood vessels,(1) as from the principles which have been stated, and from the disposition of the other abdominal viscera to sympathize with impressions produced on the mucous membrane of the intestinal canal; by which, also, other sympathetic influences are reflected by the several viscera upon the diseased organ. It may be considered analogous to those changes which are induced by the action of medicinal agents upon this membrane. But what illustrates still better the nature of this action is the result of mechanical irritations of the conjunctiva, and the mucous membrane of the mouth, in augmenting the flow of tears, and of saliva. And since the most remarkable sympathies prevail betwixt the termination of ducts and the organs with which they are associated, we may expect the greatest relief in the foregoing cases when leeches are applied near the termination of the

(1) "Leeches on the verge of the anus," says Dr. Wardrop, "are particularly beneficial in abdominal congestions, from the connection between the hemorrhoidal veins and the portal system." (a) We think the doctrine erroneous.

(a) On Bloodletting, p. 64.

mucous membrane; and therefore, too, that they may be more beneficial in inflammations of the mucous tissue and abdominal viscera, than of the serous membrane and thoracic organs.

But again it is true in a more limited sense, that the influence of leeching may be propagated along the large blood vessels to parts in the vicinity, where there is a direct vascular communication; though even in these cases, the impression is extended more through the sympathies which bind together the extreme vessels, and the nervous communication of the parts. Comparatively little, we apprehend, is due to the imputed derivation of blood in a mechanical sense. Thence, upon our principles, appears the reason why, according to Dr. Wardrop, "in diseases of the head, as well as in diseases of the eye, more particularly those affecting the internal parts of the globe, leeches applied on the frontal vessels give much more relief than is obtained by abstracting an equal quantity of blood from the temporal vessels, by leeches applied to the temples." He also states, that a like advantage will be obtained, in cerebral affections, by applying leeches to the lining membrane of the nose, or behind the ears. He thinks the effect greater than when the leeches are applied to other parts. (1)

In these cases there is a direct vascular communication between the parts to which the leeches are recommended and the affected organs. We have paid much attention to all these particulars, but have not been able to observe any appreciable difference between the effects of leeches when applied to the "frontal vessels" and to the "temples," in the cases supposed. In all the cases, however, we are disposed to believe that the effects are mainly produced through the agencies which we have stated. Whenever we have applied leeches to the nasal septum, abdominal disease attended the head-affections. The leeches have sometimes relieved the headache, when general bloodletting, cathartics, &c., had failed, whilst the gastric derangement had also persisted. But simultaneously with the relief of the head, the secretions from the bowels improved, the tongue cleared up, and the stomach and other abdominal organs

(1) On Bloodletting, pp. 19, 20.

The practice of applying leeches to the schniderian membrane, in head-affections, has long prevailed on the European Continent. It is, also, particularly recommended by Mr. Crampton. (*a*)

(*a*) Dublin Hospital Reports, vol. iii. p. 230.

were relieved. We therefore suppose that, as in the case of leeches to the verge of the anus under similar circumstances, the specific impression of leeching the nasal septum is propagated through the mucous membrane to the viscera of the abdomen, and that the head is as well relieved by thus removing this source of morbid sympathies, as by the more direct impression.(1) It cannot be entertained, it appears to us, as stated by Dr. Wardrop, that the vessels of the head are unloaded simply by the abstraction of blood from the capillary terminations of the small arteries which come through the cribriform plate to the schniderian membrane. Indeed, Dr. W. states in another place, that "in affections of the head and thoracic viscera, I have, in many instances, recommended patients to apply leeches on the head, or chest, and on the feet, alternately; and *almost universally*, I may venture to say, a decided preference has been given to the *feet*." (2) Here, then, we are admitted to be correct, and our philosophy is strikingly illustrated. The peculiarity of the effect is evidently owing to a common principle. But whilst there exists the foregoing discrepancy in the preference given to applying leeches to the nasal septum, behind the ears, &c., in many complaints of the head, and again, in apparently the same affections, to the feet, and the imputed *modus operandi* thus set aside, and although our author overlooks the principle of sympathy, he evinces much confidence in that of revulsion, which, to our mind, in all these, and analogous cases, is nothing but sympathy.

Still we will not, for the advantage of enforcing our construction of the *modus operandi* of leeching, agree with our author, that leeches to the *feet* in the foregoing cases, or to that referred to in our preceding note, are by any means as efficient as when applied to some part of the head, to the anus, &c. The latter preference of our author appears to depend, in a measure, upon the old doctrine of revulsion.

We do not, of course, agree with Dr. Armstrong, that "leeches never do any good unless they produce a decided effect upon the

(1) We are now in the habit, in all cases of delicacy, where abdominal disease may indicate the application of leeches to the anus, of applying them to the nasal septum. The difference in results is not great, though rather in favour of the former. Lancisi applied leeches to the nasal septum with great effect. De Sub. Mort. p. 154.

(2) Op. Cit., pp. 48, 65. And again, "bloodletting may be often most successfully employed in restoring the menstrual flux. In such cases, the application of leeches to the *feet* is the best mode of abstracting the blood," p. 66.

heart." (1) On the contrary, we think the opinion is practically, as well as philosophically wrong. We have seen severe pain from muco-enteritis entirely removed in five minutes after applying a dozen leeches to the anus; and we have known, and so have others, the symptoms of depressing cerebral congestion give way under the influence of leeches, when the heart has been simultaneously excited to greater action.

The view which we have taken of *continuous* and even of *remote* sympathy may not seem to be, at all times, compatible with the doctrine of reflex nervous action; but it would be very difficult to explain the propagation of inflammation along any particular tissue, in connection with other sympathetic phenomena, which we shall now state, upon any other principle than that which we advocate. And what particularly modifies the law of reflex action, or rather shows the existence of distinct modifications of the law of sympathy, is the particular liability of remote parts of one tissue, as the mucous, serous, or fibrous (2) membranes, to become involved in a common mode of disease, in consequence of a similar condition of disease in some other part of the same tissue, whilst no other tissue is influenced in the same way. Again, also, inflammation is frequently propagated from one tissue to another of a different kind, without exciting the same action in other parts; or, different tissues, wholly separate from each other, reciprocally exert a like effect. This is constantly exemplified between the skin and intestines, the mammæ and genital organs, the porotids and testis, &c. The brain and spinal marrow, and the nerves, in an organic sense, constantly sympathize amongst each other in the same way. When, too, one iris contracts, the other contracts also, though the light be excluded from the last. Affections of the motor nerves of one eye are followed by a like affection on the opposite side. Deafness in one ear gives rise to deafness in the other, &c. (3) So too, whenever blisters are applied to the skin, they

(1) Armstrong's Lectures on Acute and Chronic Diseases, vol. i. p. 336.

(2) The remarkable sympathy amongst the fibrous membranes is well shown in rheumatism. And yet it is doubted by anatomists, though certainly against all analogy, whether the fibrous membranes have any nerves. Our question is not affected, whether we believe with Chomel, (*a*) and some others, that rheumatic fever is antecedent to the local affection.

(3) Hence we understand how disease of the anterior lobe of the brain may so involve other parts of the organ, or the cerebellum, the medulla oblongata, or even

(*a*) Legons de Clinique Méd., &c. t. 2, 1837.

modify the action of parts situated beneath; and this, whether the liver, intestines, lungs, bladder, muscle, cartilage, or bone, be the seat of morbid action; however vague, too, may be the communication by means of nerves. All these phenomena, however, being analogous, are very properly regarded as the results of a common force, whatever be the medium through which it operates.

Recurring again to the paroxysm of syncope, the reason, according to our rule, why the heart often fails more permanently from leeching than from general bloodletting, is sufficiently obvious. We think, however, that in disease we should look, also, to other causes than such as are suggested by the general doctrine in accounting for the greater effect of leeching on the action of the heart. This remedy, by its greater impression than venesection upon the extreme vessels which are the direct instruments of disease, more immediately and permanently subverts the morbid actions, and thus sooner withdraws a cause that may powerfully stimulate the heart, and that contributes, also, to maintain a greater resistance in the universal capillary system, (as will be explained hereafter,) to the influence of general bloodletting.

It is not probably, alone, that modified state of the vital forces, and the contraction of the capillary vessels as in venesection, that induce the remarkable change in diseased vessels, which arises from the application of leeches. It often happens that the same quantity of blood taken by cupping will not produce half the effect. We rarely witness the same prostration from this mode of abstracting blood as we often do from leeching. It even differs much from the process of hemorrhage which is instituted by nature, since it is rare that an equal quantity of blood thus removed impresses the system with a force equal to that of leeching; whilst large hemorrhages are daily occurring, without

remote nerves, in organic sympathies, that the greatest variety of phenomena may almost simultaneously take place. And thence, we see how liable to error must be all phrenological conclusions that depend upon the anatomical signs of disease, and how imperfect, also, must be all the inductions drawn from experiments upon the brain, to ascertain the exact functions of its particular parts. The uniformity of particular results, as well as those which proceed from natural lesions of certain parts of the organ, do but conduct us to mixed probabilities; from which, certainly, the truth may be partially deduced.

very sensibly reducing the animal strength or prostrating the forces of life, which no mode of art could imitate with safety. Nature does not institute the peculiar change of action which is produced by leeching. We must regard the causes through the medium of the phenomena, and as the results vary from each other, so also must the causes upon which they depend. We seek, alone, for the existence and the nature of the latter by means of the former, and he is no philosopher who refuses an inquiry into causes, from the want of other means of investigation. The objection has never been raised in any other science than that of medicine; but here we are told by many, that we have no means of reaching even the existence of the forces of life as contradistinguished from those of inorganic matter. It is this blindness, in part, which refuses to apply to the science of life the universal fact, that the phenomena are the only index to the forces which govern the inorganic world, that has embarrassed the progress of medicine; and, as we have endeavoured to show in other places, which has encumbered medicine with a spurious philosophy.

Conscious, then, that we stand upon ground which true philosophy will recognise as her own, we shall proceed with our investigation of the nature of those vital forces, which are alone concerned in the phenomena appertaining to the various modes of abstracting blood, and to inquire still farther into the difference of the modifications of those forces which the different modes of sanguineous depletion may produce, and which, as we have said, are as truly and as clearly denoted by their respective results, as the operation of gravitation is shown by the fall of a stone. And we shall carry these principles into every part of our work, and endeavour to show, that in proportion as medical philosophy may depart from them, so must all such philosophy be fundamentally false, and become the irresistible cause of practical errors of the highest import.

From the foregoing considerations, therefore, we infer that the peculiarities of leeching are owing to some specific impression exerted by this remedy upon the forces of life, which no other mode of abstracting blood can exactly establish. This, perhaps, upon physiological principles, might have been deduced, *a priori*, from the more direct impression being made on the terminating series of vessels, which are most eminently endowed with the forces of life, and which are the chief instruments of all vital

actions. In cupping, on the contrary, the blood is abstracted from a larger series of vessels, whose office is probably but little more than to supply the terminating series. That a different mode of action is established, appears, also, from the persistence with which the blood continues to be discharged long after the leeches have performed their office, although smaller vessels are divided than in the operation of cupping. "On applying leeches to the chest," says Dr. Beddoes, "I have noticed the disappearance of suffusion of the eyes. We know that the effect goes deep." (1) More remarkable instances of this nature are familiar to all. A leech, at the anus, will relieve an inflammation of the throat, especially when it depends upon a morbid state of the digestive organs. So it will relieve headache. Will the same amount by cupping from the same, or from any other part, produce an equal effect?

Mr. Hunter ascribed a peculiar influence to leeching. "We find," he says, "that bleeding by leeches alone, will remove a tumour in the breast, having all the appearances of a schirrus, which cannot be considered as inflammatory. Its powers, therefore, extend beyond inflammation." (2) An action is produced by leeching analogous to that of secretion; and though, in this respect it resemble spontaneous hemorrhage, its essential difference in other respects is manifest from the facts already stated. Moreover, in one case the impression is artificially produced by the infliction of a direct violence upon the forces of life, whilst, in the other, the change is wholly spontaneous. In the former case, also, whilst the action is artificially produced, in the latter, it is not only spontaneous, but it is the result of a morbid process, or, in other words, of an alteration of the forces of life instituted by nature either spontaneously, or through the agency of causes which operate at large, and more or less upon the entire system. The artificial cause affects the forces of life, and through them the action of the circulatory and other organs, much more profoundly than the other. (3)

(1) On Fever and Inflammation, p. 186.

(2) On the Blood, &c., p. 338.

(3) We shall have occasion, for another purpose, to introduce some remarkable examples of spontaneous hemorrhage; but in conformity with our general rule of illustrating our statements by facts, we shall here annex an example of the foregoing import, however familiar it may be.

"I have known instances of delicate females," says the able Wardrop, "losing daily from hemorrhoids, on an average, from half a pint to a pint of blood, for many

Thence appears a superiority of leeching over cupping in particular conditions of disease, and, with certain exceptions which will soon be stated, its power of reaching more deeply and permanently the morbid action; and hence the inexpediency of applying cupping glasses to promote the bleeding in the former operation. The abstraction of blood is then so rapid, that its effects become more like those of venesection, whilst a mechanical is substituted for a natural process. Thence will arise a difference in the precise modification of the properties of the extreme vessels, it being highly probable, in these cases, that the modified action will not be the same when the blood is rapidly and violently taken away, as when it flows slowly and spontaneously, nor would a different conclusion be either compatible with the foregoing considerations, nor with the results of the best experience. In one operation, the blood being taken directly from the extreme vessels, and the supposed change propagated from them to the succeeding series, the longer the process is continued, and the more protracted its influence on their vital forces, the more profoundly will they be affected, and a modified action established in the heart and larger vessels. However distant, therefore, leeches may be applied from the part affected, as to the hemorrhoidal vessels or to the feet in diseases of the head, besides the general influence arising from the loss of blood, the specific impression is extended to the part diseased through the principle of remote sympathy. When the liver, stomach, &c., are the seat of disease, leeches to the anus are still more powerfully felt, than when the brain is thus the object of relief, from continuous sympathy being now superadded to remote. And yet, it is obvious, upon the same principle, and from the sympathetic relations betwixt the stomach and the head, that leeches to the anus for affections of the brain may exert their effect upon this organ in part, through the medium of the stomach, especially if the latter organ have been the primary seat of disease. Thence, also, appears the error of the opinion, that "the advan-

months, and even years; a quantity which, compared with the largest quantities of blood ever removed by artificial means, appears astonishing; and the slight disturbance of the system, which such profuse hemorrhages occasion seems indeed unaccountable." (a) But would they appear thus unaccountable, if we duly regarded the nature of the vital forces, and the difference in the impressions which are produced upon them by the various causes to whose action they are liable? The wonder ceases, the moment philosophy takes the true direction.

(a) On Bloodletting, p. 39.

tages of applying leeches are to be derived from opening, with a lancet, one or more veins contiguous to the affected part;" (1) an error abundantly demonstrated, also, by experience.

Again, we have other results from leeching which still farther illustrate the nature of the principles upon which it operates. Spontaneous hemorrhage, as we have said, is well known to occur to an extraordinary extent without very sensibly depressing the forces of life. The same parallel is sometimes seen to happen in regard to leeching, and in these cases we have now and then a palpable proof, that there prevails a modified state of the powers of life corresponding with that which coincides with large spontaneous hemorrhages. At other times, a profuse loss of blood from leeching is borne in consequence of the counteracting effect of some existing inflammation, as will be farther spoken of in a following section. In these cases, the leeching should be allowed to progress till the approach of syncope, unless arrested for the substitution of general bloodletting, which we have often found expedient.

In general bloodletting, the blood being abstracted more directly than in leeching from the general circulating mass, it reaches the diseased vessels almost on the instant; various sympathetic influences, (some of the most important of which are yet to be noticed,) are brought to act simultaneously upon them, and as these are the direct result of a sudden impression, the more rapid the loss of blood, the more powerfully will these causes be brought into co-operation. But it does not thence follow that the impression upon diseased action will be always as great or as permanent as when leeching is employed in certain cases. The distinction in this respect, and the respective adaptations of these modes of depletion to particular conditions of disease, we shall endeavour to indicate hereafter. We will only say now, in illustration of the foregoing principles, that general bloodletting is found most useful, when it is our object to arrest spontaneous hemorrhage. Less blood will be sufficient, and less time required, than in leeching. So, also, if the hemorrhage be abundant but not alarming, the relief will be more complete by leaving the process to nature. (2)

(1) Wardrop on Bloodletting, p. 48.

(2) "Duparcque animadverts, we fear, with too much cause, on the common empyrical course pursued by so many physicians, who see nothing but the hemor-

In certain states of the general circulation, as when the action of the heart is either excited or prostrated, and the extreme vessels at large are involved in a morbid influence, although a certain part only carry on the principal work of disease, an impression cannot be readily obtained from leeching, that shall overcome the local action and the influence exerted by the general condition of the capillaries, the heart, &c. In thus speaking of the morbid action of the extreme vessels at large, we include the entire influences that may arise from the various organs of the system. Under these circumstances, we must have recourse to general bloodletting, that we may obtain from it a sudden and universal impression upon the entire organs of circulation. But in many of these cases, where leeching may seem to be inadequate, it is not improbable that an amount of blood (if judiciously taken by leeches,) smaller than that which must be abstracted from a vein, would as completely overcome the morbid process. True, the quantity must often be very large; but, nevertheless, 100 or 150 ounces, or a greater loss by venesection is often necessary. In some of these cases, however, there is reason to think that 50 ounces taken by leeches may produce an equal influence, provided the abstraction progress so rapidly as to maintain an uninterrupted change, and by the rapidity of the loss of blood, to associate, thus, the advantage of general bloodletting with that of leeching.

Again, however, the violence of inflammation may be so great, and more especially when the whole capillary system is concerned in morbid action, as in idiopathic fever, or when the entire system is strongly sympathising with an inflamed part, the effect of the gradual loss of blood by leeching, and the sympathetic influence from the vessels immediately depleted, may be counteracted by the general disposition to morbid action. In the meantime, the tendency to disease being greater than the curative effect of leeching, it may go on increasing to a fatal extent before an ascendency can be obtained by this mode of abstracting blood. This principle is especially shown in those

rhage; and who, assuming one of the effects for the disease itself, direct their measures so as to check and arrest the discharge of blood." (*a*)

"Quæ educere oportet, quo maxime vergunt, eo ducito per loca convenientia." (*b*)

(*a*) Bell's Electic Journal of Med., vol. i. p. 151.

(*b*) Hipp. Aph. 21. s. 1.

cases where there is a general disposition to inflammatory action, as in acute rheumatism.

Again in local venous congestions, and in congestive fevers, when the circulatory organs are prostrated, an abstraction of eight ounces of blood by venesection may rouse the system more effectually, and far more immediately, than a greater quantity of blood by leeching. Here the heart is oppressed, and we now reach this organ more directly by general bloodletting. The blood has receded from the circumference to the centre, and the loss of every ounce of blood, when suddenly abstracted, is now more sensibly felt by the heart, than when it is freely circulating in the capillaries. But, as we shall see, this influence is still indirectly felt. The first great object in these cases is to establish something like an equilibrium in the circulatory systems. But, since it is the primary effect of leeching to produce a still greater and more permanent contraction of the extreme vessels, it is evident that this mode of abstracting blood would increase, for an indefinite time, the determination of the blood from the circumference to the centre. This might have the effect, and probably would in numerous instances, of increasing the whole condition of disease by the prolonged violence thus inflicted upon the heart. Nor is this an induction from principles alone, since we have seen it practically demonstrated. It is true, also, that the first effect of general bloodletting is to increase the existing contraction of the extreme vessels. But the impression of this remedy upon the vital forces is so rapid and violent, that it more or less subverts, with a corresponding instantaneousness, the morbid condition of the extreme vessels; and since the contraction of these vessels, thus induced, is wholly more transient than in leeching, and their morbid state, of which their natural contraction had been a consequence, being now partially removed, they forthwith dilate and admit the passage of blood more or less freely. This, indeed, more than the vital influences, is the true reason why the heart is relieved in cases of this nature.

And here we cannot but remark another proof, in the *dilatation* of the extreme and capillary vessels, and their augmented contents from the abstraction of blood, that the operation of this remedy, in producing a contraction of the capillaries of an inflamed part, depends but little upon a diminution of the general volume of blood.

It happens, however, in some cases of venous congestion,

which may ultimately demand a great loss of blood, that the prostration of the vital forces is so great, and so great the accumulation of blood about the heart, that stimulants must be premised before we can resort to bloodletting without injury. Here the morbid action is partially modified, the vital forces exalted, some impulse given to the action of the heart, by the temporary use of stimulants, which are always injurious in venous congestion when the *vires vitæ* are not too much depressed to admit of bloodletting. No injury, however, can grow out of stimulants in these cases, whilst the powers and actions of life are so much impaired as to be still more injured by the loss of a small quantity of blood. General bloodletting proves detrimental in such cases, chiefly in two ways; and leeches, according to the foregoing explanation, still more so. General bloodletting operates, in the first place, by directly prostrating still more the powers of life, which are now too low to react under its influence. (1) Secondly, by permanently increasing the determination of blood from the circumference to the centre. The powers of the small series of vessels, as we have said, are too much depressed to react under the influence of bloodletting; and, therefore, the extreme and capillary vessels remain in a greater and permanently contracted state. Here, also, we have another proof, that it is not to the mechanical diminution of the circulating mass, nor to depriving the heart of its natural stimulus, that we must refer the influences of bloodletting.

But in the case which we have just considered, if the prostration of the circulatory organs be long continued, the vital forces are very profoundly altered; and probably it may be stated as a general proposition, that if the abstraction of blood in such cases be necessary, general bloodletting will be more useful than leeching in proportion to the degree of that alteration. The interrupted balance of the circulation will be maintained with an obstinacy commensurate with the absolute change in the *vires vitæ;* and, for the reasons already stated, general bloodletting will sooner relieve the existing emergency than leeching; whilst the latter may not only fail, but for the reasons stated, may add to the evil.

In these cases, we have said, that general bloodletting to the

(1) This will be especially exemplified when we come to speak of bloodletting as a remedy for apoplexy; concussion would be a strong example, but this is not exactly disease.

extent of six or eight ounces may speedily relieve the interrupted equilibrium of the circulation. They now become better subjects for leeching, since this mode of abstracting blood may not establish any injurious determination of blood from the capillaries upon the cavities of the heart. This organ, too, is relieved in its vital properties, and reacts upon the tendency of leeching to determine upon it the blood from the circumference, till the process of depletion has reached that stage when the vital forces of the small vessels begin to establish their dilatation; for it must be borne in mind that we are now speaking of a state of the capillaries apparently opposite to that of inflammation, at least as it regards their tendency to a state of contraction. But if leeching now become the mode of abstracting blood in these cases, it is highly probable that it must be carried to a great extent, since it is not less probable that such would be equally necessary with general bloodletting, before the vital forces can be relieved of their morbid state. The same, or very similar obstacles now exist to the effect of leeching as in the cases we considered just antecedently to the present; and they are much the same in relation to general bloodletting.

In the two latter series of cases, however, where great loss of blood by leeching may be necessary, it is manifest, from the influences which attend general bloodletting, that the latter remedy will be more speedy in completing the cure. And although a greater loss of blood by venesection be necessary, there will be a saving of strength by sooner relieving the system, in this manner, of its prostrating weight of disease.

What we have now stated, we have fully verified in practice, and we shall ultimately show that this has been the experience of the most eminent observers. We have carried leeching to a great extent in idiopathic fever, and in inflammations that have produced great constitutional disturbance; and we have slowly accomplished the cure by less blood than we have been accustomed to take, in similar instances, by venesection. The blood, too, has been very rapidly abstracted in the process of leeching. But we have lost, in these cases, by leeching, from the greater exhaustion which has followed the prolongation of disease, and our patients have got up much more gradually than when we have carried general bloodletting to a greater extent under apparently the same, or nearly the same circumstances of disease. In cases of great prostration of the circulatory organs, we have,

at once, applied leeches to the head, or to the region of the liver for the relief of venous congestions of those organs. But we have seen the capillaries becoming more and more contracted, and the heart more and more prostrated, till we have been driven to the lancet to relieve this mischief from leeching,—this increased disturbance of the equilibrium of the circulation. The impression from the sudden abstraction of blood, so modifies the vital forces of the capillaries, that a greater momentary contraction is followed immediately by their enlargement. We have even so prostrated the action of the heart and the general powers of life, by leeching under these circumstances, that we have been compelled to associate stimulants with venesection. Perhaps eight ounces of blood would be the maximum we could thus obtain. We have steadily looked on during the operation of general bloodletting, and witnessed the increasing contraction of the capillaries, till syncope has taken place. But when the patients have revived, the capillaries have become at once expanded, and the circulation re-established in those vessels. At other times, when the prostration has been less, we have still found the pulse failing, and the surface becoming more pallid, till at a certain stage of the operation, these phenomena have suddenly yielded to others of an exactly opposite nature; whilst, by going a little farther with the remedy, the purely artificial contraction of the capillaries has ensued, followed by syncope. These patients, on reviving, have been vastly relieved. A greater loss of blood, however, has been often, perhaps generally, necessary, than where leeching has been afterwards employed; and in many cases where we have substituted leeching, and, although without the effect of prostrating the action of the heart, the patients have recovered much more gradually, and with less remaining strength, than others, under equal circumstances, whom we have continued to bleed with the lancet, and to a far greater extent. In the former instances, too, relapses have been common, taking place, perhaps, after recovery has appeared to have been fully established; whilst in the latter, the predisposition to disease, and perhaps the disease itself, has been more uniformly extinguished. These are cases, too, in which relapses are especially apt to occur at remote periods, when the treatment has not been sufficiently vigorous, or where the patients are prematurely indulged with unsuitable food after convalescence.

In the foregoing cases, being nearly alike, the other curative means have been nearly the same in all.

But it is in certain mild, though obstinate cases of purely local inflammations, and before the constitution is brought under the influence of the morbid action; or, in cases where the constitutional disturbance has been subdued by general bloodletting, that local bleeding is pre-eminently useful. In either of these cases, general bloodletting continued to a large extent, by the suddenness and violence of its impression, may so disturb the system at large, that the inflammation may be kept up by influences produced by this artificial derangement of the whole system. But here there is no countervailing action against the effect of leeching; and whilst the small vessels engaged in the inflammatory process refuse to give way, if the disease have been of short duration, there is no danger of establishing any injurious influences upon the general capillary system. This, however, will take place, more or less, when leeching exceeds that degree that is necessary to determine a change in the part inflamed. It may even follow from very copious leeching in active chronic inflammations, where morbid action is rendered obstinate by the influence of habit, before the diseased process yields. In the former case, the system is injured partly by the influences determined by the excessive change induced in the instruments of morbid action, and, in part, by the general influence from an unnecessary loss of blood. In the latter case, the bad effects appear to be wholly incident upon the loss of blood in its general relation to the system at large. In these cases, therefore, it is important to graduate the extent of leeching by the exigencies and the peculiarities of each individual case; and it is especially important with infants, upon whom leeching not only produces its peculiar effects very powerfully, but also, more than in after life, the effects that more strictly appertain to general bloodletting. Such is the obstinacy of the depressing change in the instruments of disease, or wherever leeches may be applied, in infancy, when this remedy has been carried far beyond any useful degree in inflammations of the nature we are considering, and its influence upon the whole extent of the circulatory organs is maintained with such violence, that having also superadded to it the general effect from excessive loss of blood, it may be impossible to counteract its destructive tendency. It is not alone the effect that arises from an excess of general bloodletting with

which we now contend, but a greater, perhaps, in that pernicious change which has been induced in the extreme vessels to which the leeches had been applied, and which, indeed, has been, more or less, sympathetically propagated over the whole capillary system.

From what we have now said, perhaps the reason is apparent why cautious leeching is one of the best agents in those inflammations that are now and then induced by an excessive loss of blood, and which have been embraced under the fallacious denomination, as it appears to us, of "diseases of irritation," or of "excessive reaction from exhaustion." But this subject will be considered at large in another place, especially in relation to the pathology. In these rare affections, the triumph of art is beautifully illustrated when accurately guided by the light of science. There should not be one drop of blood too much, or one too little. They are cases, too, in which the distinction betwixt general bloodletting and leeching is forcibly shown, since the former has caused the disease, and the latter cures it.

In some very grave inflammations, as in certain cases of pneumonia, where the constitution has not become much disturbed, leeching may exert a steady and powerful control over the morbid process. The reasons have been already stated. These are cases, however, for a special regard to constitution and age. We have seen them in adults and in infants. They rarely occur in sanguine temperaments; but in such instances, general bloodletting is more necessary than in the phlegmatic. We do not mean to say, however, that leeching may be commonly substituted for general bloodletting, in these cases, when adults are the subjects. On the contrary, the number of such is rare; and we speak of them only to illustrate a principle. In infancy, leeching, under all circumstances, is more efficient than in adults. We have often succeeded as well with leeches, in the pneumonia of young infants, where the circulatory organs have been greatly disturbed and excited, as with general bloodletting. We have been sometimes disposed to think, as a general rule, that the balance is in favour of the leeches. This arises, mainly, from two causes. The necessary loss of blood, whether by leeches or the lancet, is small; and the quantity which would be necessary in general bloodletting may be speedily obtained by leeching. The loss by leeching, however, being more gradual, its effect as a general agent upon the organs of circulation

will be less than if suddenly abstracted as in general bloodletting. Still, it is so rapid as to exert this effect in such a way as to overcome the countervailing action of the extreme vessels at large, and that of the heart, upon the specific change which leeching induces in the capillary vessels. This change then goes forward in an increasing ratio, since the general loss of blood is more and more destroying the counteracting state of the circulatory organs at large, whilst the specific change in the extreme vessels is constantly contributing to the same result.

Again, impressions are more readily made upon the constitution of infants than of adults by bloodletting, and by many other agents, though this is not equally true of all causes, especially of many which are called morbific. For this reason, general bloodletting, as well as leeching, operates with greater power; and the excessive loss of blood, therefore, may be more deleterious than at more advanced life. But if applied within certain limits, we see no good reason, in a philosophical sense, why it may not be as salutary and as safe in the grave inflammations of infants as of adults. When we come to speak of the philosophy of animal heat, we shall then see some exemplifications of the difference betwixt the constitutional powers of young and adult animals, including the human species.

But it frequently happens that general bloodletting is abortive in both cases. Since, therefore, it may fail in infancy, and since the effect of general bloodletting is more readily and fully obtained by leeching in infants than in adults, we may have greater success with leeching in the former case, and in consequence, also, of the greater specific impression from the local abstraction of blood.

But in the cases which we are now considering, we have found cupping more speedy in its effect, and apparently manifesting an intermediate rank between leeching and general bloodletting. Blood being abstracted more rapidly than in leeching, cupping is, therefore, in this respect more analogous to general bloodletting; whilst it stands allied to leeching in directly withdrawing the blood from the capillary vessels. But these, again, are a larger series, have their vital forces less active, and are less susceptible of impressions than the extreme vessels.

Although we generally prefer cupping or leeching, in the severe inflammations of early infancy to general bloodletting, experience and philosophy show the general advantage of the

latter in proportion as infancy advances to childhood, and so on to adult age.

But there are certain conditions of disease to which the foregoing remarks are not applicable, and which, when associated with what we have just said of the coincident effects of general bloodletting and leeching in infantile inflammations, and the differences by which they are marked at subsequent stages of life in the same states of disease, forcibly establish the distinction betwixt these remedies. For, if cupping or leeching be adapted to the grave inflammations of infantile age, it is not so in respect to the *acute* venous congestions which spring up at this period of life, and which may demand the loss of blood. This is equally true of all ages, of almost all constitutions, and however the pathology of the affected veins may be specifically modified, in different cases, by the remote causes. General bloodletting may remove the cerebral congestion upon which infantile convulsions may depend, when leeching would utterly fail. This is shown by experience; and the philosophy of it we shall endeavour to indicate farther in another place.

We learn, also, from the varions considerations which have been made, why the abstraction of blood in small quantities by means of leeches is sometimes more beneficial in chronic inflammations, than a greater quantity at more distant intervals. The impression upon the vital forces of the instruments of disease being more frequently repeated, when it is sensibly felt, maintains the change more perfectly against the morbific influence of habit. We see the same principle more frequently exemplified in the effect of blisters. Indeed, this is so true of the renewal of blisters, that the savine ointment has gone out of use. The philosophy is the same in respect to the relative effects of a large dose of calomel, and ~~half of~~ that dose divided into four; and this, too, where the latter agent appears to be merely local in its action. We are making, however, no very general affirmation, but rather refer, in respect to the mercurial agent, to particular cases, but which are sufficient to illustrate our principle. The influence of bloodletting upon the vital forces is analogous to that of all other agents which operate by modifying their degree or nature. Its influence is constantly more deeply and immediately felt, since its action is directly upon the seat of the forces, and is instantly exerted. We have not only obtained complete success in some inflammations of the ligaments, the

eye, &c., by the daily application of one or two leeches to the vicinity of the part, for a week or more, where greater numbers, at longer intervals, had failed. The same is also true of some chronic venous congestions; although, as we have said, general bloodletting has greatly the advantage of leeching in acute congestions of the veins. In some *chronic* congestions of the liver, for instance, we have often succeeded much better by the daily application of a leech or two to the anus, than by a larger number at more remote intervals.

But it also happens in certain chronic inflammations, as in muco-gastritis for instance, where the habit of diseased action has been long established, and the system at large is strongly under its influence, that we must bring to the aid of leeching the influences of general bloodletting; and this may be sometimes necessary to a great extent. Its utility, in these cases, relates more to the general condition of the system over which a pernicious influence has been established, than to the seat of the inflammation. The general modification exerts a reacting effect upon the part inflamed, and adds to the inveteracy of the diseased habit of the part, and leeching will not reach these influences. We have sometimes practised venesection, in such cases, to a large extent, repeating the operation frequently, sometimes twelve or twenty times. Leeching has afterwards either acted decisively, or has still failed; or, in some instances, nature has come to the aid of art, and the patients have suddenly vomited, or expectorated several quarts of blood, when convalescence has afterwards gone on very rapidly. In all these cases, the blood has been buffed and cupped from first to last; and when the blood has been expectorated, the disease has been mainly seated in the organs of digestion, especially the liver; the lungs being sympathetically involved in congestion. Although the hemorrhage, in these instances, has generally been enormously great, the patients have invariably got up free from disease.

For the reasons now stated, it is also evident, that cupping is more efficacious than leeching, in those cases where general bloodletting excels.

The foregoing law in regard to the peculiar influence of leeching is sometimes remotely exemplified in general bloodletting, when the greatest intensity of its influence on the capillary system is long continued. "I bled a lady," says Dr. Armstrong, "affected with abdominal inflammation, who was

neither remarkable for strength nor delicacy, to the extent of eight ounces, when syncope took place, with a marked remission of all the symptoms. But the inflammation returning on the following morning, about eight ounces more were abstracted. This second bleeding caused a long continuance of faintness; but on recovering from it, the patient expressed herself completely relieved, and from that time rapidly regained her health and strength." (1) The relief was owing to the prolonged impression of the remedy upon the *vires vitæ* of the extreme vessels of the whole system. But we shall soon see that the impression was obtained through other sources than such as are more immediately concerned in leeching; and the foregoing example may be taken as a striking exemplification of what we are about to say of the cerebral influence upon the process of bloodletting.

Venesection and leeching, therefore, appear to be in some respects essentially distinct remedies. The difference betwixt them lies in a difference of principles which are involved. Some of these principles appertain to the cerebral system, which is far more concerned in the phenomena of general bloodletting, than in leeching, especially in respect to their primary effects, and generally less in leeching, at all stages of the operation. We believe, however, that the effects of general bloodletting may be obtained by applying cupping glasses in the operation of leeching, though, where leeching is appropriate, it must be often at the expense of a greater loss of blood, and at a loss more or less of the specific influence of leeching. (2) In other cases, too, where general bloodletting would be detrimental, cupping glasses thus employed will probably aggravate disease.

When arterial action is high, either from constitutional causes, or from sympathy with some local inflammation, general bloodletting is particularly required on account of the immediate and universal change which it induces in the sanguiferous organs, thereby relieving at once the instruments of disease of a redundant quantity of blood, by effecting a change in their vital powers,

(1) On External and Internal Inflammations.

(2) We have lately seen a remark by Dr. Stokes, which induces us to think, that this accurate observer makes the same discrimination as ourselves. In the treatment of "secondary bronchitis" he has employed the lancet "in a few cases with advantage;" but even here, and especially when it occurs at the advanced stages of typhus fever, he would substitute "local bleeding, particularly by cupping," since "the patient seems to derive from it the greatest relief." (a)

(a) On Diseases of the Chest, p. 85.

and immediately reducing the force with which the blood is distributed, besides the advantage of the powerful sympathetic influence which is determined upon the instruments of disease by a great and sudden change of action throughout the arterial system. On the other hand, when the general action becomes subdued, these results can only be obtained in an inferior degree by the repetition of venesection. But little or no change of action may then be induced in the vessels generally, or the effect of general bloodletting in respect to the sympathetic result may be lost in the influence of habit. But general bloodletting, as we have said, may be important in inflammations where no such general excitement prevails; although, as we have also seen, its manifestations will be less remarkable. And though its first impressions be sensibly felt by the whole system of vessels, its action may at last be modified or resisted by the changes it has established, or by the influence of the habit which itself has induced.

At other times, an irritability of the vascular system, depending in part, also, upon causes not yet stated, may be so established by small and repeated bleedings, where a greater loss of blood is necessary to produce the change which we have indicated, that it may become impracticable, at last, to carry the remedy to the same advantageous extent that might have been obtained at the onset of the disease. The irritability thus excited may even aggravate the inflammation it was intended to subdue, or become the foundation of the same condition of disease in other parts. In these cases of maltreatment, we must lay aside the lancet, and, perhaps, resort to leeches. (1) These imperfect abstractions of blood, however, are more frequently injurious in another way, which we shall endeavour to explain in another section.

In all the immediately foregoing cases, the peculiar and good effects of leeching, undisturbed by the force of the circulation, and the instruments of disease no longer sympathetically influenced by an excited, or other morbific state of the sanguiferous

(1) "There is no maxim," says Dr. Wardrop, "of the practical correctness and importance of which I am more fully convinced than that the loss of a certain quantity of blood at the *first bleeding*, is of greater utility in stopping the progress of inflammatory diseases when general bloodletting is required, than the abstraction even of a much larger quantity of blood by successive bleedings." (*a*) Here we see modern experience confirming what we shall hereafter state as to the habits of bye-gone times.

(*a*) On Bloodletting, p. 30.

organs, and predisposed by the impressions already made by general bloodletting to a progressive change,) are fully developed.

Dr. Wardrop thinks it "extremely probable, that there will be a variation produced according to the kind of blood which is taken away; that the removal of a pint of arterial blood will produce a different effect on the system, from the removal of the same quantity of venous blood. What these differences are, I cannot pretend to specify."(1) The only proof that is offered by the Dr. is, that "in those natural or spontaneous hemorrhages which are so salutary, the blood which is discharged appears to be venous." But here the difference, whenever it occurs, is generally constituted by the quantity discharged, which often exceeds the limit of safety in the artificial process. If our philosophy be true, which we have already expressed, and which may farther appear in the sequel, the only differences that can arise betwixt arterial and venous bloodletting will depend either upon accidental causes, such as the rapidity of the abstraction, position of the patient, &c., or whether the capillary veins or arteries be the instruments of disease. In either of the latter instances, doubtless if the blood could be taken directly from the minute vessels concerned in the morbid process, a greater effect would follow than when taken exclusively from the other series. In leeching and cupping, this object is always partially attained. But as it respects the abstraction of blood from the larger series of vessels, we can see no difference; and we believe the practical results have fully confirmed this conclusion. It is also stated by Mr. Travers, that there is a remarkable difference betwen venous and arterial bleeding in erysipelas, when incisions are made in the affected part. "The venous bleeding is beneficial; arterial is neither advantageous nor safe, as has been sufficiently proved. Divided arteries should be secured. If such patients are left but for a few minutes to bleed, they are left to die, and the object and end of this invaluable practice are misunderstood."(2) This is very strong language; but we believe the affirmation is only true, so far as it may be most advantageous to take blood directly from the vessels affected, which we think it probable are the veins, as we have more particularly stated in another place. As to any danger, we have seen none from the loss of large quantities of blood in delicate subjects. We gen-

(1) Op. Cit. p. 17.

(2) Farther Inquiry into Constitutional Irritation, &c. p. 148.

erally bleed from once to two or three times, and then, if necessary, apply leeches, which are vastly preferable to the incisions. We have never any trouble with erysipelas.

SECTION III.

We have yet said nothing of the instrumentality of the brain and nervous system in producing the phenomena which arise from general bloodletting.

We conceive that the great final cause of the brain is that of subserving the intellectual powers in man, and instinct in other animals. But reason and instinct would avail but little, were their operations confined exclusively to the brain; and hence that organ is prolonged, and its connection thus established with other parts of the body and with the external world. Admirable as is this design of associating in harmonious action the immaterial with the material part, it would still be defective, and the economy of nature obviously violated, were not an organ so prominent in the animal mechanism rendered subservient to the great purposes on which its existence depends. The primitive relations are all established by direct prolongations from the organ; but those which are more especially designed to answer its secondary uses belong to a connecting medium, which clearly denotes the nature of its specific function. Whilst the irregular connection of the sympathetic system with the brain, and its devious course, evidently declare that the organs which it may especially supply are not the subjects either of reason or of instinct, it binds together in harmonious action every part of the complex machine.(1) We need not speak of the specific rela-

(1) Müller thinks it has been shown, that "the constant source of the heart's contractility is, *primo loco*, the motor power of the sympathetic nerve. But the maintenance of this power, and its excitement, are dependent not only on the brain and spinal cord, but probably on the vital stimulus transmitted by all the organs of the body through the medium of the nerves accompanying the vessels to the central portions of the sympathetic." (a) We think there is reason to doubt, as we shall show

(a) Elements of Physiology, vol. i. p. 153.

tions that exist among all its members, and which mainly depend on this communicating medium, which is everywhere interlaced with the cerebral nerves, or the principle of sympathy which is one of their distinguishing properties. But the nature and importance of these general and specific relations must be clearly estimated in fully deducing the philosophy of the effects of bloodletting, since the brain has an important agency in many of the phenomena.

We now arrive at one of the great secondary uses of the cerebral system, which is that of co-operating with the ganglionic in establishing a circle of sympathies among the vital organs, and preserving the whole in that harmony of action that is indispensable to life. For, although it be evident, that the cerebral system is especially designed for the purposes of animal life, it is still obvious, from its compound connection with the sympathetic, that its office is carried into all parts of the organization, and that each system of nerves imparts to each its peculiar influences.

Thence we learn, that in complex animals the various parts of the organic mechanism are not only indispensable to each other, but that a certain established influence of one upon the other is necessary to each, and that the functions of the whole may be fatally deranged, either directly by causes that may interrupt the common chain by which the relations are established, or indirectly under certain circumstances, by withdrawing some particular organ from the symmetrical whole.

Whatever, therefore, may affect the powers and embarrass the functions of the brain will more or less disturb this concert of action, and derange the whole series of vital phenomena. It appears to be on this principle that we must explain the difference in the results of slightly varied experiments, those by which the brain is slowly destroyed interrupting the harmony of action more gradually, and therefore less fatally, than such as destroy at once the life of the organ.

The actions which are thus influenced through the connecting

in another place, the full extent of this principle. We are not wholly prepared to give up the facts by which Haller, and other eminent physiologists of later experience, have shown a certain independence of the heart's action of the cerebral and ganglionic systems. It is manifest, from the injuries which experiments on the nerves inflict upon the organic forces of the heart, that no very sound inductions can be made from such observations; and there is no common ground between ourselves and those who deny the existence of organic forces.

medium of sympathy are not alone the great specific functions of the organs, but equally, also, those of their constitutional organization. Besides this general law of sympathy, it possesses a modification by which the properties of individual organs are more specifically affected. Of this we shall say more in another place. (1)

Many contend that sympathetic relations exist amongst organs without any dependence upon the cerebral or ganglionic system. But this has never been shown, whilst analogy seems opposed to the conclusion. If it be generally true that sympathy appertains to the nervous system, and is not known to belong to another, it is a probable inference that it does exclusively. There can be no doubt, however, that the principle exists, and often operates independently of the brain, and this, too, when there is no direct connection betwixt the nerves of remote parts. The ganglionic system, as well as the brain, controls this principle. The influence appears to be propagated, in some instances, directly from one organ to another, in virtue of the nervous principle which is implanted in their organization. What may be the nature of the tissue in which it resides where nerves are apparently wanting, as in vegetables, we must acknowledge our ignorance. It is no more remarkable, however, than their sensible motions. We entirely agree with Mr. Reid in his conclusion, "that physiologists have been exceedingly premature in framing new systems of nerves to carry on the sympathetic and instinctive movements of the body." (2)

We are sensible that there are those who deride the belief even in the power of sympathy. (3) But the ancients were wiser than

(1) Müller thinks that "we are at present entirely ignorant as to whether irritation in one organ can, through the medium of the sympathetic, give rise to sympathetic movements in another; since all the sympathetic phenomena of this kind can be explained on the principle of reflection from the brain and spinal cord." (*a*) Perhaps this conclusion is opposed by the occasional absence of brain and spinal cord in full grown fœtuses; and we think it is by certain phenomena of disease which we have indicated in another place. Indeed Müller ultimately produces a variety of examples in organic life, entirely analogous to the animal sympathies, and which he concedes cannot be explained through the cerebral system. (*b*) The difficulty arises from the disposition to limit the term sympathy to the phenomena of the latter system.

(2) On the Eighth Pair of Nerves, in Edin. Med. and Surg. Journ. 1837.

(3) "What," says a writer of the present era of knowledge, and this, too, in a celebrated treatise on *Counter-Irritation*, "what do we know of the sympathies of themselves, except as regards certain *various effects* which we imagine, (by a reasoning purely metaphysical,) to be the product of a particular principle, the nature of

(*a*) Elements of Physiology, vol. i. p. 738.

(*b*) Ibid. pp. 751, 766.

such. The doctrine was elegantly taught by Hippocrates; and even Plato says of it, "oculorum morbosos affectus sanari non posse, nisi prius curetur caput, neque caput nisi prius curetur corpus, neque corpus sine animo aiebat medicus quidem apud Platonem." (1) And then as to the converse of this, which is supplied by the history of morbid actions, "it probably never happens that individuals," says M. Louis, "who die of a disease the seat of which is well determined, are free from lesions in other organs besides that primitively affected, at least, I never met with any example." (2)

These preliminary remarks are thought to be a proper introduction to the subject before us; and by way of farther preface, we may say that the controverted question as to the practicability of depleting the brain will hardly interfere with the ground of our conclusions. We formerly submitted to the public some facts and reasons to show that the brain may be as perfectly de-

which, or indeed its *real existence*, we are positively ignorant of?" (*a*) We answer this question, by referring to gravitation, cohesion, magnetism, &c. Explain to us, if you can, the nature or the existence of any of those forces which appertain to the inorganic world by any facts than such as are supplied by their operation; or, in other words, by their "various effects." So, also, of that other principle of the nervous system, *sensibility*. If you put your finger in the blaze of a candle, will you call it a "metaphysical" induction should you suffer pain? Well, then you must concede a principle through which the sensation is produced. This must have a name, which, like most other names, is arbitrary. Just so with sympathy and all the other vital forces; and just so, too, with those of physics and chemistry.

"The word," (sympathy) says Bichat, "is of but little consequence, provided what it expresses is understood." No one has employed his knowledge of it more usefully than this illustrious man; and yet strangely enough, whilst he affirms that "we know the principle exists," he also says, that the "word is only a veil for our ignorance in respect to the relations of the organs to each other." (*b*) But it is certainly no more a veil than irritability, sensibility, gravitation, magnetism, &c. Acting upon such a principle would be to abandon all the phenomena by which the names have been suggested. We shall never know the essence of any of the laws of nature, and probably because it can be of no practical advantage; and it might be to the detriment of the great chain of effects, should they become the hobby of experimental inquiry. With the little that was known by Archimedes, he only wanted a fulcrum to upset the earth.

See elaborate essays upon the power of sympathy by Dumas in his Principes de Physiologie, t. 2; Alison in Edin. Med. Chir. Trans. vol. ii.; and Brachet's Recherches Exp. sur les Fonctions du Sys. Nerv. Ganglion. c. 8.

(1) In Charmida.

(2) Researches on Typhoid Fever, vol. i. p. 356.

(*a*) Granville on Counter-Irritation, sec. iii. 1838.

(*b*) General Anatomy, vol. iii. pp. 234, 43, 44.

pleted as any other organ.(1) We have pursued that inquiry until demonstration has acquired the most entire precision. But we do not take it that others may be as well satisfied as ourselves. We think, however, that such as dissent will agree with Abercrombie, that in bloodletting there is at least a contraction of the cerebral arteries, whilst a corresponding fulness takes place in the cerebral veins. This change in the condition of the capillary system of the brain is sufficient for all our purposes.

There is, then, a general agreement that the same contraction attends the capillary and extreme vessels of the arterial system of the brain from loss of blood, that is common to all other parts. And since all sudden impressions upon the brain, whether of a stimulating or sedative nature, whether arising from changes in its relative proportions of blood, or from other causes, are more sensibly propagated to the vital organs than similar impressions on other parts; it would seem to follow, that a certain contraction of its capillary vessels, following a strong impression upon their vital forces, would more powerfully affect the action of the heart, than a like contraction of the same vessels in other parts; unless exceeded by the great amount of influence exerted upon the vital forces of the small series of vessels throughout the system, of which their universal contraction is the consequence. We need not say how well it has been determined by Dr. Philip, (and confirmed by many others,) that the heart and capillary arteries, more especially their extreme terminations, may be greatly and variously influenced in their action by impressions made upon the brain, especially when made upon the whole organ.(2)

But there are some who deny the accuracy of Dr. Philip's in-

(1) See Med. Chir. Review, vol. xx., p. 433. 1834.

We may add, in this place, that if the doctrine of Monro, Kellie, Abercrombie, &c., in regard to the peculiarity of the circulation of the brain be true, it should be universally applicable, at least to the human species. But during the whole period of infancy, whilst the bones of the cranium are ununited, the circulation of the brain must be subject to the common law which operates in other parts. And shall we suppose that another law, essentially different, and unknown to any other part of the body, supersedes the original when the cranium becomes an unyielding cavity? In such a case it would be, *prima facie*, apparent, that either man or nature has twice violated a common rule in philosophy.

(2) Whatever merit of originality may belong to Whytt, and Spallanzani, Dr. Philip has first applied these facts in a philosophical view, and rendered them subservient to the most important purposes.

ductions in regard to the connection of the brain with the modified actions of the capillary vessels. Thus, Müller affirms, that "the numerous experiments of Dr. Philip fail completely to establish the influence of the nerves on the circulation in the capillaries."(1) But when such statements are made in a work like our author's, they should be accompanied by corresponding proof; and this, we lament the necessity of saying, has been overlooked by Müller. Indeed, all his arguments in relation to the want of independent action in the capillaries and blood vessels, and the exclusive dependence of the circulation upon the action of the heart, are amongst the most feeble that have been advanced; whilst he has neglected to answer the facts that show the reverse. That we may not fall into a like error, however, we have shown, in another place, the nature of our author's objections, and in what respect they are deficient.

Again, our author does not make a distinction in respect to the general independence of the capillaries as regards the nervous system, and the cerebral influence which may be determined upon the capillaries by impressions made upon the brain. But he certainly states many facts, and even admissions, which not only show that the capillaries have an action of their own, but that this action may be influenced through the brain. Thus, "it is certain," he says, "that *nervous influence* is the *principal cause* of the accumulation of the blood in the *capillaries* of certain parts during the state of vital turgescence." "In the instantaneous injection of the cheeks with blood in the act of blushing, and of the whole head under the influence of violent passions, the *local* phenomena are evidently *induced by the nervous influence.* The active congestion of certain organs, of the brain, for example, while they are in a state of excitement, is a similar phenomenon."(2)

To such conclusions, all must arrive at last, however it may be a favourite object to show that the whole circulation is carried on by the action of the heart alone. They may endeavour, like Billing, to speculate away the glaring phenomena which Müller admits without reserve; but their very explanations confirm the doctrine which they would disprove.(3)

(1) Elements of Physiology, vol. i., p. 232.
(2) Ibid. pp. 233, 225.
(3) See Billing's First Principles of Medicine.
Dr. B. says that, "the moving powers (of the circulation) are, the *contractile force*

Prof. Mayer, repeating certain experiments which had been made by Bichat and others, though different from those of Dr. Philip, and far less satisfactory, concludes from them, that "the brain directs and guides the vital functions. The cause or principle, however, is not in the encephalon, but the impulse to exert these functions of vegetative life emanates from it." (1) So also, Mr. Travers. "I have formerly shown that the actions of the vascular system are under the control and superintendence of the nervous, and on no other hypothesis can I comprehend the physiological or pathological phenomena of the human body. If there was an oversight in the comprehensive views of Mr. Hunter, it was not his sufficiently appreciating this all-presiding influence in the economy, and especially on inflammation and its consequences." (2) This doctrine we believe to be fully established as to the influence which the nervous system may exert on organic actions; but we believe, as we shall show in our essays on the humoral pathology, and venous congestion, that the nervous influence, under ordinary circumstances, and even in inflammation, is far less than surmised by our able philosopher; so that, on the whole, Mr. Hunter only kept within his usual limit of taking nature for his guide.

But it is important to bear in mind, that the nervous influence may be more powerfully exerted upon the extreme vessels, than upon the heart. This, we think, is shown by Brodie's (3) and Chaussat's (4) experiments, in which, although the action of the heart was maintained after decapitation, the heat of the body cooled down rapidly to an equilibrium with the surrounding air. Here, too, all secretions are at once modified. So, also, even by dividing the nerves leading to any organ, the vital forces of its capillaries are so altered that their natural actions are immediately changed, whilst the heart is not equally affected by any division of nerves.

of the heart, *gravitation*, and the hydrostatic principle above stated, of the *tendency in fluids to* RETURN *to the same level.*" But our author has a great deal to say about the actions of the capillary vessels, and not a little as to the manner in which they are affected by the nervous influence. We think, indeed, that he makes it very clear that the foregoing mechanical causes are entirely inadequate to explain the phenomena of the circulation, secretion, &c., p. 9–38.

(1) British and Foreign Med. Rev.

(2) Further Inquiry into Constitutional Irritation, p. 137.

(3) Philos. Trans. 1811, 1812, and Edin. Journ. vol. viii.

(4) Mém. sur l'Influence du Sys. Nerv. sur Chaleur Animale.

An illustration of the power and nature of the cerebral influence upon the vital forces of the capillary blood-vessels is often seen in certain constitutions, where strong emotions of mind have the effect of subverting active disease. Thus, "a patient was so alarmed at the preparation for bleeding," says Dr. Armstrong, "that syncope occurred, and completely stopped an *inflammation of the pleura*." In this way, also, we must account for the real marvels of Perkin's tractors, animal magnetism, homœopathy, &c. "Cheer up the patient," says Armstrong, "and he is almost sure to do well."

Again, "when a patient had lost only an ounce of blood, from the shock of the operation, syncope came on and effectually removed an acute inflammation of the brain." (1) We may also take this case as a striking evidence of the truth of the principles upon which we have supposed bloodletting to operate.

It is from the cerebral influence which we are now investigating, and from the peculiar impression of local bloodletting on the vital forces of the extreme, and capillary blood-vessels, that in inflammation or venous congestion of the brain, a given quantity of blood taken from the head by means of leeches, will often more powerfully control the general circulation, than when taken from the vicinity of the lungs in similar affections of those organs, supposing the contraction of the instruments of disease to be equally rapid and complete in both cases. Dr. Armstrong remarks, that the benefit of leeching has been no where more apparent than in sub-acute inflammation of the brain; the pain having been in general greatly lessened, and sometimes wholly removed, while the pulse fell considerably, and a most remarkable change for the better took place in the expression of the countenance." (2) Thus, also, Dr. Wardrop. "A lady had long suffered from headaches. I advised her to apply leeches within the nostril. One was accordingly applied on each side of the septum, and her headaches were completely relieved. Another lady, after severe mental affliction, complained of uneasiness in her head, for which bloodletting was employed. She had frequent returns of the headache, and was always relieved by depletion. On one occasion, when a sense of fulness continued in the frontal region, after trying the usual modes, I advised her to

(1) Lectures on Acute and Chronic Diseases, vol. i. pp. 155, 401, 402.
(2) On Typhus Fever.

apply a couple of leeches to the nasal septum, and the bleeding had the happy effect of completely relieving her head."(1) Dr. Fordyce, in similar affections, "has seen three or four leeches applied to the temples give considerable relief to the patient by removing the pain, and sometimes they have carried off the whole fever."(2) These cases illustrate, also, what we have said in respect to the nature of the specific influences of leeching.

Again, an apparently different direction may be given to the cerebral influence upon the vital forces of the instruments of organic action; as, in cases where those forces may be prostrated by some pernicious influence of the brain, they may be restored by whatever may remove that influence, even though it be the loss of blood. Thus, the influence of leeching is such as sometimes to reanimate the system immediately, in the worst forms of apoplexy depending on cerebral congestion, "when the smallest quantity of blood," according to Dr. Philip, "is taken from the head."(3) If, however, in these cases of apoplexy, general bloodletting be carried to any considerable extent, the pernicious influence of the brain, as we shall endeavour to show in a subsequent section, will be more completely established. Here the cerebral influence upon the vital forces is shown under different aspects.

In cerebral inflammations, however, it is often necessary to carry bloodletting to a much greater extent than in pneumonia, or in the inflammations of any other organ, before the general circulation can be reduced, or syncope take place. It is then obvious, also, that a greater impression has been made upon the instruments of disease, by equal quantities of blood, in the latter cases than in the former. So great is the sympathy between vessels of the same order, and especially those in which the vital powers are most active, that whilst those which are the instruments of inflammation refuse to contract, the whole series throughout the body are maintained in a state of correspondence. This is peculiarly true when the brain is the seat of inflammation; for, whilst the contraction of its capillary vessels tends, as a sedative, to powerfully prostrate the general circula-

(1) On Bloodletting, p. 19.

(2) Diss. on Fever, vol. ii. p. 129.

(3) Essay on Indigestion, p. 148, et seq.

In Mr. Hey's ninth case of puerperal fever, which we believe with Mr. Hey to have been one of severe inflammation of the brain supervening on a similar abdominal affection, the loss of three ounces of blood from the head was productive of relief.

tion, their refusal to contract, on the other hand, not only co-operates on the principle just mentioned to sustain the general circulation, but the influence of a stimulant is still exerted on the organ, and with its acquired peculiarities, still propagated to the heart and capillary vessels. The contraction of the cerebral vessels is partly prevented by the peculiarly modified state of their vital powers, and, in part, by the tendency of that modification to prevent a contraction of the corresponding vessels in other parts. The peculiarity of this modification arises from the nervous influence which is exercised upon the vessels of the brain in a state of inflammation, and is thus distinguished from that condition of the vital forces which affects the small vessels when the seat of inflammation is in other parts. But in inflammations of other organs, where the peculiar relations of the brain to the instruments of disease do not operate as when that organ is diseased, the influence of the law by which excited vessels hold in partial subjection the corresponding series of vessels throughout the body, is sooner overcome, and the general contraction followed sooner. More complex laws are evidently involved in one instance than in the other. Still, it is manifest that the same principle operates in inferior degrees in inflammations of other parts. They exert an influence on the brain by which the nervous power is so increased, that it contributes to maintain the excitement of the vascular system.

The peculiarity of the supposed law in respect to the brain seems to be sustained by the experiments to which we have alluded, which prove that the nervous influence is capable of acting as a stimulant not only to the heart, but to the capillary blood-vessels. The same influence was also found to be capable of acting as a sedative upon the same organs, to such a degree as to extinguish their vitality in the most direct manner. (1)

(1) In Dr. Hall's experiments, which appear to have been only two, the opium was first applied to other parts of the body, and the animal thus rendered perfectly insensible. Its action was, therefore, in this manner fully established upon the nervous system, and the nervous influence fully determined upon the heart. Under these circumstances we should look in vain for any farther effect from opium when applied directly to the brain. We must take our illustration from the 14, 15, 16, and 23 experiments of Dr. Philip, in which the animals were prepared by a blow on the occiput, or by suspending respiration. In regard to the other experiments of Dr. Philip, their accuracy cannot be questioned. He either saw or did not see the results which he so circumstantially describes, and which have been confirmed by others. Many of the phenomena of life, especially such as grow out of the passions, are almost parallel examples.

Hence, also, is the inference sustained, that the contraction of the cerebral vessels, in bloodletting, not only affects more powerfully the action of the heart than that contraction in other parts, but that the general contraction is accelerated by the same cause; the action of the heart being thus directly and indirectly affected by the contraction of the capillary vessels of the brain.

It is also manifest, that peculiar impressions will be determined upon the brain by the loss of blood, and thence propagated with varying effects upon other parts, according to the natural constitution of each individual; the nature and extent of disease, and the sympathies it exerts, and especially whether the brain may be morbidly affected, and according to the exact nature and extent of the affection. These are circumstances which often play an important part in the phenomena of bloodletting, leading to syncope from an ounce of blood, where we may have calculated upon a pound or more. The effect, therefore, of loss of blood may throw, at once, a flood of light upon some obscure conditions of disease, or upon some natural peculiarities of constitution. If, however, abstraction of blood have been clearly indicated by the vital phenomena, and syncope have followed at the onset of the operation, our doctrine is exactly the reverse of Dr. Hall's, and we shall endeavour to show that this apparent inability of the system to bear the loss is a general proof that bloodletting is highly important, and that the system may be brought into a condition to bear a greater loss by the loss itself.

It appears, however, to be not alone the action of the heart and the vessels of circulation which are peculiarly influenced by the contraction of the cerebral vessels. It is shown by the experiments of Le Gallois,(1) that the stomach, also, is as readily and as powerfully influenced through the brain as the heart;(2)

(1) Exp. on Life, p. 198.

(2) Thence it appears why the shock of a fall, or of a surgical operation, produces vomiting; why depressing emotions destroy the appetite; why disgusting objects produce nausea, &c. The experiments of M. Brachet were therefore scarcely necessary to show that a division of the nervous communication betwixt the brain and stomach would be followed by a loss of appetite. But it does not appear equally to follow from these experiments that the sense of hunger depends essentially on that communication. A violence may be thus inflicted upon its organic powers, or they may be more directly impaired through the cerebral influence; and yet, like the heart, enjoy their peculiar independence.

and this, too, appears *a priori* from the analogous effects of mental emotions upon the stomach. The aid of experiments is scarcely necessary in any of the cases, if we but follow nature in our inductions.

The stomach, therefore, undoubtedly often performs, by direct and indirect sympathies, an important part in the phenomena of bloodletting, especially such as immediately precede the paroxysm of syncope. Hence, also, the frequent nausea which supervenes on the contraction of the cerebral vessels, or as syncope approaches. The same may be more or less affirmed of the intestines, which is especially denoted by the evacuations which often begin when the brain manifests its signs of influence.

Various other conditions arise as syncope approaches, which denote an especial connection of the nervous influence with the paroxysm. But we apprehend that this influence is not strongly exerted till syncope is about taking place. The antecedent results depend more on the impression upon the vital forces at large, though in proportion as the loss of blood bears upon the organic powers of the brain, the cerebral influence will contribute its effect upon the system along with the universal impression which the loss independently produces. But much of the impression upon the brain arises from the direct influence of the remedy upon the powers of the whole system, and in proportion to this influence, and the consequent sympathy of the heart, will be in part the impression upon the brain, and, consequently, the rapidity and force with which the brain will react. And although the circle of sympathies is thus complex, it appears to us of no difficult analysis. (1)

Wepfer speaks particularly of the influence of the brain over the stomach, and the reciprocal effects which the stomach may exercise upon the brain. (*a*)

(1) It is the doctrine of many, that bloodletting has answered its end, if followed by syncope. This, however, we shall endeavour to show in another place, is not generally true, if the loss of blood have been small, and syncope not long continued. But it happens, occasionally, that syncope is protracted, when but little blood has been taken away. In these cases, a powerful impression has been produced upon the brain, when the nervous influence upon the vital forces may answer all the purposes of the loss of a greater quantity of blood. In the case already quoted from Dr. Armstrong, where the loss of eight ounces of blood "caused a long continuance of faintness," the patient, on recovering, was completely relieved of a severe inflammation. "The longer the duration of syncope," says Dr. Wardrop, "so much the more completely will the inflammatory action be subdued." (*b*)

(*a*) Hist. Apoplecticorum. Hist. 12. Schol.

(*b*) On Bloodletting, p. 27.

A powerful impression begins, at last, to be determined upon the heart by the vital influences of the capillary system, and by the cerebral influence; to which is superadded an influx of blood upon the heart, in consequence of the universal contraction of the small blood-vessels. As the embarrassment of the heart increases, it is, at last, felt not only directly by the brain in the diminished supply of blood, but through the principle of sympathy re-acting on that organ, to which are rapidly added those other influences from coincident impressions upon the vital forces of the whole capillary system; by which the phenomena of syncope are more or less speedily consummated. (1)

From these considerations, it appears to result, that the contraction of the cerebral vessels, and the consequent impression on the brain, determine a specific influence in the process of bloodletting. That the abstraction of blood, when carried to the extent of inducing syncope, exerts the impression upon the brain which we have supposed, and that this is reflected upon the system, appears also, from that permanent irritability of the organ, which is sometimes induced by excessive bloodletting. It is true, that much of this phenomenon may be owing to a direct affection of the vital properties of the entire system. But since the excessive loss of blood mainly determines inflammations and congestions of the brain, there is strong ground for believing that this irritable state of the body is greatly owing to some direct affection of the cerebral powers. We think, too, that it is sometimes in this way, that apoplexy has been induced by bloodletting; and not, as has been supposed, by its interrupting the circulation in the brain, or by the direct depletion of that organ.

In subjects, too, where the nervous temperament predominates, and in the phlegmatic, also, the cerebral influence may be sometimes fully obtained by small abstractions of blood. "It is singular," says Dr. Wardrop, "how small a quantity of blood some persons can lose; even the very impression on the mind, that blood is to be abstracted, causes sickness, and fainting comes on after a few ounces of blood have been removed. In such cases, however small the quantity which is evacuated, there is often a decided relief, the syncope sometimes causing even a permanent subsidence of the inflammatory symptoms." (a) We do not believe, however, if inflammation be at all severe, in cases of this nature, that disease will yield much to small abstractions of blood, unless syncope be protracted.

(1) Dr. Kellie supposes, that "a certain range of pressure from the general circulation is necessary for the due performance of the cerebral functions." (b)

(a) On Bloodletting, page 33.
(b) Trans. Med. Chir. Soc'y Edin. 1824. p. 104.

The properties of the brain being strongly modified in a way that constitutes a predisposition to that affection, they have been at once overwhelmed by those direct impressions upon them which we have hitherto considered. At other times, when the same strong predisposition has affected the cerebral powers, and the action of the heart has been subdued by some co-existing congestion, perhaps of the brain, the circulation being suddenly liberated, the paroxysm may be determined by the sudden and forcible impulse of blood.

Such being the influences of bloodletting upon the brain, we readily account for those inflammations, and that disease of irritation of modern physicians, which occasionally supervene on the excessive loss of blood. Here the impression has been directly on the instruments of action, and on those vital powers by which they are animated. The whole capillary system has a large share in the primary impression; but a peculiar influence is determined by the violence inflicted on the extreme capillaries of the brain. Hence, also, that delirium, coma, stertorous breathing, and those convulsions, retchings, and involuntary intestinal evacuations, some of which so frequently follow excessive loss of blood. (1) Although bloodletting, therefore, be a remedy for inflammation, the excessive use of it, as we shall farther show, may induce that affection; and even then, the cautious abstraction of blood by means of leeches still proves, by its influence, the nature of the affection, and the sanative power of the remedy when well directed. (2)

It might be also shown, that the changes which so constantly arise in the blood during venesection are owing to the impression exerted upon the vital forces of the extreme vessels of the whole system, rather than to a change of action in the part inflamed. These vessels perform the last great office in perfecting the vitality of the blood, and any universal and simultaneous

(1) Hence these developments are most apt to arise in artificial temperaments, which depend upon causes that have induced a preternatural irritability of the nervous system.

The instrumentality of the brain in the process of bloodletting may be also inferred from the phenomena that attend the sudden death of this organ, — the fluidity of the blood, its rapid decomposition, as well as that of the whole body, &c. arising from peculiar influences of the brain upon the vital forces, and their instruments of action. Thence, also, we conclude, *a fortiori*, that the usual coagulation of the blood is an evidence of its vitality, although we regard it as a consequence of the failure of life.

(2) See section on excessive loss of blood.

change in their action will be very likely to influence the blood which they hold so much in subjection. (1)

When syncope supervenes, if the subject be placed in a horizontal posture, animation returns; or, it may be again suspended by reversing the position. These phenomena, we apprehend, depend upon causes now essentially modified, — reviving, it is true, or depressing the actions of life. "No man ever saw the sensorial functions continue a single minute after the heart had ceased to move. When the whole body is horizontal, the heart circulates the blood more easily, than when any part, and especially so large a part as the head is elevated." (2) If syncope return when the head is again elevated, it will depend on a more simple cause than what originally produced it. It will now arise from a permanently enfeebled state of the heart, and "its inability to continue the circulation, and thus to supply the brain and all other parts with a current of blood;" although, it be true that this is always the last in the series of causes, in a paroxysm of syncope. In the first instance, the action of the heart is prostrated through the nervous influence of the brain and the general influence reflected from the entire series of small vessels; in the second, the functions of the brain are impaired or suspended, through the enfeebled action of the heart.

That syncope does not depend on the sudden abstraction of blood from the brain, except from the failure of the heart's action, seems to be evident from the fact, that the paroxysm is frequently produced by the loss of a quantity of blood so small as not to affect the general circulating mass in any appreciable degree. But, if we bring to these cases that modified state of the vital powers of the extreme capillary system throughout the body, as well as the coincident specific effect on the brain, the consequent influence exerted upon the heart and stomach, with their reacting influences, and the oppression of the former organ from the quantity of blood so suddenly determined upon it by

(1) Dr. Pallas, having made the blood in the capillary vessels the subject of experiment, perceived many indications, which denoted its greater vitality than in other parts.(a) Others have come to the same conclusion by a similar process; but this sort of proof is rather conjectural.

(2) Johnson's Med. Chir. Rev. vol. 18. p. 355.

It is upon this principle that syncope is more or less relieved, by elevating the extremities, or by compressing the large vessels, — especially by the latter expedient,

(a) Jour. de Chimie Med. Oct. 2, 1828.

the general contraction of the capillaries, there appears to be presented the succession of changes which leads to that enfeebled action of the heart which consummates the paroxysm. That the essential changes, too, do not depend on the abstraction of blood from the brain, is seen by suddenly obstructing the transmission of blood through the carotid arteries, — when no derangement of the cerebral functions will take place. (1)

The properties of the brain are impressed by the general abstraction of blood; but its functions are not essentially impaired till its circulation mainly ceases. And even now, syncope is produced and maintained as well through the nervous influence, as from a prostration of the cerebral functions, — since, if we crush the cervical portion of the spinal marrow, life can only be maintained by previously cutting off the head of the animal. This arises, in part, from the inability of the heart to supply the brain with blood, but more from the nervous influence on the heart when the brain is not supplied. Again, the injuries which are inflicted on the spinal cord, to determine the specific functions which have been assigned to it, are so severely propagated to the brain, and may so affect the properties of that organ, that the results which appear to flow from the spinal marrow, may be actually due to the cerebral influence, or to an interruption of that influence when the spinal cord is divided or destroyed. Both principles, in the latter case, may act in co-operation; the cerebral influence being determined through the superior nerves and the ganglionic system, and otherwise impressed by a reflected influence from below that part of the spinal cord, where its direct connection with the brain is interrupted.

Whilst, therefore, the brain remains, experiments upon the spinal cord are entitled to much less confidence than those which are made upon the brain. But even when the brain is removed, the vital forces of all parts become so profoundly modified in consequence, that we can scarcely infer with accuracy the specific functions of the spinal cord from subsequent experiments.

The foregoing observations lead us to another exemplification of the peculiar direction which is given to the nervous influence by the *sudden* abstraction of blood. We may gradually destroy the brain of a rabbit, without suspending the action of the heart; but if we suddenly crush the brain, the life of the animal

(1) See Bichat sur la Vie, &c. p. 257, and many others.

is immediately extinguished. So will this influence be felt by the heart, in general bloodletting, in the ratio of the rapidity in which the cerebral vessels are brought into a state of contraction. It was found, indeed, by Dr. Philip, that the extent of the nervous influence over the heart and arteries depended, always, on the suddenness of the impression on the brain, and that when most sudden and violent, it was capable of extinguishing all muscular power. (1) Hence one of the differences betwixt general bloodletting and leeching, and why, in the language of Mr. Travers, "Syncope is in proportion to the suddenness, rather than the quantity of the hemorrhage." (2) And we may say, that what Dr. Philip has so conclusively shown by experiment, Mr. Travers has inferred by a different method, though to a greater, and perhaps inadmissible, extent, "that the actions of the vascular system are under the control of the nervous; and on no other hypothesis can we comprehend the physiological and pathological phenomena of the human body." (3)

Thus it seems, that the various changes which take place in the action of the heart, when they arise from the loss of blood, are chiefly dependent upon the law of sympathy. At no stage of that complex series of changes, from the first impression that follows the loss of blood, till it ends in syncope, is there a deficiency of blood at the centre of the circulation. And when, at last, death ensues from hemorrhage, "quelle que soit la quantité de sang perdu il en reste encore beaucoup dans les centres circulatiores." (4)

It is the opinion of an eminent physician and physiologist,

(1) Dr. Hall thinks that "no physiological deduction can be drawn from the experiment of crushing the brain and spinal marrow, in regard to the functions of these organs, since precisely similar effects do result from crushing the stomach or a limb. (a) We may add, too, that patients have suddenly died on the opening of a thecal abscess, the removal of a testicle, &c. But is it not evident, that this result takes place through the medium of the nervous system, and by a direct destruction of the cerebral powers? To our minds, this objection appears to confirm the accuracy of Dr. Philip's conclusions. It is another mode of illustrating the principle.

(2) Inquiry into the Vital Functions, p. 340.

(3) Further Inquiry into Constitutional Irritation, p. 137.

(4) Piorry's Collection de Mémoires, p. 226.

Mr. Hales, also, states the same fact. In the horses which he bled to death, blood remained in the auricles and ventricles, "and the larger veins were full of blood." (b) It may be readily seen at any slaughter-house. The butchers generally open the heart to let out the accumulated blood.

(a) Essay on the Circulation of the Blood.

(b) Hæmastatics, Exp. 3, p. 16.

that the "action of the heart in syncope becomes enfeebled, first, from a deficient quantity of blood, and, secondly, from its deficient arterialization." (1) That the second cause assigned, like the first, has little or no connection with the phenomena is probable from the fact, that when syncope follows the loss of blood from an artery, there is no change in the natural colour of the blood, — and from respiration being often only affected at the moment of the paroxysm, and then evidently depending upon it. If this, too, were an important cause, how is the heart again brought into action after protracted syncope, and a long continued interruption of respiration?

Syncope is often consummated by removing the ligature. In this case, the action of the heart had been enfeebled almost to an accession of the paroxysm; and the additional quantity of blood suddenly thrown upon the heart overpowers its action. It is in this way, in part, when the heart has been gradually prostrated during the access of congestive fever, that a sudden development of the attack may sometimes produce syncope. Something, however, is evidently owing to the sympathetic influence of the extreme vessels upon the heart, — but more to the determination of blood from the circumference, during the cold stage. Aretæus probably had his eye upon this condition of disease, when he said, "at ubi foræ multitudine syncope fiat, et inflammatio aliqua insignis in præcordiis aut jecinore appareat, nequaquam procrastinare expedit." And it were well that his remedy should be duly considered. "Secanda est igitur vena in cubito, per minutas sectiones, id est, scarificationem; quo facultas haud ita insignem jacturæ faciat. Si vini potandi necessitas homini incidit, diligenti consideratione opus est." (2)

It appears, therefore, from all that has been said, that the loss of blood produces its effects directly, both through the irritability of the vessels and through the nervous influence variously propagated. That in leeching, the *primary* impression is mainly upon the irritability of the sanguiferous system, involving, however, the property of sympathy; whilst in general bloodletting, the nervous influence is brought into co-operation the moment the change begins in the capillary vessels, and increases as the influence of the remedy progresses. (3)

(1) Medico Chir. Trans. vol. 13, p. 123.

(2) De Cur. Acut. l. 2. c. 3.

(3) On the foregoing principles, it would appear to be of little importance in general bloodletting from what part the blood may be abstracted.

When syncope arises from the depressing emotions, or from other causes whose primary impression is upon the brain, the action of the heart appears to be directly prostrated through the nervous influence, and indirectly by its sympathy with the stomach; whilst a certain depressing effect is exerted by the nervous power upon the extreme and capillary blood-vessels, and an influence from this change is propagated to the heart. The succession of changes then, as respects the heart and blood-vessels, is directly reversed; the contraction of the capillary vessels being consequent on the failure of the heart's action. We must also explain, in the foregoing manner, the syncope which follows blows upon the stomach, the crush of limbs, surgical operations, &c.; and when death is *suddenly* produced by any of these causes, it is either owing to a sudden extinction of the cerebral powers, or to a powerful determination of the cerebral influence upon the heart, by which the action of that organ is arrested.

It is the opinion of M. Piorry, that "syncope, whatever may be its cause, consists in a suspension or diminution of cerebral action. If it take place spontaneously and from a moral cause, it is the action of the encephalon that is suspended; it is the influence of this organ upon the heart which is diminished." (1) When we consider how powerfully the heart may be influenced by slight mechanical or chemical agents applied to the brain, even when its circulation is destroyed and the whole inferior portion of the organ removed, we shall better understand in this way, how odours, offensive sights, and moral causes, produce syncope, than by supposing that they have the power of directly suspending the cerebral functions. Violent passions have, doubtless, the effect of extinguishing, at once, the powers and functions of the brain; but then, the action of the heart ceases entirely, and is clearly owing to the sudden death of the brain; whilst in syncope, the action of the heart is only diminished. If it be said that the principle is the same, and that in either case, the functions of the brain are impaired or destroyed by the influence transmitted to the heart, and that the only difference consists in the degree of that influence, it may be replied that the difference in the nature and usual effects of the causes appears to indicate a difference in the affections. It is only the

(1) Op. Cit. p. 236.

depressing emotions, like fear, grief, disgust, and such causes as in any degree exert a sedative influence on the circulation, that are known to produce undoubted syncope; whilst those like joy and anger, which always excite the action of the heart, alone extinguish life instantaneously. One affection, too, is common, whilst the other is rare; and when the latter takes place, it is probable that there exists an apoplectic predisposition. In one case, the action of the heart is suddenly depressed; in the other, it is powerfully excited. Doubtless, too, in the latter instance, the violent impulse of blood upon the brain contributes, *per se*, to the sudden extinction of the cerebral powers. Whilst, therefore, in syncope, from fear and grief, the blood is, at the onset, diverted from the head; in sudden death from joy or anger, a preternatural quantity is determined upon the brain. Hence, in part, extravasations of blood have taken place in the brain, and other parts of the body, though, we doubt not, the result of disease thus induced; and even the heart itself has been ruptured by their influence.

Bichat, being unacquainted with the influence which the brain is capable of exercising over the heart, and supposing, indeed, that he had experimentally proved that such influence does not exist, (1) concluded that the heart never dies directly in affections of the brain, but through the intermediate agency of the lungs, the black blood being the immediate cause of death in all the internal organs. (2) We have witnessed three or four examples of cerebral apoplexy where life was extinct within one minute or less after the attack. On two of the occasions we were present at its invasion. Here, as when the brain is suddenly destroyed, the vital forces are directly extinguished through the nervous power.

From the foregoing considerations, we conclude that when moral causes produce syncope, the primary affection of the brain consists only in some specific impression on its properties, and that its functions are only suspended by the failure of the heart's action. We infer, also, that the influence of the brain upon the heart, so far from being "diminished," is actually increased. This modification of the cerebral powers is constantly mistaken, as it appears to us, for a failure of functions.

(1) Recherches Phys. Sur la Vie, et la Mort. Part 2, c. 11, p. 480 — 489.
(2) Ibid. pp. 500 — 508, 527.

If, continues M. Piorry, "syncope be owing to hemorrhage, it is because the brain ceases to be excited by blood."(1) But why does the brain cease to be excited by blood? There must be a previous failure of the heart's action, which, of course, becomes the cause of a temporary diminution of the functions of the brain, and of all other organs. But the functions of the brain are, at last, not more impaired than the action of the heart. Whether the cerebral influence, of which we have spoken, be not an important cause of the failure of the heart's action is quite another question.

M. Piorry justly considers a decision of this question of practical importance, "as the treatment of syncope will depend on the precise seat we may assign to it." Our first object, therefore, would appear to be to re-establish the action of the heart; and as the brain has been instrumental in producing its partial diminuion, we must variously excite this organ, especially by its natural stimulus, and thus employ it as a powerful means of restoring the circulation. We must not only lay the patient horizontally, supply fresh air, stimulate the nostrils and lungs, but when the paroxysm is obstinate, we should agreeably excite the mind. When syncope arises from the loss of blood, we have seen that a peculiar shock has been inflicted on every part of the system from the beginning, and that every part, in various degrees, influences the action of the heart. This influence is far greater than when moral causes operate, from the greater impression on the vital forces of the extreme vessels; and when syncope follows, this general influence on the heart is greatly augmented. It is apparent, therefore, in obstinate cases, that impressions may be advantageously made on different organs: "Those who have found it necessary," says Dr. Armstrong, "to make a liberal use of the lancet in simple continued fever, must have observed how difficult it is, especially after a second abstraction of blood, to rouse the system into action. As far as my observation goes, nothing will so soon effect this, as the application of small blisters to the upper and lower extremities. They seem suddenly to excite the *capillary vessels, by which the heart is soon brought into play.*"(2) These vessels had a powerful agency

(1) Op. cit. p. 236. See also his Traité de Diagnostic et Séméiologie, t. 3, 1838.

(2) On Pulmonary Consumption.

Woodall, in his Surgeon's Mate, which would be creditable in many respects to the most enlightened age, gives the following directions. "Bende the patient

and this, too, appears *a priori* from the analogous effects of mental emotions upon the stomach. The aid of experiments is scarcely necessary in any of the cases, if we but follow nature in our inductions.

The stomach, therefore, undoubtedly often performs, by direct and indirect sympathies, an important part in the phenomena of bloodletting, especially such as immediately precede the paroxysm of syncope. Hence, also, the frequent nausea which supervenes on the contraction of the cerebral vessels, or as syncope approaches. The same may be more or less affirmed of the intestines, which is especially denoted by the evacuations which often begin when the brain manifests its signs of influence.

Various other conditions arise as syncope approaches, which denote an especial connection of the nervous influence with the paroxysm. But we apprehend that this influence is not strongly exerted till syncope is about taking place. The antecedent results depend more on the impression upon the vital forces at large, though in proportion as the loss of blood bears upon the organic powers of the brain, the cerebral influence will contribute its effect upon the system along with the universal impression which the loss independently produces. But much of the impression upon the brain arises from the direct influence of the remedy upon the powers of the whole system, and in proportion to this influence, and the consequent sympathy of the heart, will be in part the impression upon the brain, and, consequently, the rapidity and force with which the brain will react. And although the circle of sympathies is thus complex, it appears to us of no difficult analysis. (1)

Wepfer speaks particularly of the influence of the brain over the stomach, and the reciprocal effects which the stomach may exercise upon the brain. (*a*)

(1) It is the doctrine of many, that bloodletting has answered its end, if followed by syncope. This, however, we shall endeavour to show in another place, is not generally true, if the loss of blood have been small, and syncope not long continued. But it happens, occasionally, that syncope is protracted, when but little blood has been taken away. In these cases, a powerful impression has been produced upon the brain, when the nervous influence upon the vital forces may answer all the purposes of the loss of a greater quantity of blood. In the case already quoted from Dr. Armstrong, where the loss of eight ounces of blood "caused a long continuance of faintness," the patient, on recovering, was completely relieved of a severe inflammation. "The longer the duration of syncope," says Dr. Wardrop, "so much the more completely will the inflammatory action be subdued." (*b*)

(*a*) Hist. Apoplecticorum. Hist. 12. Schol.

(*b*) On Bloodletting, p. 27.

A powerful impression begins, at last, to be determined upon the heart by the vital influences of the capillary system, and by the cerebral influence; to which is superadded an influx of blood upon the heart, in consequence of the universal contraction of the small blood-vessels. As the embarrassment of the heart increases, it is, at last, felt not only directly by the brain in the diminished supply of blood, but through the principle of sympathy re-acting on that organ, to which are rapidly added those other influences from coincident impressions upon the vital forces of the whole capillary system; by which the phenomena of syncope are more or less speedily consummated. (1)

From these considerations, it appears to result, that the contraction of the cerebral vessels, and the consequent impression on the brain, determine a specific influence in the process of bloodletting. That the abstraction of blood, when carried to the extent of inducing syncope, exerts the impression upon the brain which we have supposed, and that this is reflected upon the system, appears also, from that permanent irritability of the organ, which is sometimes induced by excessive bloodletting. It is true, that much of this phenomenon may be owing to a direct affection of the vital properties of the entire system. But since the excessive loss of blood mainly determines inflammations and congestions of the brain, there is strong ground for believing that this irritable state of the body is greatly owing to some direct affection of the cerebral powers. We think, too, that it is sometimes in this way, that apoplexy has been induced by bloodletting; and not, as has been supposed, by its interrupting the circulation in the brain, or by the direct depletion of that organ.

In subjects, too, where the nervous temperament predominates, and in the phlegmatic, also, the cerebral influence may be sometimes fully obtained by small abstractions of blood. "It is singular," says Dr. Wardrop, "how small a quantity of blood some persons can lose; even the very impression on the mind, that blood is to be abstracted, causes sickness, and fainting comes on after a few ounces of blood have been removed. In such cases, however small the quantity which is evacuated, there is often a decided relief, the syncope sometimes causing even a permanent subsidence of the inflammatory symptoms." (*a*) We do not believe, however, if inflammation be at all severe, in cases of this nature, that disease will yield much to small abstractions of blood, unless syncope be protracted.

(1) Dr. Kellie supposes, that "a certain range of pressure from the general circulation is necessary for the due performance of the cerebral functions." (*b*)

(*a*) On Bloodletting, page 33.
(*b*) Trans. Med. Chir. Soc'y Edin. 1824. p. 104.

The properties of the brain being strongly modified in a way that constitutes a predisposition to that affection, they have been at once overwhelmed by those direct impressions upon them which we have hitherto considered. At other times, when the same strong predisposition has affected the cerebral powers, and the action of the heart has been subdued by some co-existing congestion, perhaps of the brain, the circulation being suddenly liberated, the paroxysm may be determined by the sudden and forcible impulse of blood.

Such being the influences of bloodletting upon the brain, we readily account for those inflammations, and that disease of irritation of modern physicians, which occasionally supervene on the excessive loss of blood. Here the impression has been directly on the instruments of action, and on those vital powers by which they are animated. The whole capillary system has a large share in the primary impression; but a peculiar influence is determined by the violence inflicted on the extreme capillaries of the brain. Hence, also, that delirium, coma, stertorous breathing, and those convulsions, retchings, and involuntary intestinal evacuations, some of which so frequently follow excessive loss of blood. (1) Although bloodletting, therefore, be a remedy for inflammation, the excessive use of it, as we shall farther show, may induce that affection; and even then, the cautious abstraction of blood by means of leeches still proves, by its influence, the nature of the affection, and the sanative power of the remedy when well directed. (2)

It might be also shown, that the changes which so constantly arise in the blood during venesection are owing to the impression exerted upon the vital forces of the extreme vessels of the whole system, rather than to a change of action in the part inflamed. These vessels perform the last great office in perfecting the vitality of the blood, and any universal and simultaneous

(1) Hence these developments are most apt to arise in artificial temperaments, which depend upon causes that have induced a preternatural irritability of the nervous system.

The instrumentality of the brain in the process of bloodletting may be also inferred from the phenomena that attend the sudden death of this organ, — the fluidity of the blood, its rapid decomposition, as well as that of the whole body, &c. arising from peculiar influences of the brain upon the vital forces, and their instruments of action. Thence, also, we conclude, *a fortiori*, that the usual coagulation of the blood is an evidence of its vitality, although we regard it as a consequence of the failure of life.

(2) See section on excessive loss of blood.

change in their action will be very likely to influence the blood which they hold so much in subjection. (1)

When syncope supervenes, if the subject be placed in a horizontal posture, animation returns; or, it may be again suspended by reversing the position. These phenomena, we apprehend, depend upon causes now essentially modified, — reviving, it is true, or depressing the actions of life. "No man ever saw the sensorial functions continue a single minute after the heart had ceased to move. When the whole body is horizontal, the heart circulates the blood more easily, than when any part, and especially so large a part as the head is elevated." (2) If syncope return when the head is again elevated, it will depend on a more simple cause than what originally produced it. It will now arise from a permanently enfeebled state of the heart, and "its inability to continue the circulation, and thus to supply the brain and all other parts with a current of blood;" although, it be true that this is always the last in the series of causes, in a paroxysm of syncope. In the first instance, the action of the heart is prostrated through the nervous influence of the brain and the general influence reflected from the entire series of small vessels; in the second, the functions of the brain are impaired or suspended, through the enfeebled action of the heart.

That syncope does not depend on the sudden abstraction of blood from the brain, except from the failure of the heart's action, seems to be evident from the fact, that the paroxysm is frequently produced by the loss of a quantity of blood so small as not to affect the general circulating mass in any appreciable degree. But, if we bring to these cases that modified state of the vital powers of the extreme capillary system throughout the body, as well as the coincident specific effect on the brain, the consequent influence exerted upon the heart and stomach, with their reacting influences, and the oppression of the former organ from the quantity of blood so suddenly determined upon it by

(1) Dr. Pallas, having made the blood in the capillary vessels the subject of experiment, perceived many indications, which denoted its greater vitality than in other parts.(a) Others have come to the same conclusion by a similar process; but this sort of proof is rather conjectural.

(2) Johnson's Med. Chir. Rev. vol. 18. p. 355.

It is upon this principle that syncope is more or less relieved, by elevating the extremities, or by compressing the large vessels, — especially by the latter expedient.

(a) Jour. de Chimie Med. Oct. 2, 1828.

In the experimental cases, the effusion takes place chiefly from the lungs, (1) where the whole mucous tissue is actively engaged in the vital process of eliminating the aqueous part of the blood; and this is a farther proof that the augmented effusion depends upon the same process of the vital actions. M. Andral admits that the vessels must be distended for sometime; during which we infer that the irritation from the stimulus of distension produces the results. Bloodletting, by diminishing irritability, counteracts, or overcomes that irritation.

If this extraordinary production of serum under circumstances of health, and the result of fluids injected into the circulation, be truly a vital effect, how much more probable is it, that it depends upon some analogous law when it arises spontaneously. If there can possibly be a passive state of the vital forces, as strangely recognised by Bichat, (2) allowing the mechanical transudation of the fluids, it should certainly be exemplified in the strongest case which M. Andral could bring to the question. But even there we have demonstrated, as we think, that the whole process is determined by an activity of the vital forces, and that there is no such thing, as surmised by Bichat, as "an annihilation of the organic sensibility" (3) in the living states of the vessels.

The foregoing experiment appears to be particularly calculated to illustrate the principles which relate both to the influence of bloodletting, and to preternatural effusions. As to the absence of vascularity in the natural cases of dropsy, it is equally a vital phenomenon as the inflammation on which the effusion depended; the vascularity having been removed by this depleting process of nature. If the effusion still go on, it is still the result of inflammatory action combined with the force of habit in the secreting vessels. Still there may be no remaining vascularity, for we shall endeavour to show, in another place, that an injection of the capillaries is not necessary to the process of inflammation. It is admitted by all, that in many cases of dropsy, the facts show a vital process, and that this is inflammatory. If they be less obvious in others, let us seek other proof. We have shown it in part, and shall briefly resume the subject again.

(1) See Magendie's Notes to Bichat, Sur la Vie, &c. p. 263.

(2) General Anatomy, vol. ii. p. 79, &c.

(2) Anat. Gén. t. 2. p. 564. "Dans celles où *la sensibilité organique anéantie* semble permitter une simple transudation à travers les exhalans."

For ourselves, we would be content with the analogies that exist in the healthy and morbid phenomena. But the question is important, for upon it depends the weal or the wo of the patient. We except from the foregoing remarks those effusions of serum which depend upon obstructions in the lymphatic system. But we shall endeavour, at all times, to support the doctrine of those physiologists who maintain that the living body has no inorganic pores, and that all the fluid products are the result of vital action with which the laws of physics and chemistry have little or no connection. (1)

SECTION V.

We have seen that the capillary arteries and extreme vessels sometimes contract powerfully from slight causes, and again very large abstractions of blood are necessary to produce this effect. Why is this so? It appears to depend upon several causes, an examination of which may be practically useful, and serve to illustrate our theory.

In the first place, why is syncope induced with so much difficulty, and why, in such cases, do the capillary arteries refuse to contract? This may depend, mainly, on the condition of the cerebral influence both on the heart and extreme vessels. This influence may be the result of inflammation of the brain; or it may arise from peculiar irritations of that organ, as we see sometimes exemplified in hydrophobia, and some other affections in which the nervous system is especially concerned. But we sometimes meet with a similar persistance of action in the sanguiferous organs where other parts are the seat of disease, and this will be commonly found to be of an inflammatory nature. In all these cases, we suppose that the contraction of the general capillary system, and therefore the failure of the heart's action,

(1) See Lupi's Nova per poros inorganicos secretionum refutata, t. 2. 1793. Kreysig's Specimen Secundum, de Secretionibus. Foquet, sur la Sécrétion, Diction. Encyclop. Bichat's Anat. Gén. t. 2 and 4.

is prevented, in part, by the sympathetic influence which is exerted over them by the diseased vessels, according to a well known law, by which parts of the same organization and function are disposed to sympathize with each other, especially when they exist in continuity. (1) There are important exceptions, however, in which the tolerance of the loss of blood is sustained by peculiar and unknown conditions of the cerebral and ganglionic systems.

The nervous influence, as we shall state more particularly hereafter, is, doubtless, concerned also in the various phenomena of disease. Perhaps it often performs a more important part than we have yet ascribed to it; however the circulatory or other organs may be independent of its influence in their healthy actions. We believe its influence is exercised, more or less, in the pathology of inflammation, especially over the general circulatory system; and we think there is sufficient proof that this influence is of an exciting nature. It will, therefore, contribute in preventing a general contraction of the capillary system, as well as of the immediate instruments of disease. Sooner or later, however, this influence will be overcome by the loss of blood, and then will follow the depressing effect in proportion to the increasing influence of the loss upon the brain. To this general principle, however, there are exceptions, as will be more fully stated hereafter. These exceptions consist particularly in the several varieties of venous inflammation, as phlebitis, purpura hemorrhagica, erysipelas, and venous congestion; though even here the modification of the principle will be various, according to the countervailing influence of a particular cause. In these cases, a depressing effect may be exerted on the nervous influence, as will be farther explained, partly by the nature of venous inflammation, and often by some pre-existing disease of other parts, or by a co-existing constitutional affection. (2) Conse-

(1) The power of any local inflammatory action over the whole capillary system is seen, also, in the resistance which it opposes to the constitutional effect of mercury, it being a familiar fact, that ptyalism often comes on only after the decline of inflammations, that bloodletting, cathartics, (a) &c., promote its accession.

(2) This is well exemplified in "the brain fever of drunkenness," where there are commonly strong marks of venous congestion of the brain. In some cases, as is well known, bloodletting is injurious, and quickly induces syncope, whilst in others it may be advantageously employed, and to a considerable extent. These cases have ap-

(a) See Fordyce on Fever.

quently, in part, there is a manifest difference in the effects of venous inflammation and that of other tissues on the general organs of circulation. In one case, they may be depressed, or little excited, whilst in the other, the whole vascular system may be thrown into tumultuous action. The agency of the brain, as will be seen, is more concerned in the phenomena of the former than of the latter affection, in which there is a more direct sympathy of the organs of circulation with the immediate instruments of disease. Therefore it is, that in the latter case, the general capillary system is especially maintained, under the influence of bloodletting, in a state of excitement by the direct influence of the local disease.

There is another condition of inflammation, now and then occurring, in which, more than all other varieties, the foregoing principles as to the cerebral influence, and the sympathetic excitement of the capillary system, are modified. This is the variety which is brought on by the excessive loss of blood, and which has been often confounded with simple irritation.

The persistence of the extreme vessels and capillary system in refusing to contract, when not promoted by other causes than loss of blood, will depend very much upon the activity of disease, and the extent and specific manner in which the instruments of morbid action may be involved. In common pneumonia, therefore, it is often necessary to carry bloodletting to a great extent before syncope ensues. But when pneumonia occurs epidemically, and the vital forces are specifically altered by the remote causes, and the whole constitution more or less invaded by their influence, as in instances related by Cleghorn,(1) Boyd,(2) and others, the necessary loss of blood may be great beyond anything required for the cure of common pneumonia. Hence, also, the reason that bloodletting may be carried far in acute anasarca. "I think it important to mention," says Dr. Hall, "in a very especial manner, that in some forms of acute anasarca, there is a great tolerance of the loss of blood."(3) A

peared to us to be sufficiently contradistinguished from each other by the phenomena appertaining to each, without resorting to the test of bloodletting, and it has seemed probable that the differences have been greatly constituted by the different modifications of the cerebral powers, to which a variety of causes, external and internal, have been accessory.

(1) On the Epidemical Diseases of Minorca, c. 6, p. 164—168.

(2) Thesis Inaug. de Febre Minorcæ. 1817.

(3) On Loss of Blood, &c. p. 123.

similar statement is made by Dr. Crampton(1) and others. We have found the same fact to exist in many cases of ascites, a disease which we have treated with success, in several instances in its early stages, by general bloodletting, (2) leeching, and blisters. In these cases, however, there is no excessive action. But the extreme vessels engaged in the morbid process are involved over an extensive surface, and are not only strongly under the influence of diseased habit, but have established sympathetic influences throughout the system; so that the power by which they hold the general capillary system in subjection is owing more to the amount of the vessels immediately concerned, and to the other causes now stated, than to the intensity of the inflammation. If the inflammation be still lower, the contraction follows more speedily. Thence the observation of Dr. Hall manifestly relates to that stage of the disease in which the inflammation has not yet been reduced by the effusion. And thence the *rationale*, why, in its more advanced stages, the tolerance of loss of blood is greatly reduced; and why it sometimes happens that the present tolerance may be a test of the quantity of blood which ought to be abstracted.

Again, in other cases of inflammation of a specific nature, as in acute rheumatism, the vessels are in a peculiarly irritable state, and produce an excessive influence upon the whole sanguiferous system. Here the modified irritability may only yield to great losses of blood, especially if the chief dependence be placed upon this remedy. So, on the other hand, when inflammation is aggravated or induced by the excessive loss of blood, such is the combined nature of the exciting cause and its curative effects, that the modified irritability of the vessels may readily yield, at the moment, to a farther loss; yet the remedy will soon go on with its deleterious influence.

(1) Report on Dropsies, p. 259, et passim.

(2) "A man," says Dr. Wardrop, "was admitted into the Hospital of Surgery with dropsy, which he expected would be relieved by tapping. A few hours after his admission, he was seized with bleeding from the nose, and although his pulse was feeble and his skin cold, having already lost not less than two quarts of blood, I bled him at the arm, when the hemorrhage ceased. Next day, the pulse became firm and wiry, and I bled him again till he was faint. On the second day, he was bled a third time, and tapped. Fourteen days after his admission, he left the hospital completely relieved, and a few weeks afterwards I saw him in perfect health." (*a*)

(*a*) On Bloodletting, p. 39.

From these considerations, we perceive that too much has probably been ascribed to the nature of the organ inflamed, in explaining the tolerance of the loss of blood.

When, however, the brain becomes the seat of inflammation, much is undoubtedly owing to the peculiar influences exerted by this organ. Here the contraction of the capillary system is prevented by more complex causes than prevail in other cases. To understand them correctly, we must carry in mind the laws which may be deduced from Dr. Philip's experiments to determine the influence of this organ on the heart and capillary vessels, and those sympathies which arise from an equal influence that is exerted upon the stomach.

In cerebral inflammation, the capillaries of the brain are maintained in a state of excitement partly by the morbid irritation of the organ. Therefore are the corresponding capillaries of the body held in subjection not only by the law of sympathy, but probably a greater influence is often propagated upon them, and the heart also, by the nervous power of the brain. In inflammations of other parts, the capillaries of the brain contract the moment, at least, that this change begins in the instruments of disease; and the depressing nervous influence then becomes a powerful co-operating cause of the general contraction. But it is obvious, when the brain is the seat of inflammation, that this influence is obtained with greater difficulty. Before it can be established by the loss of blood, the contraction of its highly excited capillaries must be effected, and that opposite state of nervous influence which arises from their excitement must be first overcome. (1) We have already said that this influence may

(1) "The quantity of blood which may be abstracted in cerebral fever," says Dr. Jackson, "without compromising the safety of the patient's life, exceeds a measure, which, were my experience of the fact not clearly ascertained, I should not venture to put before the public. Sixty-four ounces, taken away at one time, may be considered a moderate bleeding in the more concentrated forms; ninety-six ounces have been taken on several occasions, and even one hundred and twelve in some. The practice, so formidable in appearance, implied no danger. It saved life by direct effect. It may, perhaps, be carried farther without compromising the patient's safety. It is the effect produced which constitutes the rule for judging the measure." (*a*)

In a perfectly authenticated statement, it appears that Dr. Audouin, during the prevalence of a phrenetic fever, bled the daughter of the Mayor of Beaumont twenty-one times, — almost always to syncope. "The symptoms were not calmed and dissipated till after the last bleeding." Another was bled to syncope thirteen times

(*a*) History and Cure of Febrile Diseases, vol. ii. p. 137.

be often most perfectly obtained and extensively propagated by long continued syncope. This will sometimes happen in most inflammations when syncope is prolonged after the loss of small quantities of blood. But in inflammation of the brain, syncope is rarely effected but by decisive practice. Its prolongation may then suddenly subvert the disease. Thus, a case is related by Dr. Wardrop, of a medical student, who had an attack of inflammation of the brain, and who was repeatedly bled both by venesection and leeches, without relief. "The pain became more intense, though his pulse was so natural in frequency, and apparently so feeble as to discourage his attendant from repeating the bleeding." He however again opened a vein, and faintness soon followed; but the patient not satisfied with the amount of blood lost, the ligature was soon afterwards replaced, "and a very large quantity of blood was removed before he again fainted, and the syncope was so complete, that he remained insensible upwards of five hours." His relief was complete, and he required no farther remedial means.

The following is another exemplification of the foregoing principles, — showing how the loss of blood may variously affect its operation, whilst it furnishes, as we think, a useful exemplification of bad practice in a most deservedly high quarter. Dr. Rush "visited Dr. Say in a malignant fever, attended with pleuritic symptoms, in consultation with Dr. Physick. An acute pain in his head followed six successive bleed-

within five days, and went out on the twelfth. Another in the same way fifteen times, — and so on. (a)

We have had occasion to carry this treatment into effect in several instances of cerebral inflammation, and with corresponding results. We have no doubt that they are often regarded as cases of "irritation from the loss of blood," and the patient abandoned to this hypothesis. We were not long ago consulted by two very able physicians as to the means of arresting a supposed "irritation" of this nature, in a case of phrenitis, where the physicians were apprehensive that they might have proceeded to an injurious extremity with the lancet. The quantity of blood taken was certainly very great; but as our remedy was the continued loss of blood, we were allowed, on taking the responsibility, to have our own way. We bled this patient afterwards, repeating the operation once or twice a day, till ten more bleedings were added to those which had been supposed to have produced the "irritation." The quantity which we sometimes took was twenty-four ounces; and it was not till near the last bleeding, that the symptoms gave way in any encouraging manner. The case certainly looked very much like some of those which are represented in the books as examples of "irritation from excessive loss of blood." The patient recovered.

(a) See Beddoes on Fever and Inflammation, p. 171.

ings. After a seventh bleeding, he had no pain. His fever soon after left him. They were *wholly guided by the force of the pulse.*"(1) In this case, where it is highly probable that two or three decisive bleedings would have staid the disease, moderate depletion, "guided by the force of the pulse," by adding *force* to the general circulation, developed immoderate action in the brain, and thus increased the cerebral influence, as well as the constitutional disease, in the same direct manner as it produced the cerebral disturbance. The system, therefore, probably sustained a much greater loss of blood than might have been necessary, or than might have been borne by more efficient practice at the onset. But the greatest loss to the patient accrued from the prolongation of disease.

What we have just said in regard to the development of an exciting influence of the brain by the abstraction of blood is sometimes exemplified in venous congestions of the organ. The cerebral powers, and the *vires vitæ* of the system, may be so modified by the depressing influence of this disease, as we shall endeavour to show, that only small abstractions of blood may be borne at first; but the force of the general circulation rising as the congestion is overcome, arterial action may be so increased in the brain, or actual inflammation of the organ set in, that very large quantities of blood may be afterwards abstracted. This principle, however, frequently fails in venous congestions of the brain, and, as we shall see, the congestion, at the onset, often determines the nervous influence in the same way, though to a less extent, as inflammations of other organs. But the depressing effect of venous congestion is especially manifested when it holds its seat in other parts, — and it is here that we shall see the tolerance of loss of blood remarkably increased by the primary loss.

Such are the peculiar relations of the brain to other parts of the system, it may happen that its counteracting influence over the effects of bloodletting, in its inflammatory diseases, may be variously modified by the progressive changes which affect its structure, or its powers and functions. Alterations may take place in these respects, which shall so greatly prostrate the powers of the circulatory system, and of organic life generally, that the loss of blood may speedily depress or extinguish them. Such is particularly liable to happen where any serious lesion

(1) Rush's Medical Inquiries, vol. 4, p. 297.

of the brain exists, and especially if accompanied by pressure. Much, however, may depend upon the part of the brain affected, and upon the amount of the pressure; but, at other times the nature of the modifying cause is shrouded in mystery. Thence the importance, in the treatment of apoplexy, of duly considering the extent of the nervous influence upon the vital forces of the system in each particular case. Such is often the violence of this influence upon the heart and other viscera of organic life, at the incipient stage of apoplexy, that abstractions of blood at the onset of the attack, or before the nervous influence shall have abated, will produce a contraction of the capillary system, and a farther increase of the nervous influence, that shall speedily arrest their functions. Instances have occured of instantaneous extinction of life, from apoplexy, where the brain had been for sometime the seat of disease. In many cases analogous to the foregoing, an opposite state of the nervous influence may exist till the moment of, and even after the apoplectic seizure, when large quantities of blood may be abstracted with safety and advantage.

"Dr. Physick drew ninety ounces by weight, from Dr. Dewees, in a sudden attack of the apoplectic state of fever, at one bleeding, and thereby restored him so speedily to health, that he was able to attend to his business in three days afterwards." (1)

So, also, in acute hydrocephalus, general bloodletting will be less and less borne, in proportion to the amount of pressure, as well as to the extent to which inflammation may have been overcome by the effusion. This ratio increases in chronic hydrocephalus, although the brain have become gradually accustomed to the pressure; but there is less inflammation, and therefore less of those influences which sustain the action of the circulatory organs.

Again, in certain diseases, where the cerebral and ganglionic systems appear to be much involved, but in an unknown manner, and where, perhaps, there may be no special marks of inflammation in any part of the body, vast quantities of blood may be lost without inducing syncope. In these cases, there is great nervous irritability. We have seen upwards of thirty ounces of

(1) Rush's Medical Inquiries, &c. vol. 4, p. 337.

We have stated some cases from Lancisi, and others, where many pounds of blood were advantageously lost by spontaneous hemorrhage from the nose, in threatened attacks of apoplexy.

blood taken from the arm of a man, in hydrophobia, after the radial artery had ceased to be felt, — the patient being all the while in an erect posture, and remaining to the last without any sense of faintness. (1) Similar cases are recorded by the East India physicians. Professor Trolliet mentions a case of this disease, where the patient was first bled to the extent of eighty ounces, and thirty-two ounces more were taken at two other bleedings, within twelve hours after the first.

On the other hand, we have the opposite state of the cerebral influence in some cases of mania, where, from its depressing effect, the condition of the system has been erroneously compared to that of debility. In these cases, bloodletting is imperfectly borne, — evidently from its strong impression upon the brain, by which the vital forces of the system become farther impaired. (2) In those cases, without, however, the necessary experience to decide, we should infer upon general principles, that leeching the head would be more or less advantageous. But where we have met with such instances, there has been a morbid state of the digestive organs, upon which the cerebral derangement had especially depended; and our treatment has consisted mainly of internal remedies, attention to food, and other means of a general nature. We cannot well reason upon the modified state of the cerebral powers in these cases. We see the effects and trace them to their sources; and these facts conduct us safely to the conclusions which we have indicated. The contrast betwixt the conditions of cerebral influence in the foregoing cases of mania and hydrophobia, in neither of which may there be any physical signs appertaining to the brain, is certainly very imposing. But we are mainly concerned about the phenomena, and these in all cases constitute our best guide. Still, we have reason to think, as will be more fully stated in another place, that there is more generally a vascular

(1) He was perfectly relieved for a few hours of all the distressing symptoms, and drank water freely. But the paroxysm returning, he was again bled largely, but without relief, and died in about 36 hours from the first bloodletting. The disease had existed about two days before this remedy was resorted to.

(2) "So nearly is maniacal excitement allied to *debility*," says Dr. E. Percival, "that I have often witnessed tremors, fainting, and cold perspiration, in *strong* subjects, on removing a few ounces of blood." (*a*) Nothing, perhaps, could show more plainly the misapprehension as to *debility*, than this account of the symptoms as occurring in "strong subjects," whilst it illustrates the physiological effects of bloodletting.

(*a*) Dublin Hospital Reports, vol. i. p. 128.

excitement of the brain in hydrophobia, than is commonly admitted; that there is either a state of venous congestion or of increased arterial action. Inflammations of other tissues are apt to be more or less present; but these will not explain the tolerance of loss of blood long after the radial artery becomes indistinguishable.

In certain constitutions, and under some of the peculiar conditions of which we have been speaking, syncope may be readily induced in acute inflammations of the brain, and the disease thus broken up by the loss of a very small quantity of blood. Thus, "when a patient had lost only an ounce of blood, from the shock of the operation, syncope came on and effectually removed the cerebral affection." (1) Here the violence of the mental shock was more instrumental in producing syncope and modifying the cerebral powers than the loss of blood. But these cases are rare, and are doubtless favoured by idiosyncrasies.

Analogous modifications will also arise from any peculiar manner in which the vital forces of other organs may happen to be affected; not only in specific inflammations, but from those shades of difference which attend common inflammation. Particular influences will be determined directly upon the whole system by these modifications, as well as indirectly through the medium of the brain, according to the manner in which that organ may sympathize with the diseased part, and the influences produced upon it by the direct sympathies of the system with the organ affected. Thence it will also appear, that much will depend upon the natural relations of other organs to the brain, and to other parts of the body. Other and more accidental causes, which we shall soon notice, may contribute to these results. They have all an important bearing upon the effects of bloodletting. Thus, it is often useless or hazardous to bleed a patient far advanced in typhoid pneumonia, in pleurisy supervening upon confirmed phthisis, or in pneumonia affecting the aged, if there be a depression of the vital powers. (1) So also, in some inflammatory affections complicated with certain organic lesions of the heart. Here, if syncope suddenly follow, the nervous influence may totally arrest the action of that organ. Again, where the *vires vitæ* are greatly altered and perniciously pros-

(1) Armstrong's Lectures, vol. 1. p. 402.

(1) In the two latter cases, leeching may be cautiously used with advantage.

trated. Ramazzani, for instance, speaks of an epidemic pleurisy, in which "multos *pleuriticos* certe scio inopinato perisse, postquam bis, vel ter secta fuisset vena." (1) In a bilious pleurisy which prevailed in Edinburgh, February 1736, it is stated that bloodletting was prejudicial. (2) Sydenham, even, is inclined to this opinion. (3) It will be seen, however, farther on, according to the experience of Cleghorn in Minorca, that whilst abstraction of blood in the worst forms of bilious pleurisy and bilious pneumonia was at least useless in small quantities, it was always successful when largely applied.

If the brain be capable of exerting an important influence upon the heart and arteries, and other viscera, under circumstances of health, when its organic functions are moderately disturbed, as shown by the experiments of Philip and others, and as inferred from the common phenomena of life, how likely is it that this influence will be variously modified by its diseases, and that there will arise corresponding differences in respect to bloodletting; considering the influence of this remedy upon the cerebral powers. The nervous influence, we believe, ~~with many~~ of the illustrious men ~~we shall have~~ mentioned, to be more or less developed, also, by inflammations of every part of the body. (4)

Thence is farther seen how syncope, with exceptions like those which we have stated, is less easily induced in inflammatory affections than in other diseases; since the nervous influence not only more powerfully controls the capillaries which are the instruments of the inflammatory action, but exerts the same general effect over the whole vascular system.

From these, and former considerations, we may understand, also, why inflammation is sometimes remarkably resisting under the influence of bloodletting, and the fallacy of that rule which aims at syncope with the least expense of blood, and which would make it a test by which bloodletting should be limited. The nervous influence of the brain upon the heart and stomach, and the capillary vessels, may be often obtained in very grave inflammations by the sudden abstraction of very small quantities of blood; and syncope may follow mainly from these influences,

(1) In Constit. Epid. Urb. a. 16.
(2) Edin. Med. Essays, vol. 5. p. 32.
(3) Works, s. 5. c. 5.
(4) See Traver's Farther Inquiry concerning Constitutional Irritation, p. 139.

or may depend chiefly upon the gastric irritation. But this is a very different result from that which springs from the direct impression of the remedy upon the vital forces of the extreme capillary system. Thence, as in syncope from moral causes, the action of the vessels concerned in the morbid process, and that of the whole sanguiferous system, will be generally re-established as soon as the nervous influence ceases; and sometimes, as we have seen, in greater force.

The discredit which has been brought upon bloodletting in the treatment of pneumonia, and some other inflammations, and congestions, has arisen, in part, from neglecting a proper consideration of its physiological effects,—and so far as experience is concerned, from an imperfect application, or from a delay, of the remedy.

It should not be forgotten that, in inflammatory affections, and venous congestions, the apparent debility may be scarcely more than a temporary prostration of muscular strength, or at most, a depression of the powers of life by the existing burthen of the disease; nor, that small abstractions of blood may add force to the circulation, or may so modify the condition of the instruments of disease, that their vital forces shall be excited to a higher degree of morbid action.

How does it happen that spontaneous hemorrhages may take place, with safety to life, to such an extent, that, in the language of Lommius, "should any one attempt, in imitation of such evacuations, to take away a like quantity of blood from the very same persons, by opening a larger vessel in the arm, he would, undoubtedly, a syncope arising, take away the strength and life of his patient, together with the disease." (1) Cases are on record where the evacuation has been so great as to give to the remaining blood the appearance of serum, without greatly impairing the strength of the patient.

In these cases, the effect upon the circulatory organs is wholly less than from the abstraction of blood by leeches. Much is owing to the general influence of the inflammatory action on which the evacuation depends. But common inflammation, in itself, would be inadequate to the effect. We cannot, however, offer any other explanation, than that the remedy being instituted by nature for the removal of disease, and the parts concerned in

(1) On Continued Fevers, p. 26.

the morbid action being made the instruments of relief, it is reasonable to suppose that nature has established some peculiar modification of action upon which spontaneous hemorrhage depends, that resists the usual effects of the loss of blood. We see this principle operating with various effect in inflammations, according as they are modified by remote causes, and according to the activity and extent of disease, the organ, or organs affected, the general state of the system independently of the local influences, &c. We have also remarked that where spontaneous hemorrhages have been large, the subjects have simultaneously borne venesection and leeching to a large extent. This is likewise a proof to our mind, that spontaneous hemorrhages generally depend upon morbid action, and not upon a rupture of the capillary blood-vessels.

SECTION VI.

One of the finest illustrations of the direct effect of bloodletting upon the vital properties of the extreme vessels of the circulatory system, and of the instrumentality of the brain in promoting the effect, is shown by the changes which take place in the blood whilst flowing from the arm in inflammatory diseases.

These changes may be sometimes induced by the loss of a quantity of blood so small as to leave no doubt of the manner in which they are effected. Such an instance has just occurred in our practice. The patient labours under an inflammation, which has supervened after convalescence from a fever of considerable duration. She was placed in an erect posture, and three ounces of blood were drawn into three wine-glasses, when syncope took place. In the first wine-glass the blood had a strong, thick buff, depressed, and with a fimbriated edge; in the second, the buff was sensibly less, and the other characters more diminished; in the third, they had all disappeared. The blood was received in a full stream into each glass.

If syncope take place, the blood not only generally loses its inflammatory characteristics, as in the foregoing case; but, ac-

cording to Gendrin, the clot is then often much softer, and more voluminous. (1)

Blood, "drawn from a person about to faint, coagulates very rapidly." (2) This we have constantly witnessed in bleeding animals to death. The rapidity of coagulation appears to bear an exact ratio to the loss of the vital powers of the solids. But, again, when death is suddenly produced through the nervous system, as by blows on the stomach, apoplexy, &c., or by running, lightning, organic affections of the heart, &c., or when the powers of life are greatly reduced by malignant fevers, the blood remains fluid. These are paradoxes, which can only be resolved by supposing peculiar influences of the solids according to the specific modifications of their vital forces.

Again, should the inflammatory symptoms return, after the patient recovers from a state of syncope, the blood will be found to have re-assumed its inflammatory characteristics. And here we may remark, that the principle upon which bloodletting operates, in the treatment of inflammation, is well shown by its exactly opposite effect in producing irritation, and sometimes

(1) Hist. des Inflam. t. 2. s. 1413. See ibid. p. 430—462, for various changes induced in the blood by the operation of bloodletting, and by accidental circumstances that may attend it.

(2) Scudamore's Essay on the Blood, p. 40. Also, Wilson on the Blood, p. 53. Thackrah on the Nature and Properties of the Blood, p. 48. Gendrin, Piorry, &c.

So far as experiments go, which are made upon healthy animals, they appear to sustain, especially those of Thackrah, the conclusion of Hewson that the blood which is first drawn in inflammation, coagulates most slowly. That it is generally so, we think is well settled by experience, though it is still controverted. It is reasonable to suppose, however, from the changes that are induced in the appearances of the blood by the size of the orifice, the shape of the cup, &c. that some of the varieties of inflammation may sometimes exert such a modifying effect as to reverse the foregoing rule, especially if idiopathic fever co-exist. This consideration, strengthened as it is by the observations of some, show us how unsafe it may be to reason from experiments on animals in health to such as are the subjects of disease, — since Thackrah grounded upon the supposed analogy his conclusion that "the speedy occurrence of concretion, on the effusion of blood, affords a reason sufficiently urgent for the discontinuance of depleting measures." (a) Nor can we neglect this opportunity, among the many we shall embrace, of objecting to the assumption of a particular symptom, — especially when it may be only a contingent result, — for regulating the application of great remedial agents. It is opposed to philosophy, and to that practical observation on which philosophy is founded.

Suppose that others are right in their statement, that the first drawn blood in inflammation coagulates most rapidly; bloodletting then, according to Thackrah's rule, should be avoided.

(a) On the Nature and Properties of the Blood, p. 90.

local inflammations,(1) when carried to excess; as it is, also, in establishing the vascularity of inflammation in the membranous tissues, when animals are bled to death, according to the experiments of Mr. Seeds.

We shall resume the consideration of this subject when we treat of the humoral pathology.

SECTION VII.

If our theory of bloodletting be correct, it should bear the general test of observation. We have already submitted many facts for the purpose of illustration,—especially such as are connected with inflammatory action. But it should explain the phenomena that may arise from blood-letting under all circumstances of disease; unless, where it may appear to be inadequate, or to be contradicted, it be shown that causes exist to modify the phenomena, whilst the principles remain the same.

We have seen that in a state of health, a contraction of the capillary vessels, and syncope, may be readily induced. When mental emotions produce those results, we have seen that their influence is directly propagated upon the heart and capillary vessels; but that the heart is most powerfully influenced, and that the contraction of the capillaries is more from sympathy with the heart and the failure of the heart's action,—being the reverse of the changes, in those respects, that follow the loss of blood; at least in the incipient stage of bloodletting. Whatever change, however, may be made in the vital powers of the capillaries, it cannot be maintained without the continued operation, for a certain time, of a diminished supply of blood to the capillary system; but, more especially by that impression which is made upon the *vires vitæ* by the remedial agent.

There are diseases in which bloodletting is indispensably important, where the loss of very small quantities of blood is followed by syncope. This may be chiefly owing, as often happens, to some morbid irritability of the stomach or intestines. In these cases, the heart fails in its action in a way somewhat cor-

(1) See a following section on this subject.

responding with its prostration by mental emotions. The causes, however, are more complex. The loss of blood has contributed something according to its influences as already expressed. But a powerful sympathy is reflected from the stomach upon the brain, by which its nervous influence is strongly determined upon the heart and capillary vessels, whilst the heart itself and capillary systems undergo a direct influence from the stomach. If inflammation exist, the foregoing impressions counterbalance the opposite influence of the inflamed part. This principle is applicable to other cases in which analogous causes modify the operation of bloodletting, and which are likely to embarrass the practitioner as to the extent of the influence which the remedy has produced. Posture alone, and especially emotions of mind, affect the results, though not the principles.

But let us examine the operation of loss of blood in venous congestion, where syncope is often easily induced by this remedy independently of other causes. Here, too, the loss of a small quantity of blood may be sufficient to maintain the restoration of the general circulation where it has been depressed, though it do not remove the local congestion. As manifesting another disparity, also, with what we have seen of bloodletting in inflammations, this disease, as we shall endeavour to show, is constituted by inflammation of the coats of the veins. But if this mode of action exist, it is in a low state, and the instruments concerned, which are the vasa vasorum, are too limited in amount to exert those exciting influences upon the vascular system which we have seen to attend some cases of sub-acute inflammation, —as in anasarca.

But the foregoing are far from being the principal causes why the same constitutional excitement does not prevail as in common inflammation. Venous congestion, indeed, is often distinguished by a perfectly opposite effect. We shall therefore find the cause which we are seeking in the constitutional nature of the venous tissue. It might not be easy to demonstrate this but by the extended process which we have attempted in another place, did not unequivocal examples of venous inflammation supply the proof. In acute phlebitis, especially when spontaneous, or induced by insensible causes, the capillary vessels at large may be in a contracted state, and the general circulatory organs prostrated. General bloodletting, in this condition, is illy borne, and a few ounces will induce syncope. The cause of this early

phenomenon will be soon explained, when we consider it in relation to venous congestions. We may now say, however, that in these cases of spontaneous phlebitis, when there is no general excitement and bloodletting is imperfectly borne, there is a pernicious affection of the *vires vitæ* of the system, produced by remote causes, as well as by the influence of venous inflammation. But when the forces of life are differently modified, as in traumatic phlebitis, or in cases that are induced by injections of an irritating nature, there occurs an excitement of the organs of circulation somewhat analogous to that from the influences of common inflammation. Still, it is not the same; but resembles more the reacting stage of typhus fever. Even in these cases, however, there is often a prostration of the circulatory organs, like that which frequently attends uterine phlebitis, and more frequently venous congestion.

Again, in chronic cases of venous congestion, we frequently see the tendency of local inflammation to excite the action of the heart and arteries. Not that this excitement is often extended to the capillary system; for it is mainly limited to the heart and larger vessels, although the capillaries are not contracted. It is especially discerned in a quickness and hardness of the radial artery, which is frequently, too, more voluminous than natural, though it may be again somewhat contracted. But in most cases of venous congestion, whether the pulse be full, contracted, slow, frequent, or quick, irregular, &c. or whatever the state of the capillaries, there is an attendant hardness; and if the blood be drawn with proper care, it will generally present a buffy coat. (1)

In the cases of chronic congestion, however, which we are considering, if there be a sudden increase of the morbid state of the veins, as frequently happens, all the phenomena of acute congestion may immediately supervene. The skin becomes cold, and comparatively bloodless, the countenance sinks, the pulse diminishes in volume, and the heart is faint in its efforts.

It also frequently happens in venous congestion, especially when acute, that the arteries, leading to the parts, pulsate with

(1) It is remarkable that in the foregoing cases of chronic congestion of the veins, whilst the entire capillary circulation appears to be free, and there is no symptom of pulmonary embarrassment, the blood is generally morbidly dark, — no matter in what organ the congestion is seated. It is more so, however, when the liver is the seat, than when the brain, or any other organ excepting the lungs.

the same violence as in cases of common inflammation; and this even when the action of the heart and capillary system is prostrated.

In all the foregoing cases, however, the disposition of inflammatory action to involve the system in a state of excitement is more or less counteracted by the peculiar constitution of the venous tissue, the tendency of which, when inflamed, is rather to depress the *vires vitæ* and with them the circulatory organs. Whenever this depression occurs to a considerable extent, there is a contraction, more or less, of the capillary blood-vessels, and a consequent recession of blood from the circumference to the centre. The heart being now also mechanically oppressed, the phenomena of prostration, and a farther tendency of the capillaries to contract, are promoted by this mechanical condition.

We suppose, also, that there is generally a tendency of venous congestion to pass into common inflammation, and consequently a predisposition in the capillary arteries, which communicate with the congested veins, to assume that mode of action. But this tendency to inflammation will oppose little or no obstacle to the general contraction of the capillaries, under the influence of bloodletting; since no excitement follows a state of predisposition, and remedies act with the greatest power in the forming stage of disease. In that stage of venous congestion, too, especially in its acute forms, when bloodletting is commonly practised, the capillary blood-vessels are generally in a contracted state; and therefore, thus predisposed, the loss of blood sooner reduces them to that state of contraction which is followed by syncope. It will be readily seen that the foregoing condition is co-extensive with all parts; so that the vascular change, under such circumstances, is early and powerfully felt by the brain, and the cerebral influence is early developed. The vital organs, too, with which the heart so readily sympathizes, although they be not the seat of the affection, suffer in their powers, and maintain a depressing influence on the heart and capillary system. Blood is accumulated about the centre of the circulation, and the moment that a greater contraction begins in the capillaries from loss of blood, that accumulation is increased, and the heart, already oppressed and now suddenly overpowered, beats only with faint pulsations. From the profound alteration, too, of the vital forces, especially of the congested organ, and the smallness of the quantity of blood abstracted, we may under-

stand why it is often necessary to repeat the impression from the loss of blood, to overcome the local affection, although syncope may follow each repetition of the remedy. It is also probable that the *vasa vasorum* are less controlled by the nervous influence, than the greater system of arterial capillaries. There is also reason to believe, that they experience less of that sympathetic influence which is imparted to the instruments of action in common inflammation by the general change in the vascular system which follows the loss of blood.

In uterine phlebitis, syncope often supervenes, also, on the loss of small quantities of blood. This is equally owing to the causes just stated; to the depressing influence of venous inflammation, to the sympathies determined by the affected organ, and to the consequent contraction of the capillary vessels, by which the blood is determined about the great internal viscera. This recession of blood from the surface is well denoted by the frequently haggard and collapsed state of the countenance; (1) a coincidence, by the way, which often attends severe forms of venous congestion.

It is owing in part, also, to the accumulation of blood about the right cavities of the heart, that some of the most sensible phenomena of inflammation are so much modified in severe cases of venous congestion. This observation applies not only to the congested veins, but to inflammations of other tissues, that may co-exist; the latter being also influenced, locally and constitutionally, by any depressing tendency of the former.

But the foregoing peculiarities of venous congestion are more rarely manifested when the brain is the seat of this affection. We have seen, that under certain circumstances, the tendency of common inflammation to excite the heart and arteries is displayed by venous congestion, especially when of a chronic nature, whatever organ may be its seat. It is probable, therefore, from what has been said of the effect of cerebral inflammation, in powerfully maintaining the action of the circulatory organs, that corresponding influences will often attend cerebral congestions. This induction is confirmed by experience; since we frequently find the same phenomena attending abstractions of blood in venous congestions of the brain, as when it is the seat of common inflammation. Besides the general effect of an

(1) See Lee, on some of the most important Diseases of Females, pp. 62, 75, 78, 86, &c. Also, other authors.

exciting nervous influence, the capillaries of the brain contract less readily than when the affection is seated in other parts, and a greater loss of blood is sustained without syncope. All this is abunbantly shown in cases where apoplexy is impending, and where the morbid condition of the brain is almost always constituted by venous congestion. "In *congestion*, or adhesive inflammation of the brain," says R. Jackson, "the quantity of blood abstracted at any one time was rarely less than 48 ounces, frequently 64 or 80, sometimes 96 or 112 ounces." (1)

But it sometimes happens, that, either from the pressure of the accumulated blood upon the brain, or from some unknown influence attending cerebral congestion, a totally opposite state of nervous influence suddenly supervenes, and the whole circulatory system is in a moment prostrated. This is seen in certain cases of apoplexy, and sometimes in the cerebral congestions of infants, without effusion; and, as we shall endeavour to show in another place, if the lancet be employed in these cases before the *vires vitæ* have recovered in a measure from the shock, it will be at the expense of increasing the evil. We need not add, that in the cases we are now considering, dissections have disclosed nothing as it respects the blood-vessels of the brain but turgescence of the cerebral veins, and in many instances there is no other morbid appearance. (2) These are cases, too, as we have seen, where the lancet might have been employed to a great extent before the supervention of the prostrating paroxysm. (3)

Again, complex cases of venous congestion of the brain not unfrequently arise from highly morbific causes, in which the usual exciting influence of venous congestion of the brain is counteracted by a profoundly depressing effect of venous congestion affecting other organs. We shall more distinctly set forth our views upon this subject, when we speak of the philosophy of venous congestion. But, we may now say, that in

(1) History and Cure of Febrile Diseases, vol. i. pp. 126, 223, 226. Vol. 2. p. 137. We need not say that Dr. Jackson means by congestion, venous congestion.

(2) As to common inflammation of the brain, we believe it is never, *per se*, a cause of apoplexy.

(3) "When the approach of apoplexy or hemiplegia is foreseen," says Dr. Wardrop, "it is astonishing to what an extent bloodletting must sometimes be carried, in order to check the progress of the malady." (a) We shall have occasion to speak, for another purpose, of some extraordinary instances.

(a) On Bloodletting, p. 66.

these cases, there appears to have been a great amount of abdominal venous congestion, of which the cerebral congestion is more or less a sympathetic result. The vital forces are so greatly prostrated by the cause already assigned, and, perhaps, by the sedative nature of the blood, which has now assumed universally a venous character, as, also, by an accumulation of blood about the right cavities of the heart, that the system may bear, at first, an abstraction of no more than four or eight ounces of blood.

Four cases of this nature lately occurred under our observation in one family at Harlem. Two of them were unusually malignant. The explosion of disease, which was rather abrupt, had been preceded by a slow accession of congestion of the abdominal viscera, especially of the liver, which had passed on without any particular attempt at relief. The most remarkable symptoms attending the full development of disease related to the brain. There was most excruciating headache, great sensibility to the light, intolerance of noise, cold skin, throbbing of the carotids, whilst the radial pulse was small, rapid, and easily compressed, but remarkably hard if a slight pressure only were applied. Two of these patients constantly fainted from the loss of six to eight ounces of blood. These two we saw in consultation with Dr. Peck. Both of them soon became affected with delirium; one also, with hemiplegia, and the other with convulsions. Bloodletting was repeatedly pushed as far as possible; but the operation was constantly arrested by the loss of six or eight ounces. The patients, however, always derived a sensible relief from the remedy, but ultimately perished from the inability of the system to bear the necessary loss. Other remedies, such as mercurial cathartics, &c. were, of course, associated in the treatment.

The other two cases were exactly similar in their pathological character, only the amount of disease was less. The subjects were relatives of the deceased. They removed to New-York immediately after the fatal issue of the former cases, and were at once taken ill. We bled them without delay, but syncope followed the loss of five or six ounces. The operation was frequently repeated, in quick succession, and every new attempt was more successful than the last, till finally the disease was conquered in these two cases.

Dr. Wardrop generally prefers leeching and cupping in the treatment of venous congestion.

But he says, "there are cases in which bloodletting must be carried to a great extent, more particularly where the brain or chest is the seat of disease."(1) And the only case by which he illustrates this practice, and the nature of venous congestion, is that of a gentleman who suffered an attack of it in his brain, — when fifty-two ounces of blood were taken at one bleeding.

For reasons which we have already assigned, as well as from experience, we cannot but object to the preference which is given by Dr. Wardrop, as a general rule, to leeching and cupping, in the treatment of venous congestions, especially if the local congestion be accompanied by idiopathic fever. We shall see, too, when we speak more particularly of the practical application of bloodletting, that a preference has been actually given to dry cupping in those formidable cases of pneumonia typhoides, where venous congestion is rendered most malignant by the predisposing causes, and by its complication with a bad form of arterial inflammation,(2) besides the probable existence of an idiopathic fever.

In the cold, or formative stage of intermittent fever, we have an instance in which the vital forces of the capillary blood-vessels are so impressed that their universal contraction is the consequence. There being, also, none of the attendant circumstances of inflammation to resist the farther contraction of the capillary system, and blood being already accumulated about the heart, syncope is often induced by the loss of moderate quantities of blood. This result, however, may be promoted by other

(1) On Bloodletting, p. 65.

(2) When we speak of *arterial inflammation*, we mean only to employ the term to contradistinguish what is usually called inflammation from venous congestion; not to imply an hypothesis that the extreme arterial capillaries are themselves inflamed. For the same reason, we also speak of *common inflammation;* and in such cases of contradistinction we embrace all the varieties of that disease. We are opposed to every thing like mere hypothesis, — as, for instance, to all the attempts which have hitherto been made to explain the nature of the alteration of the vital forces and actions in idiopathic fevers, and to the assumption that there is a stagnation of blood in the capillaries during the exacerbation of fevers. In inflammation, on the contrary, there has been some show of proof that the blood is more or less stagnant in the instruments of disease; and the hypothesis, therefore, is entirely legitimate in this case, till it be shown, as we shall endeavour to do, that the facts are inadequate, and are contradicted by others more substantial.

obvious causes. But the morbid contraction depending on a different cause, and the vital powers being differently affected from what bloodletting produces, although favouring the effect of that remedy, it often happens that as great a loss of blood will be sustained as in health, before that change can be established in the powers of the capillaries which is necessary to perfect the contraction that is incident to bloodletting. The natural contraction which so often affects the capillary system in venous congestion, when unattended by idiopathic fever, is more the result of sympathy with the affected venous tissue; but in idiopathic fever it is a primary derangement, and the organic forces are differently and more profoundly altered.

If we mark the succession of phenomena as they result from bloodletting in intermittent fever, it will be apparent that the primary and great impression of the remedy is exerted on the capillary vessels. Blood is already, in the cold stage, accumulated about the heart, and it is erroneous, therefore, to suppose that a greater failure of its action is produced by diminishing the volume of blood in the cavities of the organ. On the contrary, as we have already seen, the quantity accumulates, at first, about the heart, and its augmentation is at least a concurring cause of syncope. (1) Again, if we carry the abstraction to the point of approaching syncope; the usual succeeding stage of the paroxysm may be either prevented or greatly modified. But if we stop short of this effect, the hot stage will generally follow. And how has this arisen from the loss of a small quantity of blood, when no amount, perhaps, would have arrested the disease, if taken at any other period? It is also manifest, that the quantities to be abstracted with success will depend, in part, upon the precise period of the cold stage,—whether at its beginning, or near its termination in the hot stage. Less, *cæteris paribus*, will be necessary in the former than in the latter in-

(1) It should be here stated, as will appear more fully in our Essay on Venous Congestion, that the accumulation of blood which takes place in the venous system during the cold stage of an intermittent, or other fever, is very different in respect to its cause, from that which constitutes what we denominate congestion of the veins; and that bloodletting, in affording relief in these affections, operates on different principles.

Some light may be gathered upon this subject from the experiments of Dr. Reid, in the Edinburgh Medical and Surgical Journal. April, 1836, p. 387.

stance; (1) and so the result will be influenced by the application of the remedy during the first paroxysm, or by its delay till a later; and this often in proportion to the delay. Diseases generally yield most readily in their forming stage; (2) but in intermittent fever, the disease may be said to be renewed, in a measure, at each paroxysm. Like other affections, however, it acquires ob-

(1) "It is difficult," says Dr. Mackintosh, "to determine the quantity of blood it will be necessary to draw in any given case. Sometimes it requires twenty-four ounces: I have known three ounces suffice, and in one case, an ounce and a half produced the full effect. I was once successful by bleeding in a cold stage that had lasted twenty-six hours." (*a*)

Mr. Twining limits the amount of blood to be taken from an European to twenty ounces, and twelve ounces from a native, and the patient should be bled in a horizontal posture, to enable these quantities to be abstracted. (*b*)

Many eminent practitioners have borne their testimony to the power of bloodletting in arresting intermittent fever, when employed in the cold stage. Besides the foregoing, are Haviland, Malden, Kelly, Mackenzie, Buller, Dempster, Bucleau, Alison, Cambridge, Stokes, Griffiths, &c.

Dr. Stokes applied this treatment with various success in one hundred cases, during an epidemic. (*c*)

It has obtained some favour in America, though we believe it has been tried by a few only. Dr. Higginson (*d*) relates thirteen, and Dr. Henry (*e*) four cases where it was successful. Dr. Rush advocated the practice. "Bleeding," he says, "is certainly safe during the coldness of the body which takes place in violent fevers. It generally removes it." (*f*) Robert Jackson, and other British army-surgeons, practised the method with various success. (*g*)

Dr. Mackintosh says, "he believes that bleeding in the cold stage, conjoined with the subsequent use of the sulphate of quinine, and laxatives, to be as certain a cure for intermittents, as any other set of remedies can be said to be cures for any other class of diseases." (*h*)

This practise is commonly supposed to be of recent origin; but it was employed by Morton, Pringle, Priens, Coliny, and their cotemporaries, — some giving it their sanction, and others condemning it. Senac speaks of it as "unreasonable and hazardous." (*i*)

(2) We see it stated by Dr. Fuller in his excellent Prize Dissertation on *Scarlatina Anginosa*, that "a much less quantity of blood taken during the chill will suffice to moderate the reaction, than would control it after it has already taken place; and here a grain of prevention is worth a pound of cure." (*k*)

(*a*) Elements of Pathology and the Practice of Physick, vol. 1. p. 64.

(*b*) Clinical Illustrations of the more Important Diseases of Bengal, &c. vol. ii.

(*c*) Theory and Practice of Medicine, p. 358.

(*d*) Transylvania Journal, 1837.

(*e*) Western Journal of Med. and Phys. Sciences, 1837.

(*f*) Note to Cleghorn's Diseases of Minorca, ch. 3. p. 116. 1809. And Rush's Works.

(*g*) Jackson on Fever, &c.

(*h*) Op. Cit. vol. ii. p. 63.

(*i*) On Fevers, p. 195.

(*k*) Boston Med. and Surg. Journal, vol. 19. p. 247.

stinacy from the force of habit, (1) and from the influence of local inflammations and venous congestions, which often spring up in its progress.

That bloodletting should suddenly arrest the progress of intermittent fever, when applied in the cold stage, would seem probable from what we have said of the principles upon which this remedy operates. The capillaries, being in a state of universal contraction from disease, if bloodletting have its special influence upon the vital forces of those vessels, it would be the probable effect of such a cause, which should suddenly, and greatly, and as universally increase that contraction, though by inducing a different condition of the vital forces, (2) so to modify the morbid state, as to interrupt the succession of the hot stage, which depends on a specific condition of the capillary vessels in the cold stage. In accounting for the decisive effect of the remedy, all that we have said of the co-operation of the nervous power with the direct impression of the loss of blood upon the capillary system must be fully considered.

These considerations enable us to understand why bloodletting is more useful towards the subsidence of the stage of reaction than at its earlier periods. (3) Nature is now consummating her

(1) The power of habit is more or less broken at the end of each paroxysm, and there is a partial respite of morbid action. Still there is often a progressive increase of both at each succeeding paroxysm; and the strength of the former, as is well known, may reproduce the disease after an interval of many months, and sometimes years.

Mr. Twining says that the physician should endeavour to be present at the invasion of the cold stage. (*a*)

(2) The action of the vital powers, by which the contraction of the vessels is produced, is manifestly different in the two cases, from the essential difference in the nature of the causes.

(3) This fact was noticed in the early ages of the art. Celsus says, that "to take away any blood in the very height of a fever, is to kill the patient. Wherefore, if the fever be found to abate either in the evening, or in the night itself, and to rage most in the morning, that time is undoubtedly to be preferred to this." Avicenna was of this opinion. (*b*) "Whenever," says Lommius, "there appears the greatest remission of the fever, then is the most suitable time to open a vein."

This rule was restricted by Lommius to the intermittent fever, who continues to remark that, "the observation just made is useful in those fevers which prevail every third day, and likewise in continued quartans and quotidians, in which, since the paroxysm does not always fall on the same time of the day, we must vary the bleeding, and perform it at noon, often in the morning, and even in the very dead of the night." (*c*)

(*a*) Clinical Illustrations, vol. ii. And Calcutta Transactions, vol. v.

(*b*) L. 1. Fen. 4. c. 20.

(*c*) On Continued Fevers, p. 52.

efforts at relief. The capillary vessels are everywhere about to contract. Other important changes, however, attend the process; but this is one of the most remarkable, and the restoration of the secretions, the equalization of temperature, &c., are consequent on the change in the capillary arteries, of which their contraction is the most visible feature. Bloodletting will now exert the sensible effect which it produces on the capillary vessels in their more healthy condition. It accelerates the healthy process of contraction which nature was instituting; and this contraction, the result of a certain modification of the vital powers, or of their increasing tendency towards the normal state, thus artificially induced, will be followed by a more complete and speedy restoration of the healthy functions. At any other stage of the reaction, the same consequences would not ensue. The vital forces of the capillaries are then in a state directly opposite; and if, by larger abstractions of blood we effect the contraction of the capillaries, or, in other words, that alteration of the vital forces on which the contraction depends, so great is the tendency of those forces towards a morbid state, that the impression is comparatively powerless, and they are likely to resume their excited action. (1)

We may here notice some apparent contradictions, which are frequently presented by the phenomena from the operation of the loss of blood. We have stated that there is an early contraction of the capillary arteries, which is always the beginning of that series of changes which terminates in syncope. But it frequently happens when the capillaries are already in a state of contraction, that one of the earliest phenomena is an increasing force and fulness of the pulse, whilst the blood is discharged in a greater volume. Here, however, the capillary system, venous and arterial, is contracted from disease, and its expansion depends on the influence upon its *vires vitæ* of the moderate abstraction of blood. Still we have sufficient evidence that the primary effect of the loss is that, at least, of momentarily increasing the contraction. This is especially shown by the increasing slowness with which the blood flows from the arm for a certain time, however carefully the ligature may be adjusted. Dr. Mackintosh alludes to this phenomenon, and partially explains its cause. (2) But the pulse may soon rise, and the blood

(1) See Essay on Inflammation; Appendix regarding Fever.

(2) Op. Cit. vol. i. p. 110. "The blood," he says, "on other occasions, will

flow more and more freely; and if we now bind up the arm, the equilibrium of the circulation may be fully restored. If the morbid contraction of the capillaries have depended on venous congestion of some organ, the impression on those vessels will have been so slight, that the local affection, which we suppose to be sub-acute venous inflammation, may remain, although the general equilibrium of the circulation be re-established.

But it is not wise, as we think, to stop at the rising of the pulse, especially if the local congestion be complicated with idiopathic fever. On the contrary, going on with the operation, the artificial contraction of the capillaries soon returns, being promoted by their previous state. Again the blood flows slowly, and before much is abstracted, the patient falls into a state of syncope. A strong impression is now made upon the *vires vitæ*, and the local congestion, as well as its sympathetic results, may be perfectly subdued.

Whether, however, bloodletting may be useful or injurious, in the cases of which we have just spoken, will sometimes depend on the nature of the remote causes which have induced the morbid contraction of the capillaries. If it be the sympathetic result of local disease, and by moderate abstractions of blood we can overcome the contraction, we shall never fail of obtaining an important advantage, since we remove, by effecting this change in the capillaries, the obstruction on which, in part, the interrupted equilibrium of the general circulation depended.

But cases somewhat analogous are often occurring, where it might be fatal to the patient to abstract blood. The shock of a fall and certain apoplexies present examples where the vital forces are extensively prostrated, and the capillary circulation embarrassed as in the cold stage of fever; and these are instances in which the lancet has been often fatal. They are, however, as we shall show in our essay on congestion, in all essential respects, unlike the foregoing examples of disease. These are cases, too,

spring from the orifice in a large stream; but it suddenly stops before a table-spoonful is evacuated. Some think this is owing to an alteration in the position of the arm; others to the tightness or slackness of the bandage. But it must be recollected that the blood is moving slowly in the arteries, while the veins are gorged." It is evident, however, that this does not account for the "spring" of the blood "suddenly stopping before a table-spoonful is evacuated." The original state of the circulation remaining, the blood would still flow freely. But in most of these conditions, syncope follows the loss of small quantities of blood, — sometimes, a table-spoonful only. The contraction of the capillaries, therefore, begins very easily.

which illustrate still farther the principles upon which bloodletting operates. A severe shock of a fall prostrates the vital powers, determines the nervous influence strongly upon the circulatory and other organs, and the whole capillary system contracts. An ounce of blood now lost may increase all these phenomena.

SECTION VIII.

WHY is a second or third bleeding often better borne than the first? The first bloodletting lessens the irritability of organs not directly concerned in any local disease, and thus diminishes their susceptibility to the loss of blood. It diminishes the sympathetic influences which had been exerted on the heart and capillary blood-vessels. In the sinking states of venous congestions and congestive fevers, it so modifies the powers of the capillary system, that its tendency to contract is counteracted, and restores, in some degree, the balance of the circulation, by which less blood is accumulated about its centre. And when it more rarely happens in simple inflammation that the tolerance is increased by the first bloodletting, it appears to be chiefly owing to the former causes. That the power of the system, as affirmed by some late writers, to bear the increased loss of blood is generally owing to an actual increase of inflammation, appears to be contradicted by its frequent ability to sustain this loss in a few minutes after the first operation; and in venous congestion, an increase of this condition of disease often diminishes the tolerance.

The causes, then, of which we have now spoken, appear to constitute what is called "the power of the system," in the cases supposed, to bear the loss of blood. But to say that inflammation enables the system to bear the loss, or a greater loss, "by increasing power," is only asserting its ability; and to say that the greater power, in the cases supposed, is owing to an increase of disease appears to be an error.

When, however, inflammation actually increases, there can be no doubt that its tendency is, *cæteris paribus*, to increase

the tolerance of loss of blood; especially in its early stages. It is upon this principle that it is sometimes better to suffer inflammation to advance to a certain limit before resorting to bloodletting. This is especially true where inflammations recur, or spring up, after considerable debility has been already induced. We suppose the case to be one where general bloodletting will, in the end, be most advantageous. If we bleed, in these instances, before the system is influenced by the new local action, we may, at first, increase its severity by increasing irritability and the force of the general circulation. If we await the constitutional sympathy, the loss of blood will, then, only reduce the sympathies and moderate the irritability; (1) the latter, as well as the former, being now consequent upon a cause whose remedy is the loss of blood. Thus, a general balance only will be established, whilst the local disease will receive an impression that cannot be disturbed by constitutional influences.

Whilst, therefore, in regard to bloodletting in inflammations, it may be sometimes expedient to allow the disease to advance, that it may be the more effectually subdued; on the other hand, if general bloodletting be prematurely or excessively applied, so that the constitutional influences, thereby induced, shall excite or increase local inflammation, the system may be ultimately so far brought under the direct influence of the local affection, that it may bear with advantage a farther loss of blood.

SECTION IX.

If the foregoing views be correct, that opinion would seem to be erroneous which would render syncope a uniform criterion of the quantity of blood, which the nature of the case may, at the moment, demand. If moral affections have been particularly instrumental in producing the paroxysm, or the stomach have contributed strongly to the result, we readily perceive that the

(1) The first application of bloodletting may "lessen" or "increase" irritability, according to the existing state of the vital properties, as known by the phenomena. Nor is the statement in our text incompatible with the general fact that "diseases yield most readily in their forming stage." It does not concern any absolute law in physiology.

loss of blood has had little to do with the contraction of the capillaries, upon whose vital forces it is the object of the remedy to produce, in a direct manner, a strong impression. The change must commence in these vessels, and be propagated from them to the heart. The difference appears to be obvious betwixt that impression which is made directly upon the vital powers of the capillaries, and thence extended by sympathy to the same properties of the heart, and a direct failure of the heart's action, with its consequent contraction of the capillaries, as takes place when syncope follows mental emotion, or gastric influences. The contraction induced by the loss of blood is of a vital nature, and the result of the action of the vital powers of the capillaries; whilst in the other case it arises, mainly, from a direct suspension of the heart's action, and a want of supply. In the latter instance, the vessels probably contract more from their elastic property; but however this may be, the contraction not being the result of a loss of blood, the alteration cannot be maintained in the instruments of disease after the circulation is restored. But, the important difference consists in the series of changes which leads to the common paroxysm of syncope, and their being determined, in one case by loss of blood, and in the other by the cerebral influence. These differences denote very different influences in the two cases. In one there is a profound alteration of the vital forces, whilst in the other it is more superficial or wholly evanescent. Such, however, is the effect of the nervous influence upon the organs of circulation, that when a powerful impression can be determined upon the brain by the actual loss of a small quantity of blood, the effect of a greater loss, under different circumstances, may be thus obtained.

"Dr. Moseley," says R. Jackson, "advises us to bleed, *ad deliquium*, in yellow fever. I coincide with him in recommending extensive bleeding in this form of disease; but I do not accede to the rule which he assumes for judging of the measure. It is vague and uncertain. Deliquium occurs sometimes from the loss of a few ounces of blood, sometimes scarcely from the loss of six pounds. The act of fainting is not, therefore, a rule of dependence for regulating practice." (1)

We give a case from an author who considers syncope an "unerring test of the quantity of blood that will be necessary."

(1) History and Cure of Febrile Diseases, vol. i. p. 226.

"A patient," says Dr. Wardrop, "suffering inflammation of the brain, had been repeatedly bled, both by venesection and leeches, but without relief. The pain became more intense. He was bled again and fainted. The bandage was removed; but as soon as the patient recovered, he found the fixed pain in the head remaining. The bandage was immediately reapplied, and a *very large quantity* of blood was removed before he again fainted. A state of perfect syncope remained upwards of five hours. Relief was now for the first time obtained, and it was complete and permanent."(1) This case is also interesting as exemplifying what we have said as to the exciting effect of the cerebral influence in inflammations of the brain; whilst it shows the progress of that influence even under the loss of blood; and how, also, the ability of the system to bear the loss of blood may be promoted by the loss.

In all severe inflammations and congestions, therefore, where syncope follows the loss of small quantities of blood, the operation should be resumed in some ten or fifteen minutes, when, as in the foregoing case, "a very large quantity of blood" may, perhaps, be removed.

The importance of establishing the impression upon the *vires vitæ*, of which we have spoken, and of making that impression decisive in all grave diseases which demand the loss of blood, we shall variously illustrate by examples in a subsequent section. "When a large quantity of blood," says Dr. Wardrop, "is not taken away at the first bleeding, or at a second depletion quickly succeeding, I have generally found that on all future occasions it is seldom practicable to abstract any considerable quantity, however necessary it may appear; and thus it is, that when copious bleedings are not employed at the commencement of the treatment of inflammatory diseases, and if the patient afterwards recover, it has generally been from the employment of a great number of bleedings." "Moreover, it is only *such cases* wherein the pernicious effects of bleeding are exemplified."(2) This was especially Robert Jackson's doctrine. These small and frequent bleedings, by their oft impression upon the vital forces, irritate the vascular system, and frequently add to the force of the heart's action. The consequence of all this may be even some new inflammation, which is confounded with simple "irritation." It

(1) On Bloodletting, p. 37. (2) Ibid. p. 31.

will be readily seen, too, from what we have said of the effect of bloodletting upon the brain, that this organ will be especially liable to be the seat of the artificial disease. There is great reason to believe that nearly all the cases of this nature, and of those that may properly be called "diseases of irritation," have been produced by thus teasing the vital powers by small and repeated bloodlettings, and not by "excessive loss of blood." We are supposing, however, that the cases have been such as require the loss of blood, — not those instances where bloodletting is not called for, or in hemorrhage which arises from accidents or parturition. In these cases, a very copious effusion of blood may bring on constitutional "irritation," and local inflammations may supervene. So the forces of life may be perniciously affected, inflammations, &c., excited, by a powerful cathartic, or any other agent that may operate with violence, — but which in diseased states of the system would be in the highest degree advantageous.

"Dr. Rush," says R. Jackson, "carried abstraction of blood to a great extent in yellow fever; but the quantity substracted was obtained by repeated substractions, — not by abstraction at one time. The mode of depletion was not abrupt — such as arrests disease by force, and such as I have in view in the present history." (1) "There is no maxim," says Dr. Wardrop, "of the practical correctness and importance of which I am more fully convinced, than that the loss of a certain quantity of blood at the *first bleeding* is of greater utility in stopping the progress of inflammatory diseases, when general bloodletting is required, than the abstraction even of a much larger quantity of blood by successive bleedings." (2)

We shall hereafter see that these opinions have long prevailed amongst the most eminent in medicine.

It may be well imagined, that important differences will arise in respect to the influence of bloodletting upon the *vires vitæ* of the vascular system, from the quantities abstracted, and even the modes of taking away the blood, when the fact is considered that the blood itself is affected by these contingencies; "that coagulation takes place more or less quickly, according as the orifice from which it flows be small or large, or the stream fast or slow." But the manner in which the component parts of the blood se-

(1) Op. Cit. p. 227. (2) Op. Cit. p. 30.

parate from each other, the appearance or disappearance of the buffy coat, according to the foregoing circumstances, especially the quantity of blood abstracted, denote a change in the constitution of this fluid. These phenomena are manifestly owing to differences in the impressions which are produced upon the blood-vessels and nervous system, by which the properties of the blood are more or less altered; these differences being, therefore, an evidence that bloodletting produces its direct effects upon the *vires vitæ*, and that these forces will be affected according to the quantity of blood taken, and the manner of taking it.

There are many instances, however, of inflammations and venous congestions where the predisposition to morbid action may be so established by some peculiarities in the remote causes, or by an excessive alteration of the vital forces by causes that determine the most simple forms of inflammation; or, again, of chronic inflammations and congestions, that are confirmed by habit, constitution, &c.; or, yet again, of inflammations affecting certain tissues where the vital forces have a natural tendency to maintain a morbid alteration, as in the ligaments, &c., that it may be necessary to repeat the impression from loss of blood many times before the morbid action can be overcome.

From the foregoing considerations will appear the fallacy of the statement made by Dr. Arnott, "that it is a great modern improvement in the practice of the healing art, in bleeding for the cure of inflammation, to take the blood away as *quickly as possible;* since intense inflammations of the brain, lungs, bowels, &c., are equally removed by faintness, whether it happens after the loss of ten ounces of blood, or of fifty; or even, as sometimes occurs, when it happens without bleeding at all, after merely tying the arm in preparation." (1) This is notoriously only true in

(1) Elements of Physics, vol. i. pp. 470, 472.

Hence, too, appears the fallacy of applying the "elements of physics" to considerations of this nature.

Dr. Arnott has gone so far as to construct a machine as a substitute for bloodletting, which "diminishes the atmospherical pressure on a considerable part of the body, on the principle of the dry cupping-glass used very gently, and thus suddenly removing for a time, from about the heart, a quantity of blood, sufficient, by its absence, to produce faintness." (*a*)

Dr. Graves remarks upon the same subject, that "if we have to deal with an extensive and violent inflammation, we do not abstract blood by a minute opening; we make a large orifice, or we open a vein in both arms at the same time; we place

(*a*) Op. Cit. p. 574.

rare cases, when the beneficial results of syncope supervening on small abstractions of blood depend upon peculiar causes already indicated, the principal of which is some unusual relation of the brain to other organs, especially the sanguiferous. This relation may be temporary, when of course, it is preternatural.

On the other hand, all the symptoms of disease may totally vanish before syncope takes place, as is often seen in croup, pleurisy, &c. When this manifestly arises from the direct impression of the remedy upon the sanguiferous system, it would be very inexpedient to extend the loss of blood beyond its object. But then the change must be radical; it must be exerted by a cause which reaches deeply the vital forces of the capillaries, or rather, the extreme vessels. It has been well said by Mr. Porter, that "it is not sufficient to diminish an increased action, unless the constitution be kept, until the period of danger is over, in a condition that will render a renewal of that action unlikely to occur."(1)

Here, then, is opportunity for the adjuvants of bloodletting; and according to their judicious use, bloodletting will be more or less circumscribed,—perhaps superseded, when it might be otherwise indispensable to life.

For the reasons now stated, and others which we shall present, we reluctantly dissent from another author of well merited eminence, who establishes the rules, that "the quantity of blood

the patient in an erect posture and endeavour to produce deliquium. It sometimes happens that the patient *faints from fear*, or before any considerable quantity has been lost; and this faintness, as Dr. Arnott remarks, *answers as well* as that which results from venesection. This I can also testify,—for I have seen all the good effects of bleeding produced by the terror with which the operation frequently inspires persons of delicate or nervous temperaments." (*a*)

All this, however, is not only contradicted in most instances by experience, as we shall still endeavour to show, but by the principles upon which disease is overcome by loss of blood.

It appears, however, from Dr. Davies, that we are much in the minority among our British friends. "It is the practice in this country," he says, "to bleed the patient, in pneumonia, in an upright posture, and to make a large orifice in the vein, so that fainting may be speedily induced." This is to save "the vital fluid." (*b*) But it should be recollected that a strong current is now setting against the abstraction of blood, even in pneumonia, from a greater fear of debility than of the disease. It is the predominant feeling "to save the vital fluid."

(1) Observations on Surgical Pathology of the Larynx and Trachea.

(*a*) London Med. and Surg. Journ., vol. iii. p. 391.

(*b*) On Diseases of the Chest, p. 186.

which has flowed becomes accurately diagnostic of the nature of the morbid affection, demonstrative of the powers and susceptibilities of the patient, and taken in conjunction with other circumstances, indicative of the propriety or safety of the further detraction of blood;" that "there is, in *every instance*, a *strict alliance* between the tolerance of the loss of blood and the exigencies of the cure;" that "the power of the system is *exactly* in proportion to the necessity for bloodletting, and its susceptibilities, to the caution required in the administration of this remedy;" "and, therefore, the quantity of blood permitted to flow, before the first appearance of approaching syncope is manifested, is the precise quantity which ought to be taken;" that "the quantity borne will be precisely in proportion to the exigencies of the case;" &c. (1) And again, in a more recent work, he says, "by placing the patient upright, and looking upwards, and bleeding to incipient syncope, the violence of the disease, the powers of the system, and the due measure of the remedy, are determined at the same time." (2)

The import of these rules is sufficiently obvious; since no man ever ventured to carry bloodletting beyond absolute syncope; and therefore syncope is an undisputed test of the present ability of the system to bear the loss. Besides, whatever might be the rashness of man, nature here places an obstacle in his way.

Dr. Wardrop gives us the same rule. "The state of fainting," he says, "is to be considered an index of the quantity of blood which is necessary to be removed for the *relief* of the disease; and as I have already said, it will be always found that the quantity is in the ratio of the propriety and necessity of abstracting it." Again, "the state of fainting, which I have already endeavoured to point out as an *unerring* criterion for estimating the extent to which blood should be removed in those cases where general bleeding is most expedient, is by most practitioners taken as a guide." "But for the abstraction of a certain quantity of blood, in the treatment of diseases requiring depletion, *fainting ought not* to be considered as an index of the quantity of blood proper to be withdrawn;" &c. But again he says, "This

(1) Marshal Hall on the Effects of Loss of Blood, p. 170, *et passim*.

(2) Hall's Principles of the Theory and Practice of Medicine, p. 79. Our author and ourselves, are now considering cases of acute, rather than of chronic disease.

fact coincides with the observations I made on the quantity of blood necessary to produce syncope in inflammatory diseases which, I remarked, was always in proportion to the quantity necessary for remedying the disease." (1)

But the most exceptionable part of Dr. Hall's rules, as it appears to us, applies to the repetition of bloodletting: "If much blood has flowed," says Dr. Hall, "before incipient syncope has been induced, revisit your patient *soon;* and you will probably have to repeat the bloodletting in consequence of the severity of the disease, especially if you were not called in early in the first instance. If, on the contrary, *little* blood has flowed, *neither does the disease require*, nor would the patient *bear*, farther general depletion. Is not this an interesting and important piece of information?" (2)

In the first place, in all candour, we are obliged to answer the interrogatory in the negative. If delay be justifiable in either case, we think all experience warrants us in saying, that it is most so when *much blood* has been taken. On the other hand, cases are constantly presented to the observation of all, where syncope follows the loss of a few ounces of blood at the first operation, but where the system will sustain the loss of one or two pounds in a few hours, or less, afterwards. These, indeed, are cases which demand the most assiduous attention, and a prompt and repeated abstraction of blood. This, as is well known, is especially true of congestive fevers.

Again, our author says, "bloodletting should, in general, be the more prompt, the greater the tolerance of the loss of blood in the previous bloodlettings." (3) These quotations fully explain the meaning of our author as to "the tolerance of loss of blood," and the principles embraced in our introductory extracts from this writer. The facts, also, show us that it is not common inflammation alone that sustains the system under a great loss of blood. For we daily witness what we have just stated as to the tolerance of the system after the first loss of blood in congestive fever, when there is no other inflammation than what may appertain to the congested veins; and even this, at the onset of the attack, is of a depressing nature, as is, also, the acute form of simple venous congestion, and, often, of spontaneous phlebitis. But there are

(1) On Bloodletting, pp. 36. 42. 44.

(2) Lectures, &c. Dr. Hall thinks this rule, which he calls *his* "remains neglected, either from inattention or *jealousy*." (3) P. 154.

complexities attending the former condition of disease, and their influences upon the system as modified by bloodletting, which we have already attempted to explain, and shall attempt more fully hereafter. Whatever share the ultimate tendency of the inflammation which constitutes the important pathological state of the congested veins, (1) (abstracted from the counteracting influence of the peculiar forces of the venous tissue,) may exert in enabling the system to bear an increasing loss of blood, it is sufficiently obvious that the sustaining principle belongs, in part, to some alteration of the vital powers at large, which differs from that of inflammation. This, indeed, may be generally more or less affirmed of idiopathic fevers, when unattended by inflammation or venous congestion. It is also an important fact, to be well regarded; and was only neglected by us when considering the tendency of inflammation to sustain the system under the loss of blood, to avoid any embarrassing intricacies. We must take the facts as they are, and by them arrive at our conclusions. We see, for instance, that in the cold stage of an intermittent, the patient faints from the loss of eight ounces of blood; whilst in the succeeding stage he may bear the loss of two pounds. This happens, where there is no inflammation, and in subjects who could not sustain the loss of ten or twelve ounces in a state of health.

Again, in simple venous congestions, and in the worst forms of phlebitis, it generally happens that less blood will be borne at successive bleedings, than in congestive fevers unattended by common inflammation.

Our forefathers were better acquainted than many of us with the ability of the system to bear great losses of blood in severe inflammations; and they have left behind them the most palpable demonstrations to this effect. Had we paid more respect to the past, the foregoing facts would not be now coming up as new. We should not now hear it asked, "is not this an important piece of information?" But they went even farther, and much beyond what we have generally yet failed to discover. By a vast series of observations, they established the fact, that the system, in congestive fevers, and in many cases of simple venous congestion, by cautious bloodletting at first, soon came to bear the loss of great quantities of blood. They ascertained, that these cases de-

(1) The "pathological state" is of no importance to our conclusions. We are only concerned, at present, about the *effects* of bloodletting.

mand a prompt and repeated application of the remedy, and that it is much more indispensable to life in congestive fevers, than in inflammatory affections. They had also found out, that where they could carry bloodletting to a large extent in inflammations, as was the common usage, they could go quietly home with the conviction that its repetition would not, probably, be demanded. But they were full of apprehension when their patients fainted from the loss of only a few ounces, and hurried back to consummate the operation. But these facts appear to be unknown to many, or to have passed from their memories. Having had some curiosity, ourselves, to see what was the fashion of our fathers, we have turned over their ponderous folios; and having gleaned from them the foregoing facts, we have thought it might be interesting to some to know exactly the grounds of our conclusions; and, therefore, we have collected their own statements, and arranged them in their appropriate place. We have listened to the voice of this experience, and have tested it in practice. Our results, in all respects, correspond with the assurances that had prompted our action. They have established our faith, and now lead us to protest against the rule that, "if much blood have been taken before incipient syncope, more will probably be demanded soon; or, if, on the contrary, little blood have flowed, neither does the disease require, nor would the patient bear, farther general depletion." We are apt, indeed, if bloodletting be indicated at all, to reverse the proposition; and we think we see, that it is in this modern rule that the "bark and wine treatment" of congestive fevers has had its origin.

We cannot, therefore, but feel that our author's rule is replete with danger, considering the vast prevalence and malignancy of congestive diseases, which so often admit at the onset only a very moderate loss of blood, but which demand, and may receive, within a few hours, or even in a few minutes, after the first loss, an extensive application of the remedy.

We cannot but think, too, that the hazard of the rule is increased by the reference which is made to "irritation and exhaustion" in the following quotation; since the attention is thus diverted from the great class of congestive diseases, which are attended by the greatest prostration, to affections of rare occurrence, unless from the ravages of prolonged disease. "If much blood," says our author, "has flowed before the occurrence of syncope, inflammation must be suspected; if little, we must sus-

pect that, however similar the symptoms, the case is, in fact, of a different nature, perhaps of irritation,—perhaps of exhaustion." (1)

We shall ultimately see to what a serious result the foregoing doctrine may lead; and as it is especially exemplified by Dr. Hall in puerperal fever, we shall here quote the experience of an author who comes under review by Dr. Hall, and who was well qualified to give us practical advice by the disastrous effects which attended his early observation of the rule as laid down by Dr. Hall.

We speak of Mr. Hey, who admits that "there is a vast difference in the puerperal fever at different times, and in different situations and circumstances. In some cases, it appears like a phlegmonous inflammation; in others, it destroys with more rapidity and certainty than the plague. But the means of cure are precisely the same in both; but in the *latter* their measure is *greater* and less limited; and the *period* within which they must be employed is *far more circumscribed.*" (2)

Here then we see the most enlightened experience opposed to the rules of Dr. Hall. "If *much* blood had flowed," says Dr. Hall, "revisit your patient soon, &c." "If, on the contrary, *little* blood has flowed, neither does the disease require, nor will the patient bear, farther general depletion." (3)

In those cases of puerperal fever which resembled "phlegmonous inflammation," Mr. Hey got a great quantity of blood from his patient at the first bleeding, and slept soundly till morning,—when he often found that the disease was slain. But not so with those which "destroyed with more rapidity and certainty than the plague," when subjected to the procrastinating system, or to the "bark and wine treatment." Here he was all nerve and vigilance. (4) If, when called at 10 o'clock in the evening, he

(1) P. 81. The distinction which is here made betwixt inflammation and irritation, when the symptoms are "similar," we think untenable, and shall assign our reasons in a subsequent place.

Dr. Hall's Theory and Practice of Medicine has been published since we made most of the foregoing comments. It is due to this distinguished man to say, that in his late work, he admits that there may be exceptions, and he states one which we had made. But the general import of the subject is not affected as it relates to our author's rules.

(2) Puerperal Fever, p. 183.

(3) Ut cit. See, also, Dr. H. on Bloodletting, p. 154.

(4) "It required," says Mr. Hey, "more prompt attention and vigorous treatment."

got but six ounces of blood before the patient fainted, instead of going quietly to bed, he called again in an hour, and took 12, 16, 20, 40, 50 ounces more, just as the first six ounces may have happened to raise the vital forces of the system. And just as these ratios happened to occur, so was he contented or anxious. If his patient continued to faint from small losses of blood, he continued to call from hour to hour and to re-open the orifice, or did not quit his charge till victory was won. Such was Mr. Hey; great as a philosopher, physician, and man.

When we come to the more practical application of our subject, we shall then see that the world has been full of illustrations like the foregoing.

In local congestions, or congestive fevers, the amount of disease is sometimes so great, and the blood so accumulated in the venous system during the state of collapse, as to render cordials and stimulants necessary at the onset, to sustain the vital powers; although it be obvious that the congestion will not yield till the lancet can be liberally employed; and this, perhaps, may never happen in the case,—the vital powers continuing to sink under the weight of congestion. (1) If we bleed under these circumstances, we shall only hasten their extinction by the direct impression upon them, and by more greatly deranging the balance of the circulation.

"Natura, malum sentiens, gestitat magnopere mederi;" (2) sed, "repugnante natura, nihil proficit medicina;" (3) et, "deficiente natura, quicquam obtinet medica ars, perit æger." (4)

Again, there are other conditions of the system in which bloodletting may be indispensable to the removal of disease, yet can-

(1) These are cases in which Dr. Rush says that, "so great a prostration of strength takes place, that even a single evacuation from the bowels has induced death;" (*a*) and yet the lancet may invigorate such a patient.

"Daily experience convinces me," says Dr. Mackintosh, "that there are diseases which require a combination of bleeding and stimulants, and that it is not inconsistent with sound notions of pathology to bleed first, in order that we may be able to stimulate, and to stimulate for the purpose of enabling us to draw blood." (*b*) So, also, Dr. Armstrong, and some others.

(2) Galen, l. de Natur, Facult.
(3) Celsus, l. 3. c. 1.
(4) Hippocrates, l. de Arte.

(*a*) Note to Cleghorn's Diseases of Minorca, c. iii. p. 130. See, also, Hunter on the Blood, &c. p. 346.

(*b*) Pathology and Practice of Physick, vol. 2. p. 241.

not be applied without great danger to life. Our author shall state one of them. "The injection of inflammation" in cerebral hemorrhage, says our author, "is probably seated in the minute arteries and the capillaries; whilst the morbid anatomy, in these cases, consists in congestion or rupture of the minute veins and capillaries of the membraneous substance of the brain." Without inquiring, here, whether this doctrine is at all sustained by analogy or by direct observation, we shall adhere to our intended illustration. "The principal point," continues Dr. Hall, "which I wish to impress upon your minds in reference to bloodletting, is its different measure proper in mere injection and actual rupture. In the former, there is extreme tolerance of loss of blood; in the latter, the system is extremely, and even dangerously susceptible of this loss." (1)

Here, then, are two conditions in which the loss of blood may be equally necessary to the safety of the patient. In one, the tolerance of bloodletting is commensurate with the exigencies of the cure; but in the other it may be wholly inadequate. The effusion is generally too small to have altered the state of disease, or its relation to bloodletting; (2) but, on the contrary, the irritation of the clot, the laceration of the brain, the supervention of other derangements, are aggravating causes of the cerebral congestion, and a continuance of the hemorrhage is more or less probable whilst the congestion remains. Such, however, has been the prostration of the vital forces, consequent upon the sudden and violent injury of the cerebral powers, that an immediate resort to bloodletting may extinguish them at once, or place them beyond their own recuperative efforts.

We esteem very highly the practical tendency of our author's remarks upon the subject of bleeding in cerebral hemorrhage; but we cannot but think that they are too comprehensive, and should only apply to the rarer cases in which the cerebral influence has very seriously prostrated the organic forces, and then, only, during the existence of that prostration. On this question we shall speak farther in a subsequent section, for the purpose of extending our practical illustrations of the principles upon

(1) Lectures on the Nervous System, lec. 7, pp. 144. 146.

(2) This is admitted by Dr. Hall, who says "if the patient be able to lose a large quantity of blood without change, pursue and repeat the remedy boldly; *his life depends* upon ample depletion of the sanguiferous system." (Ibid.)

which bloodletting operates. We already see, however, how it is the tendency of the cerebral influence to counteract the exciting effect of inflammation, by its controlling power over the organs of circulation and the general forces of life; as, also, how it is the effect of loss of blood to determine, in the most powerful manner, the same influence towards the same results. Just so is it, also, in those congestive affections of which we have just spoken. The forces of life are so depressed by various influences, though, doubtless, very greatly through some pernicious effect of remote and internal causes upon the nervous system, that the cerebral impression upon the organs of circulation is at once obtained by the loss of small quantities of blood. But this is a very different state of the system from that of debility, and one in which the very common practice, especially in Europe, of applying tonics and stimulants, is generally disastrous.

Again, the power of the system to bear the loss of blood that may be necessary to overcome inflammation, may be destroyed by other remedies. Thus, it frequently happens in croup, that emetics, especially of tartarized antimony, render bloodletting impracticable, — particulary when they produce catharsis instead of vomiting; and the patients may then die from their inability to sustain the necessary loss of blood. Thence appears the importance of carefully considering the primary application and the extent of emetics, or of nauseating remedies, in cases of inflammation where the speedy loss of blood may be essential. We are certain, from observation, that bloodletting has lost its reputation, with some, in pneumonia, &c., from its having been applied unsuccessfully under the prostrating influence of tartarized antimony, and when, in consequence, the powers of the system would only admit of a moderate loss of blood.

Again, on the other hand, that the power to bear the loss of blood is rapidly developed by the loss itself, is evident from constant observation. "Immediately upon the application of warmth to the surface, and light diffusible cordials to the stomach," says one of the best original writers on congestive typhus, "take a little blood; perhaps two, four, six or eight ounces, according as the patient may bear it. If he be a little faint, it is nothing but what is common; a little time will remove it. He will soon bear a second bleeding in this condition better than the first." (1)

(1) Gallup on the Epidemics of Vermont, p. 182, 1815.

Aretæus not only describes this disease, but advises the same enlightened practice,—especially if the congestion be the occasion of great prostration and "syncope." "Venas itaque in cubito protinus cædito, multumque sanguinis, *sed non semel*, totum mittito; imo et bis, et ter, alio die, *quo interim vires instaurentur* repetito." (1) Alexander of Tralles discourses in the same manner upon this subject. (2) The language of A. Parey is very remarkable in describing the treatment of the Plague and "Pestilent Diseases." It corresponds with the best philosophy of our own day.

"So soon," he says, "as the heart is strengthened and corroborated with cordials and antidotes, we must come to phlebotomy and purging." "You may perceive that the patient is ready to swoon when that his forehead waxeth moist with a small sweat suddenly arising, by the aching or pain at the stomach, with an appetite to vomit, and desire to go to stool, gaping, blackness of the lips, and sudden alteration of the face into paleness; and, lastly, most certainly by a small and slow pulse: and then you must lay your finger on the vein, and stop it until the patient come to himself again, either by nature, or else restored by art; that is to say, by giving him bread dipped in wine, or any other such like thing; then if you have not taken blood enough, you must let it go again, and bleed so much as the greatness of the disease or the strength of the patient will permit or require; which being done, some of the antidotes that are prescribed before will be very profitable to be drunk, which may repair the strength, and infringe the force of the malignity." (3)

Sydenham, who must have had a good knowledge of this disease, advances the same opinion as the foregoing observers. (4) So, also, Lommius: "I have seen some in acute fevers, who, when I had taken away, at the beginning of the distemper, only eight ounces of blood, immediately fainted; and yet in the progress of the disease, these very persons have voided three pints or more without the least langour." (5) Moseley observes that, "it has frequently happened in the fever of the West Indies, that accidental bleeding from the orifice, when the patient

(1) De Cur. Acut. Morb. l. 2, c. 3, de Syncope, and c. 7, de Venis.
(2) L. 12, c. 3.
(3) Works, Book 22, c. 24, p. 508.
(4) Works, (Swan's Edition,) p. 567.
(5) On Continual Fevers, p. 26.

has fallen asleep, to far greater quantities than has ever been directed to be taken away, has carried off the fever entirely; and the surprise on discovering a profusion of blood in the bed has been changed to joy, for the alteration produced in the patient."(1) "The distinguishing feature of purpura hemorrhagica," says Plumbe, "is, on the one hand, according to received notions, identified with the lowest degree of debility; on the other, it is ushered in with symptoms which cannot subsist many hours uncontrolled by *active treatment*, without the most *imminent peril* to internal organs. In one point of view, the probability exists of extinguishing life by extracting a *few ounces* of blood; in the other, such a measure appears a matter of absolute necessity."(2) We admit, that this, as a general picture, is highly drawn; but it represents some cases truly, in a *practical* sense. Dr. Burnett, in describing the congestive fevers of the Mediterranean, says, "it will often happen, after a few ounces of blood have flowed, that syncope will be induced." But, "in the course of an hour, the bleeding may generally be repeated, and thirty or forty ounces may be taken away without producing syncope."(3) In these cases, he gives the bad advice of "bleeding the patient in a horizontal posture."(4)

(1) On Tropical Diseases, p. 448.

(2) On Diseases of the Skin. Sect. 5, c. 1, p. 268.

(3) P. 20.

(4) We are also surprised, that so great an advocate for bloodletting as Dr. Wardrop should advise that "the patient be placed in a horizontal posture;" and this more especially as he insists that the loss of blood should be carried generally to the point of approaching syncope." (*a*) Dr. Rush fell into the same error, and in a class of cases where, most of all, it should be avoided. "Where there is great disposition to syncope," he says, "and where it is attended with alarming and distressing circumstances, blood should be drawn in a recumbent posture." (*b*) So, also, Drs. Macbride, (*c*) Copland, (*d*) Van Swieten, (*e*) and others. This practice has been perpetuated from the ancients. Thus, Avienna: "Phlebotomandus præterea supinus jaceat, quando phlebotomatur." (*f*)

We would also object, here, to Dr. Wardrop's advice, in case of a repetition of bloodletting, that we should separate "the agglutinated lips of the wound with a probe, or the head of a common pin." We practised this method once to our sorrow; phlebitis having immediately followed, of which our patient died. We

(*a*) On Bloodletting, pp. 42, 61.

(*b*) Medical Inquiries, &c. vol. iv. p. 341. Leeches should be substituted.

(*c*) Theory and Practice of Physic, B. 7. p. 268.

(*d*) Dic. of Prac. Med. Art. Blood.

(*e*) Commentaries, vol. ix. s. 890. Vol. viii. s. 854.

(*f*) L. 1. Fen. 4. c. 20.

We sometimes see the same prostration of the forces of life from acute inflammations, especially of the intestines when attended with much pain. Here, too, is the same inability at first to bear the loss of blood, but the ability is increased by its abstraction. Thus Dr. Wardrop: "A gentleman was seized with acute pains in the bowels, accompanied by a good deal of tenderness on slight pressure, along with some degree of *febrile excitement*. On opening a vein in his arm, only a few ounces of blood were removed, when the pulse sank and he fainted. In two hours afterwards I bled him again, and he did not fall into a state of syncope until he had lost about thirty ounces of blood." (1)

In the foregoing case, from the peculiar relations of the intestines to the brain, the pain had so affected the cerebral powers, that as soon as the capillary contraction began from the loss of blood, the prostrating effect of the cerebral influence was strongly determined upon the organs of circulation, and syncope followed, notwithstanding the state of "febrile excitement."

We might thus go on multiplying exceptions, till we should make out our case, in this manner, that the *general rule* upon which we are commenting is itself an exception to another general rule. Dr. Hall, indeed, states, that so small a cause as varying temperatures of the feet may influence the operation of bloodletting. (2)

Such is often the prostration of the vital forces at the invasion of many acute diseases, when the prompt application of bloodletting may be indispensable, that a few ounces of blood may induce syncope; and were we to be governed in these cases by our author's rules, that "the violence of the disease, the powers of the system, and the due measure of the remedy, are determined at the same time;" or, again, "if little blood have flowed, neither does the disease require, nor would the patient bear, farther general depletion," — it were well, for reasons already assigned, that such cases should be left to the chances of nature. If syncope happen after the loss of four ounces of blood, as is fre-

moreover adopt the advice of the British and Foreign Med. Rev. (a) of making a longitudinal incision; that the process of healing may be completed more speedily than in the usual transverse incision.

(1) On Bloodletting, p. 33.

(2) On Effects of Loss of Blood, p. 235.

(a) No. 11. p. 239.

quently true of congestive fever, and even of some grave inflammations when complicated with venous congestions, that, of course, limits the operation. And are we to assume this as a criterion of the "exigencies of the case, — of the power of the system, — of the due measure of the remedy?" We needed not the precaution that bloodletting should stop at the invasion of syncope. This is obvious enough, and our author, therefore, even leaves us with the conclusion that this is the test of the exigencies of the case. If the system bear the loss of 60 or 80 ounces, it is very well, and shows so much power, that we should soon call again and take as much more. But if syncope follow the loss of four or eight ounces, the exigencies for bloodletting are small, and "neither does the disease require, nor would the patient bear, farther general depletion." In truth, however, these are often the very cases where the patient may be alone saved by the loss of 100 or 150 ounces.

Our author also leaves out of consideration by these exclusive rules the various accidental causes that may greatly modify the present effect of bloodletting. We have already mentioned several; to which we may add imagination, moral emotions, the rapidity with which the blood flows, the erect position, &c., that may alone compel us to desist, in severe inflammations, before six ounces of blood have escaped from the orifice.

Another defect, as it appears to us, in our author's adherence to the foregoing rules, as manifested by their frequent repetition, is the want of other coincident signs for the application of this great remedy, and their consequent tendency to divert us from what is really of greater importance in arriving at a right decision, namely, the symptoms of every case. If, therefore, we are asked to provide a better guide, we should say, carry the remedy as far as may be demanded by the symptoms, and watch them closely, till the patient is out of danger.

But our author's opinions, or *his rules*, are not wholly novel. We believe that they have guided many practitioners at all ages, and that it has been owing to the alarm at the rapid approach of syncope when bloodletting has been practised in congestive fevers, that this remedy has not been repeated, or has been wholly abandoned, and even that most fatal practice, "bark and wine," extensively substituted.

It will be also seen from what we have said of the nervous influence in bloodletting, that if blood be abstracted rapidly, the

power of that influence may be such as to determine a paroxysm of syncope long before it would be induced by a much greater quantity when gradually taken. The loss of this greater quantity, therefore, may be sustained, and may be necessary to the removal of the disease. Hence we think the advice erroneous, that in pneumonia, and in all grave inflammations, the blood should be taken from a large orifice. The practice is founded, as we have said, and as we shall see, upon the dread of debility. We would only apply it to milder inflammations, and to idiopathic fever, where it may be an object to induce early syncope; and here, as in other cases we have mentioned, the smaller quantity of blood, of which we have just spoken, with the aid of the cerebral influence, may be entirely sufficient.

The power of the system, therefore, to bear the loss of blood may not depend so much on the attendant circumstances of disease, as on certain contingencies which may be regulated by art. If we bleed from a small orifice, the loss of a large quantity may be borne; if from a large in the same case, we may be very soon arrested in the operation by the cerebral influence. To which of these instances, so common in practice, shall we apply the rule of Dr. Hall, that "the violence of the disease, the powers of the system, and the due measure of the remedy, are determined at the same time?" Or, who will affirm, with Dr. Wardrop, that "I have always remarked that the quantity of blood necessary to produce syncope, in inflammatory diseases, has always been in proportion to the quantity necessary for remedying the disease?" Who will say with Dr. Hall, that, "in doubtful cases, if little blood have flowed before syncope occurs, we must suspect, *however similar the symptoms*, the case is, in fact, of a different nature from inflammation,—perhaps irritation, perhaps exhaustion;" that "in inflammation, on the contrary, and *precisely* according to its degree, duration, severity, and extent, much blood flows before deliquium is experienced, and *this is exactly what is required to subdue the disease*, and is well supported by the actual condition of the system;"(1) our author, also, always recommending the erect posture.

We have already endeavoured to show, how important it may be to regulate the foregoing contingencies, that an amount of

(1) On Loss of Blood, pp. 154, 209.

blood may be abstracted that shall not only impress the cerebral system more or less fully; but shall establish a change in the instruments of disease independently of that influence. We have already shown the frequent importance of the combined effect of these impressions upon the forces of life. It is well exhibited by the following case. "A patient, at the point of death from acute inflammation of the pleura and lungs, was bled to the extent of fifty ounces, when he had obtained no relief. If we had stopped here, in two hours the patient would have died. After abstracting about six ounces more blood, syncope came on, from which he recovered convalescent." (1) If this patient had been bled in an erect posture and from both arms, and had syncope followed the loss of 15 or 20 ounces of blood, it is scarcely probable that he would have been saved. Here the importance is fully shown, not only of abstracting a certain quantity of blood, but of obtaining a full impression from the cerebral influence, in many cases of inflammatory affections, — and the error of the recommendation that "bloodletting should never be carried to actual syncope, but only to the very first signs of approaching syncope, which is, in fact, to be prevented by immediately laying the patient in the recumbent position." (2) Many similar examples will be adduced, when we come to the more practical application of bloodletting.

Where so many circumstances, therefore, are concerned in the due application of this remedy, how important is it, that it should be administered by the hand of science, and not by barbers! (3) "On its precise application its efficacy exists." (4)

Again, violent inflammations may spring up after excessive loss of blood, but with which the loss has had no connection

(1) Armstrong's Lectures on the Practice of Physic, vol. i. p. 402.

(2) Dr. Hall on Loss of Blood, pp. 176, 169, *et passim*.

It will be recollected, too, that Dr. Hall enjoins it upon us to bleed our patients in the erect posture. This we generally approve, and almost always practise; but, then, less blood is obtained before syncope comes on than in this horizontal posture, and therefore the greater may be the necessity of producing syncope. But our author has the fear of "exhaustion," "excessive reaction," "irritation," or, what is worse, absolute death. (*a*) In America, however, where we are a bleeding people, the present writer has never heard of an immediate death from the use of the lancet, — but he has known of many from the neglect, or inefficient use of it, — especially since our author's and M. Louis's works have been extensively circulated amongst us.

(3) A prevailing usage in large cities.

(4) Armstrong's Lectures, vol. i. p. 342.

(*a*) Ibid. p. 154, &c.

as an exciting cause, which we believe to be generally the case. Where will be the relation of the remedy, in these cases, "to the exigencies of the cure?" Sir B. Brodie says, "where very large quantities of blood have been already taken away, if inflammation of the brain should show itself, our resources are comparatively limited, and we are not able to meet it with that energy and vigour which the circumstances of the case require." (1) "A patient," says Dr. Armstrong, "had been once bled, after which the inflammation of the pleura and lungs returned. He had nearly expired from the bleeding; but the symptoms were so urgent that I determined to bleed him decisively, and I told his friends that he might perhaps even die under the operation. I bled him decisively, and syncope came on suddenly and continued some time, so that I thought he would have died. He recovered afterward with small doses of calomel and opium." (2)

Many of our patients perish, manifestly, for the reason alone, that the system has not the power to bear the loss of blood that would otherwise overcome the disease. We have already seen that this is not unusual in congestive fevers; and we may sometimes utterly fail, where circumstances favour a large depletion, from the ultimate failure of the system to bear the necessary loss of blood. Thus, in an extreme case, a patient was bled upwards of one hundred and sixty ounces, in three or four hours under an attack of laryngitis, besides having thirty leeches applied over the larynx; "yet he died of inflammation of the larynx in a few hours." (3)

Before concluding this section, we cannot forbear adverting, once more, to the remarkable fact, that the tolerance of loss of blood may be often promoted by the loss itself. This, indeed, is constantly true of venous congestions, and congestive fevers; and from the formidable nature of these affections, and their insidious tendency to divert us from a steady purpose of wielding the depletory treatment, to indulge the fatal delusion of "sustaining the strength" by bark and stimulants, we have been led to examine, at a weary length, those almost exclusive rules for bloodletting, by which able and eminent men completely shut

(1) Medico Chir. Trans. vol. xiv. p. 382.

(2) Op. Cit. p. 402. We have quoted this case before; but it serves as well as a new one to illustrate another principle.

(3) Armstrong, Op. Cit. p. 392

out from the pale of the healing art a large proportion of the most formidable maladies of the human race.

We have even seen one of these eminent observers exhibiting a case of cerebral inflammation, where small quantities of blood constantly led to syncope, without abating the disease; and where, at last, after one of those fruitless paroxysms, the ligature was immediately replaced, and the patient sustained "the loss of a *very large quantity* of blood before he again fainted." The ability to bear the necessary loss was finally established, and "the relief was now complete and permanent."

In all the foregoing affections, there are certain causes existing, as we have endeavoured to explain, which counteract the tendency of inflammation to sustain the organs of circulation under the loss of blood. These causes, which so baffle the treatment, will generally yield to moderate impressions from bloodletting; and it will be only then that we shall find, that what is denominated the power of the system to bear the loss of blood will become developed as in other cases.

Whilst, however, in the treatment of particular affections, a general principle as to bloodletting, &c., should prevail, the extent of its application will be constantly affected by those modifications of disease which are determined by the nature of the remote causes, sympathies of the constitution or of particular organs, age, idiosyncrasies, &c. Thence, an important rule, that it should be our unvarying object to limit the activity of all remedies within the existing force of the vital properties of the system, as well as the force of those which are more immediately concerned in the morbid process. But, however the vital properties may be altered or prostrated, it may be the effect of moderate remedies to exalt them rapidly, and to place at their disposal a vigorous treatment.

Finally, we shall advert in our eleventh section to cases of acute inflammation, in which bloodletting, beyond a moderate extent, aggravates the disease, and where, at last, it will only yield to the influence of bark. These examples, however, are not common; but they occur especially in that great class of intermittent fevers, which Drs. Hall and Wardrop have left out of consideration. Here the peculiarity of the constitutional affection may counteract the sustaining influence of inflammation under the loss of blood, — especially in particular localities. The able translator of Lænnec on Diseases of the Chest remarks, that it is

the result of M. Bailly's clinical and pathological researches among the pernicious fevers of Rome, that "there exists an absolute necessity of administering the bark in order to check the progress of the fever, even in cases complicated with the greatest visceral inflammation." (1)

All this is more or less confirmed by Morgagni, Torti, Morton, and others of their day. We are told, also, by these, and later observers, that bark, and not bloodletting, is the cure for intermittent apoplexy; in other varieties of which a most sanguinary, and indiscriminating practice is adopted.

Still we are skeptical as to the utility of so much bark, and no depletion, in the foregoing modified forms of inflammation.

Finally, it is due to Dr. Hall that we should state his declaration, that "I have heard of some recent criticisms upon this subject, (his rules;) but I have never heard one single intellectual observation in opposition to my proposal. I will venture to say that you will, in your future practice, frequently remember what I am now stating with ineffable satisfaction." (2)

SECTION X.

ON SOME OF THE INDICATIONS FOR BLOODLETTING.

We cannot forbear noticing a test for bloodletting which is indicated by Dr. Wardrop. "The leading symptom," he says, "by which the constitutional disturbance demanding venesection is indicated, will be found in the *quality of the pulse.*"

We need scarcely say, after what we have already stated, that we regard the foregoing doctrine as liable to great qualifications. There is scarcely any symptom, *per se*, that is less to be trusted than the pulse, unless it possess certain positive characters. One of these, *incompressibility*, our author defines well. *Hardness* is another characteristic which goes far towards indicating the propriety of bloodletting,—since both are commonly the re-

(1) P. 248, note.
(2) Lecture on Puerperal Diseases.

sult of inflammatory action. But the philosophical physician will be determined in all his remedies by the general assemblage of symptoms; especially local ones, if they exist. Again, the pulse is well known to be subject to the greatest variety of changes from transient causes. Celsus advises us never to feel it, till the patient has recovered from the agitation which arises from the entrance of the physician to the chamber of the sick. Most of all, however, the usual conditions of hardness and incompressibility may be wholly absent in inflammations of the most formidable character. We shall now speak especially of the incompressible pulse.

After stating that the "pulse varies according to the organ which is affected, and is even different in inflammatory affections of different textures of the same organ,—except in one particular," Dr. Wardrop proceeds to say, that this particular "is sufficiently well indicated by the term *incompressibility;* which may be considered as pathognomonic of inflammation in whatever organ or texture that inflammation exist." He then states an excellent method of ascertaining this precise condition of the pulse. "Whether the pulse be full or small, hard or *soft*, frequent or slow, if with the point of one finger the artery be pressed at the wrist, we shall perceive with another finger applied to the artery beyond, and at the distal side of the first finger, that unless a very considerable degree of pressure be employed, the pulsation of the radial artery will not be entirely destroyed; but the sensation, as if of a fine thread, or hair, will remain." (1)

We cannot think, as we have said, our author entirely correct in supposing the foregoing modification of the pulse a pathognomonic symptom of inflammation. We have certainly known it to have been wanting in many instances of severe inflammatory affections, and to have been present where there was no inflammation. Our author's rule, therefore, might betray us into an erroneous diagnosis, and malpractice. Even in some formidable cases of simple pneumonia, but especially in that variety which has been called pneumonia typhoides, the pulse may be as easily compressed as in health. Again, when an incompressible pulse has prevailed, this particular character may be wholly overcome by bloodletting, and the current of blood may be

(1) On Bloodletting, pp. 25, 26.

arrested even by a very slight pressure; and yet the disease remain unsubdued, and require for its removal a farther loss of blood. In these cases, a more uniform attendant of the pulse in inflammatory affections generally remains; and this is *hardness*. If a very slight touch with the four fingers be now made, the pulse will be felt striking the fingers, simultaneously over the whole extent, like a stick. The moment the pressure is increased, it feels soft, and a little harder pressure arrests it entirely. Yet, since we have applied Dr. Wardrop's test, we have been sometimes surprised to find the thread-like pulse where the general volume of the vessel had yielded to a moderate pressure. It is undoubtedly an improvement, as far as it goes.

But, it is in venous congestions, unattended by common inflammation, and where, especially, that stage has occurred in which the circulatory organs are prostrated, that this guide would often betray us into mistake as to the utility of bloodletting. Here, there may be still remaining *hardness*, (for we shall endeavour to show that this is a common though obscure attendant of venous congestion,) but slight pressure will often arrest the pulse. Bleeding, now, will add to the incompressibility, whilst it may diminish the hardness. In these, and in some cases of common inflammation, the pulse is only to be trusted so far as its near extinction may denote a great weight of disease, and the central accumulation of blood which may be oppressing the organs of circulation. The entire combination of symptoms must be our guide; a rule, indeed, which should always prevail whenever this great remedy is contemplated. But, in cases of venous congestion, where the stage of collapse does not take place, Dr. Wardrop's method has been useful to us, and we have often detected the incompressibility in such cases; — when, without this knowledge, we had arrived at a different conclusion as it respects the incompressibility of the pulse.

Our experience has taught us, that the existence of an incompressible pulse generally denotes the propriety of bloodletting, but not its extent. It is not a better guide, however, than hardness of pulse; and if this be combined with a buffy and contracted coat of the blood, having fimbriated edges, the hardness which may afterwards remain becomes a pretty safe criterion for farther bloodletting. It was the frequent appearance of the buffy coat

in diabetes which induced Dr. Watt to treat that disease by bloodletting.

We entirely concur with our able Dunglison, (1) that a buffy coat of the blood should never, in itself, constitute a criterion for bloodletting. Many reasons for this objection will appear as we advance with our subject. There is no one solitary guide, in relation to the proper use of this great remedy.

We think that very few will agree with Dr. Wardrop, that "there is usually no appearance of the buffy coat in blood removed from persons affected with violent inflammations until the latter stage of the disease, and at the very period when the further abstraction of blood would be pernicious. (2) On the contrary, indeed, we find in ninety-nine of one hundred such cases, that the buffy coat is presented at the first bleeding; and has disappeared, more or less, when the "further abstraction of blood would be pernicious."

Dr. W. also thinks that "a vigorous action of the heart may be regarded as a useful guide in the employment of bloodletting, and when taken into account along with the incompressible state of the pulse, will afford additional confidence in deciding on a depletive system of treatment." (3) This action of the heart is regarded in the same light by M. Lænnec (speaking of pneumonia,) even where the pulsation of the arteries is comparatively feeble. (4) Doubtless, this is generally true in either case, especially the former. But in certain diseases, where peculiar nervous influences prevail, the rule in a general sense, especially as advanced by M. Lænnec, may lead us into error. There are conditions of the constitution which require much judgement in the application of bloodletting, and where it is highly important to connect the various circumstances that may attend each individual case. The truth of this remark will be appreciated by those who are familiar with delirium tremens. Sometimes bloodletting may be important in these cases, and sometimes injurious, where there is strong action of the circulatory organs.

"If the heart and pulse are in both cases weak," (in pneumonia) says Lænnec, "the detraction of blood will almost always

(1) General Therapeutics, &c. p. 413.
(2) Op. cit. p. 41.
(3) Ibid. p. 28.
(4) On Diseases of the Chest, p. 249.

occasion complete prostration of strength." The learned translator adds, in a note, "I need not point out to the reader the high practical importance of this observation." The reverse of this has been the result of our experience, and we believe of American physicians generally. We regard a prostrated state of the circulation, in pneumonia, as imperiously demanding the lancet, — cautiously at first, but almost always decisively before the disease can be slain. Nothing else will reach this form of the disease, at least in America. Such patients would generally die in our hands without bloodletting. They are examples which demonstrate the fallacy of depending upon the pulse alone, when we are deliberating as to the treatment, or the diagnosis.

Again, says Dr. Wardrop, "in almost every case where venesection is necessary, there is present along with the disturbed action of the arterial system, some local pain, more or less severe." (1) Now this is notoriously not the case in very many instances of venous congestion, in many chronic inflammations, and often in severe cases of pneumonia; in all of which bloodletting may be indispensable.

As to the assumption of any particular symptom, or circumstance, as a rule for bloodletting, or for the application or neglect of any other active remedial agent, we hold it to be indefensible, and shall endeavour to prove it so in subsequent sections of this essay.

Celsus has a general rule in regard to the pulse in all diseases, which is still too little regarded. "We rely," he says, "most upon the pulse, although deceitful, and therefore should be trusted with caution. It beats faster or slower, according to the sex, age, and nature of our bodies." (2) So also Galen, (3) Bellinus, (4) Pechlin, (5) Tulpius, (6) Berger, (7) and many others, who agree in ascribing a greater importance to the combination of symptoms. "Nor should the physician at once feel the pulse; — first take a chair, and be cheerful." (8) And so Hippocrates. (9)

(1) P. 29.
(2) L. 3. c. 6. p. 129.
(3) L. 4. c. 1. 2, de Puls.
(4) De Puls. p. 84.
(5) Obs. Phys. Med. L. 2. Obs. 5.
(6) Observ. &c. L. 3. Obs. 45.
(7) Phys. Med. p. 73.
(8) Celsus, ut supra.
(9) De Decent. Ornatu, s. 9. 1. et seq. et passim.

They speak of the important indications of the tongue. Baglivi says it supplies the most important;(1) M. Louis the least;(2) an error which has been extensively propagated on his authority. Respiration was to Hippocrates, what the pulse is to us.

It is a rule very often urged upon us, after surrounding the remedy with every variety of precaution, to avoid the abstraction of blood where we have any doubt as to its propriety,; whilst the patient is left entirely at the mercy of cathartics, emetics, &c. A different practice has obtained with us, and we have rarely had any reason to regret it. If we have had doubt as to the utility, and especially the possible injury, of any active internal agent, we have delayed it till the indications may have become more distinct. But if, after surveying the whole aspect of a case, we remain in doubt about the propriety of abstracting blood, we generally take out our lancet and bleed the patient. We would not, however, leave this subject, without saying, that in these doubtful cases the symptoms are supposed not to preponderate against the remedy. But even here, where we yield to the demand of the symptoms, and avoid bloodletting, disease has been sometimes so latent, that, with all our decision with this remedy, we have lost patients from its delay or neglect. But however painful may be these recollections, we must still adhere to the necessity of taking nature as she may please to manifest herself to our observation, or as we may be able to interpret her.

We have long since, too, come to the conclusion, that it is safer to put this "two-edged sword" into the hands of the ignorant, or the imbecile, or those who make a trade of the profession, than to forever blunt its edges, so that it will not cut, before it be trusted to their use. We every where see victim after victim sacrificed to timid admonitions, and worse example; whilst you, and all of us know, that it is a rare phenomenon that a patient is slain, seldom injured, by the lancet. This is the test, and the stength of it we will endeavour to show hereafter.

On the other hand, is it not too often the case, that eminent and able teachers, who constantly instruct us to pause where bloodletting is indicated, observe a phlegmatic silence as to the injurious tendencies of active internal agents, or urge them upon

(1) Prac. Med. L. 2. c. 9.
(2) On Typhoid Fever, passim.

us as if they were as powerless as water? These, not bloodletting, make up the great abuses of practice. We have known some, for instance, feed the flame of scarlatina with the daily, and sometimes almost hourly, exhibition of calomel, and senna, or repeated emetics of tartarized antimony, who would look with horror upon a drop of blood to retrieve the wrong they had inflicted.

Again, there are others, who, in the pursuit of some theory that leads to many embarrassing precautions as to bloodletting, throw off the restraint where it is most required, for the sake of carrying out the doctrine. Thus, Dr. Hall, would have us bleed chlorotic females in doubtful cases, to ascertain by the favourable or injurious effect of the remedy, whether the disease depend or not upon inflammation.

SECTION XI.

IRRITATION AND INFLAMMATION FROM BLOODLETTING.

Are there not conditions which may be justly denominated "irritation," and "excessive reaction from loss of blood?" We certainly think so, and that they may be superinduced upon existing inflammation, by which its obstinacy, if not its severity, will be increased. We also think it may be shown that inflammation is sometimes excited by the excessive loss of blood, and that this, instead of simple irritation, or "excessive reaction," alone, is the usual result of an excessive evacuation of blood. True, constitutional irritation always attends the loss when it becomes the exciting cause of inflammation. It is, indeed, the foundation of the local developments. The whole condition is owing to that powerful impression which is made on the vital forces, both by the direct effect of the agent, and through the operation of the nervous influence on the heart and the instruments of vital actions. In this state, the heart, having less blood to move, and stimulated by the morbid irritability of the brain and that of the general capillary system, beats either with greater frequency or greater force. But, in all the reputed

cases of "excessive reaction" following the loss of blood, the symptoms, and morbid products, denote either the antecedent existence of inflammation, or inflammation resulting from the loss of blood, or from an antecedent predisposition. The two latter causes may act conjointly; and, as we have hitherto explained, a timid abstraction of blood is not an unusual cause of inflammations. Excessive bloodletting, on the other hand, we believe to be a rare phenomenon; but, unlike too small a loss, it will establish inflammation independently of any antecedent predisposition.

Again, irritation, and excessive action, may spring up from other causes, such as morbid conditions of the stomach, &c. But here there may be no local developements in consequence; none of the distinguishing symptoms, and none of the products of inflammation. There may be great commotion in the system; but, *cæteris paribus*, it is universally the same. The pulsation may be most sensibly felt in the head; but here the capacity of the arteries is great, and here is the ultimate seat of sensation.

The consequences from an excessive loss of blood are essentially different from the foregoing sympathies. An injury is inflicted, suddenly and severely, upon the *vires vitæ* of all parts, and the nervous influence is powerfully determined upon all. The brain suffers the impression particularly; and if "irritation," or "excessive reaction" follow, we know of no recorded instance which has not also presented the usual phenomena of inflammation, either in the brain or some other organ. If death ensue, effusions of serum or of lymph, or disorganization, &c., are the concurring results. Still another cause may have operated, and have been overlooked.

We ought to premise, however, that we are not speaking now from personal observation. We have never witnessed one of those instances in which the excessive loss of blood, *per se*, induces that condition of reaction which is described by Dr. Hall, and to which he assigns the characteristic marks of inflammation; and although we look upon most of Dr. Hall's examples as having little or no connection with the excessive loss of blood, there are cases on record where inflammation was manifestly induced, or aggravated, by an excess of this remedy, as in acute rheumatism; whilst the whole philosophy of its operation shows the possibility of such a contingency.

Simple "irritation," or "exhaustion," or "excessive reaction,"

are marked by none of the foregoing signs or consequences; unless we except effusions of serum, which we believe to be rare. The various physical results, as well as the vital signs, which have been described as contradistinguishing irritation from inflammation, when they arise from the excessive loss of blood, are exactly the characteristic symptoms of inflammation. Dr. Hall, who especially makes the foregoing distinction, concedes that "exhaustion from loss of blood is not only not incompatible with repletion and a tendency to effusion within the head, but it actually supposes that condition of the encephalon, when long protracted." (1) He also states that leeches to the head are one of the remedies. So, too, will general bloodletting relieve the symptoms, but perhaps only temporarily; since, when inflammation is aggravated or induced by loss of blood, such is the combined nature of the exciting cause and its curative effects, that the modified irritability of the vessels may readily yield, at the moment, to a farther loss, — yet it may soon obey the original principle. (2)

Mr. Travers, in contending for the existence of a pathological condition, termed irritation, states that "its symptoms are not less marked than those of inflammation or fever, neither of which approach to substitutes, for in numberless instances neither are present." (3) This is all very well. But when the symptoms of inflammation do exist, it is very proper that the disease should be called inflammation, and treated accordingly, — however modified the treatment should be by the particular causes.

Much as we may esteem the writings of Dr. Hall, we have too much enthusiasm for the genius of Hunter, to allow this opportunity to escape of applying to Dr. Hall the principle of "palmam qui meruit ferat," which he has exercised with unsparing justice towards his cotemporaries, in his late "Experimental Essay on the Circulation of the Blood."

(1) Hall on Effects of Loss of Blood, p. 164.

(2) Thence appears the reason why, when excessive bloodletting establishes, either in disease or health, a susceptible state of the system, an habitual use of the remedy may be in some rare cases almost unavoidably incurred. Thus a case is related, where a young female was bled 1020 times in nineteen years, or about once every sixth day, to cure her of hysterics. (a) But whilst its application extends no farther than that of subverting morbid irritability depending on other causes, the vital forces then returning to their natural state, no foundation is laid for periodical bloodletting.

(3) Farther Inquiry into Constitutional Irritation, p. 207.

(a) Journ. de Med. Mai, 1755.

It is remarkable that our author does not advert in his "Researches" to Mr. Hunter's opinions on the leading objects of his treatise; whilst there is a liberal quotation from others, who recognise our author's claim to originality. (1) We are sensible that "nihil dictum quod non dictum prius; methodus sola artificem ostendit." But "sumsi, non surripui" is as necessary to the effect of a new model.

We have carefully compared the 10th chapter, particularly, of Mr. Hunter's work "On the Blood," with Dr. Hall's "Researches on the Loss of Blood," and we have not been able to discover in the latter a single principle which is not either strongly implied, or distinctly stated by Mr. Hunter. The whole subject of irritation and excessive reaction from loss of blood is lucidly explained; but, it is true, with his logical and characteristic brevity. It is our main object, however, in speaking of Mr. Hunter's opinions, to exhibit the experience and inductions of a profound inquirer after truth upon an important subject; whilst we would give to every philosopher his just degree of merit. It should be, however, observed, that in Mr. Hunter's cases, and explanations, we have rather the effects of the misapplication of bloodletting, than of an excessive loss in its extreme acceptation; whilst this is almost the only useful view of the subject. Nevertheless, bloodletting misapplied, however small the quantity, is always excessive.

In respect to one of the most important views of the subject, Dr. Hall's treatise does not contain a case more directly in point, than some which are produced by Mr. Hunter to enforce a proper circumspection in regard to the use of bloodletting, of which he was a decided advocate. We may quote an instance in illustration,—and this we do more especially for its practical value, and to show, once more, that we were right in contesting Dr. Hall's rule as to the ability of the system to bear the loss of blood in all inflammations.

"The following case," he says, "is another strong instance of great action in a weak, irritable habit. A lady had a violent inflammation at the root of the tongue, so as to form a consider-

(1) One of Dr. Hall's correspondents is quoted by himself as saying, that "in this case, (one of exhaustion and reaction) the effects of loss of blood on the constitution stole almost imperceptibly upon me, and I was not aware that such symptoms as you have detailed in your work could arise." (a)

(a) Researches on the Loss of Blood, p. 29 *First edition*. Our references are generally to the second American edition.

able suppuration; with a pulse of 120, 125, and often of 130, in a minute. Her blood was extremely sizy, yet she received but little benefit from the first bleeding, although the blood coagulated pretty firmly, which indicated strength. She was of an irritable constitution, so as to receive less benefit from bleeding than another; and when bled three times, the blood became extremely loose in its texture, which bark removed, as well as the other symptoms. Upon leaving off the bark, the symptoms all recurred, and when she was bled again for the second attack, which was the fourth time, the blood, although inflammatory, had recovered a good deal of its proper firmness; but in the second bleeding, for this second attack, it was less so, and in the third it was still less. Suspecting that bleeding, in the present case would not produce resolution, I paid particular attention to the pulse at the time of bleeding, and found that in this last bleeding, the pulse increased in its frequency even in the time of bleeding; and within a few minutes after the bleeding was over, it had increased ten strokes in the minute. These bleedings retarded suppuration; but by producing irritability, they could not effect resolution."

To the foregoing case, Mr. Hunter subjoins the following note: "This, of the pulse increasing upon bleeding, is not always to be set down as a sure sign of irritation being an effect; for in a sluggish pulse, arising from too much blood, the increase of strokes, and freedom given to the circulation, is salutary; but when a pulse is already quick, an increase must arise from irritation." (1)

It will be thus seen that Mr. Hunter extended his objection to bloodletting to certain cases in which active inflammation exists, even of an important part; whilst Dr. Hall maintains unequivocally, that "in the case of inflammation, no one would think of trusting the safety of his patient to any other remedy than bloodletting." "Bloodletting is the remedy and the only remedy for inflammation." "That local inflammation always imparts to the system the power of bearing this remedy, and in proportion to the exigencies of the case," and, "of the cure" (2)— with many similar statements which we had occasion to notice in a preceding section.

Again, "if the constitution," says Mr. Hunter, "is already

(1) On the Blood, &c., p. 341.

(2) Researches on the Loss of Blood, pp. 80, 228, 170, *et passim.*

below or brought below a certain point, or gives the signs of it from the situation of the disease, then an irritable habit takes place, which is an increased disposition to act without the power to act with. This, of itself, becomes a cause of the continuance of the original disposition, and therefore will admit neither of resolution nor of suppuration, but continue in a state of inflammation; which is a much worse disease than the former." Here again, he admonishes us that bloodletting will increase irritation.

In another case, "a gentleman," says Mr. Hunter, "had one of the most violent inflammations I ever saw, in one of his eyes, attended with violent pain in his head, the blood extremely sizy, all of which denoted great action in the parts; and although he was bled pretty freely, yet he never found any relief from it." "These cases should be classed with the irritable constitution, where there is too much action with small powers, and in which bleeding should be performed with very great caution." (1)

We shall make other quotations to the same effect from Mr. Hunter, as these examples illustrate so well what we have said, hitherto, upon the physiological effects of bloodletting. "In irritable habits, where the inflammation becomes more diffused, greater caution is necessary, with regard to purging, as well as bleeding; for I observed on the subject of bleeding, that in such constitutions, no more blood should be taken than would relieve the constitution, as it were mechanically; but not such a quantity as to have a tendency towards lowering or weakening that constitution; for in such cases the action is greater than the strength; and whenever the disposition between these two is of this kind, we cannot expect any thing salutary from this mode of treatment, and therefore should not increase it. In such cases, the very reverse of the former method should often be practised; whatever has a tendency to raise the constitution should be given,—such as bark, &c." (2)

It is Mr. Hunter's opinion, indeed, that bloodletting generally increases irritability, for a time. "The means," he says, "of producing absolute weakness are bleeding and purging; but the

(1) Op. Cit. p. 341. In cases of this nature, the bloodletting is, in most instances, either too sparingly adopted, or the disease is complicated with a species of miasmatic fever which sometimes demands, for the cure of inflammation, the agency of bark in conjunction with the loss of blood.

(2) Op. Cit. pp. 345, 347.

bleeding also produces irritability for a time." "The inconvenience arising from bloodletting is, that the sound parts must nearly, in the same proportion, suffer with the inflamed; for, by bringing the inflamed part upon a par with health, the sound parts must be brought much lower, so as to be too low." "Purging will act as an auxiliary to bloodletting; for so far as irritation (from bloodletting) is a cause, this will also lessen it. The two should go hand in hand; for whenever we lessen power, we should, at the same time, lessen the disposition for action, or else we may *increase the disposition.*" "This practice should not be carried so far as to produce the sense of too much weakness, for then *the heart acts with great force, and the arteries dilate.*" "Too little blood produces debility and irritability; because there is loss of powers, with an increased action to keep up, which is not now supported. From whence we must see that bleeding can *either relieve* inflammatory action, or *increase it*, and therefore is not to be used at random." "Where there is a strong susceptibility for any one disease, in which weakness might also become a predisposing cause, I can believe that, in such cases, weakness, especially if suddenly brought on, may become an *immediate cause* of that disease." He then speaks of bloodletting as the principal agent; thus corroborating our reasoning by which we endeavoured to show that bloodletting may become a cause of inflammation. "Weakness," he continues, "produces a consciousness of its own want of powers, or incapacity, which produces increased action, that even proceeds the length of unnatural actions, called nervous." "These effects are no less visible in acute diseases in such constitutions; for they run into too violent action, which is not of a salutary kind, and therefore may be called unnatural action."

"A lady had a violent cough, tightness in respiration, loss of appetite, strong sizy blood, and the symptoms continued to the sixth bleeding, when the blood was not quite so sizy; but the most remarkable change was, its remaining flat on the surface. Upon this bleeding, all the symptoms disappeared; and here, although the blood became weak in its power of coagulation, yet it did not produce irritability in the constitution, the vessels of the inflamed part having still had power to contract. On the other hand, there may be indications for bleeding sparingly; first, *where* there is *too much action with weakened powers*," &c. (1)

(1) Op. Cit. pp. 231, 335, 336, 339.

It was Mr. Hunter's misfortune to have come upon the stage just as the prejudices against bloodletting sprang up in England, and they were not stifled till the close of his life. His philosophical mind, however, withstood them better than any other; but it is reasonable to infer, that he carried his doctrine of irritation and excessive reaction from loss of blood farther than he would have done, could he have witnessed the subsequent demonstration, as soon after manifested by a renewal of the former practice of depletion, that the climate of England, contrary to his belief, had not undergone a change that required any modification in the mode of treating inflammations and fevers by bloodletting.

We think it no injustice to Dr. Hall to say that, the foregoing extracts from Mr. Hunter's Work on the Blood embrace every principle that is contained in Dr. Hall's Treatise on the "Effects of Loss of Blood." Nor would we have said this, but that our author claims no little originality as it respects Mr. Hunter's doctrine, and is exceedingly vigilant when others overstep the limit of their own domain.

It is due also to Mr. Hunter, that we express our opinion that his doctrines are generally philosophical, and divested of the faults, as we shall still endeavour to show, that appertain to the views of Dr. Hall.

In our remarks upon the physiological effects of bloodletting, we endeavoured to show that, when the loss of blood is carried to a state of syncope, and more especially to any injurious excess, the greatest severity of its influence is sustained by the brain. Here it is, then, that we should generally look for the local injury, if any attend the reputed cases of irritation and exhaustion from an excessive loss of blood. This is exactly what we find to be stated by the late writers who have treated of this subject, — at least in a general sense.

It is enough for our purpose if only two of the examples out of twenty were actually constituted by the excessive loss of blood. The remaining eighteen, being inflammations from other causes, show, by the coincidence of symptoms, the inflammatory nature of the other two.

There is, it is true, a morbid irritability established throughout the system, — partly from the direct effect of an excessive loss of blood upon the general forces of life, and also, from a powerful determination of the nervous influence upon those

forces in every part of the organism. We believe, farther, that the general state of irritation is ultimately maintained, in most cases, by the persistence of cerebral disease, *per se*, and the unceasing operation of the nervous influence.

But it is generally in the brain alone, that any peculiar phenomena are observed; such, for instance, as are most characteristic of inflammatory action. In every other part of the system, the symptoms are uniformly alike; whilst they offer none of the foregoing peculiarities that relate to the brain. Or take another organ, — the uterus, for instance, — and here we shall find, that instead of the brain, this organ has suffered the material injury, or, perhaps, both in connection, and one or both evince the common signs of inflammation, — whilst there is nothing but an uniform manifestation of simple irritation or constitutional sympathy in all other parts.

From these facts, supposing the cases to have actually arisen from excessive loss of blood, it appears that certain organs, especially the brain, are generally more or less liable to be affected in a different way from the rest of the system; or, that what is simple irritation or reaction, in the latter, is something else in the former. It will, also, ultimately appear, from the statements of those who are opposed to the conclusion, that the local developments are constituted by inflammation.

All authors agree as to the frequency of early cerebral symptoms. Dr. Hall's highly characteristic description of the severe grades of what he calls "exhaustion with excessive reaction," strongly evince the existence of the various conditions for which we contend. "The beating of the temples," he says," "is at length accompanied by a throbbing pain of the head, and the energies and sensibilities of the brain are morbidly augmented; sometimes there is intolerance of light, but still more frequently intolerance of noise and of disturbance of any kind, requiring stillness to be strictly enjoined, the knockers to be tied, and straw to be strewed along the pavement; the sleep is agitated by fearful dreams, and the patient is liable to awake or to be awoke in a state of great hurry of mind, sometimes almost approaching to delirium; sometimes there is slight delirium, and occasionally even continued delirium; more frequently there are great noises in the head, as of singing, of crackers, of a storm, or a cataract; in some instances there are flashes of light; sometimes there is a sense of great pressure or tightness in one part or round the

head, as if the skull were pressed by an iron nail, or bound by an iron hoop." (1)

Now, the foregoing symptoms, which our author considers as denoting a state exactly opposite to that of inflammation, when they attend considerable losses of blood, are exactly such as we have found to be characteristic of cerebral inflammation when induced by other causes. But, as if to remove all doubt as to this conclusion, our author reiterates the foregoing account, and designates other phenomena not less significant of cerebral inflammation, such as "frequent delirium," "hardness of pulse," "buffy blood," &c.; (2) and to give to the subject its utmost force, he calls to his aid the opinions of Cook, Coke, Kellie, Tweedie, Hammond, Cox, and others; all of whom agree in testifying to symptoms that mark, exactly, the character of inflammation of the brain.

And then, as to our author's excellent description of the general phenomena, which he imputes to this disease that is considered so opposite to inflammation, they are, to our mind, conclusive against our author's doctrine. Where there had been bloodletting, we think that we generally recognise the occurrence of cerebral inflammation independently of the loss of blood, and often of its pre-existence; and as sincerely believe that farther bloodletting was the proper remedy for the disease. Nay more, it appears to us the inflammation was sometimes aggravated by the sparing use of the remedy. And in the cases where irritation, or excessive reaction are said to have been consequent on spontaneous hemorrhage, we think it apparent, that in most instances inflammation had already existed, or there was a strong predisposition to it.

But in all fairness we ought to exhibit one of Dr. Hall's strongest cases in illustration of what he regards as "irritation," or "excessive reaction," whichever it may be, from loss of blood. And to show that the case is considered a strong one by our author, we shall begin with his concluding remarks. "It is impossible," says our author, "to imagine a more interesting and instructive train of events. Nothing but a careful examination is wanting to make it complete as an illustration of the effects of extreme loss of blood upon the brain, lungs, and other organs of the body." We may premise, also, that we quote this

(1) Op. Cit. p. 31. (2) Pp. 54, 70, 234.

case, long as it is, which came under the treatment of Mr. Hey, that we may be instrumental, if possible, in protecting this able man against the imputation of having mistaken the nature of the disease, if not of culpable malpractice. We think, also, with Dr. Hall, that this case supplies a fair example of his own illustrations.

It is Mr. Hey's *ninth* case of puerperal fever. The subject was "a stout middle-aged woman." After delivery there was "too great an effusion of blood." Next day, June 19th, "no complaint but languor." Pulse 120. On the 20th strength improved; pulse rather more frequent; a gentle laxative.

"21st, 11 o'c. A.M. Pulse *down to* 96, *full and strong;* tongue dry in middle. 3 o'c. P.M. A slight chill, succeeded by heat, vomiting, and a continued, though not violent, pain in the abdomen, with soreness when touched; a saline mixture. 5 o'c. P.M. Pulse 112, and by no means weak; venesection to ℥ vij; large blister to abdomen. 8 o'c. P.M. Blood exhibited a very thick inflammatory crust, — crassamentum remarkably firm; pulse 130, hard; venesection ad ℥ vij. 10 o'c. P.M. Crassamentum firmer, like a piece of liver, could scarcely pierce it with my finger, buff not so thick; pulse down to 120, and more full; tongue dry in middle; loose stools; a saline draught every three hours, and laudanum 30 drops.

"22d. Throughout the day, pains slight and distant, tongue moist and clean; took nourishment sitting up in bed; pulse *below* 100 *in morning*, 116 at evening; perspires; a surprising quantity of fœces again discharged, by a dose of calomel and rhubarb given this day; decoction of cinchona ℥ j; anodyne repeated.

"23d. No pain in night; pulse 110, and very strong; clyster repeated.

"In the evening, a new and unexpected train of symptoms. Affected throughout the day with an irresistible propensity to sleep. Awoke in evening with pain in head, giddiness and ringing. Face flushed. Pulse 132, and *strong*. Took from temporal arteries three ounces of blood. *Just before the bleeding, pulse* 120; *after it*, 112. Blister to nape of neck.

"24th. Patient sitting up in bed to take nourishment; had slept well several hours in the night. Countenance good. It was rather singular that the left side of the head, from which the blood had been taken, was easy, but the opposite side pain-

ful. Crassamentum, as before, *extremely* firm. Pulse 126. Took three ounces of blood from the temporal artery of the right side, and the evacuation greatly diminished the pain.

"In the evening she experienced a seizure somewhat similar to that of the preceding day. Having been excited by several friends, who had inconsiderately talked and read a good deal to her, she was suddenly affected with a sense of great confusion and noise in the head, accompanied with much heat and flushing of the face. Pulse 140. In consequence of the relief before experienced, she was very desirous to lose some more blood from the temples; and therefore, though the pulse appeared less strong, I took an ounce and a half from the temporal artery.

"The case having become more alarming by this relapse, a consultation was requested; and a physician who had attended *several of these* melancholy cases with me, was called in. My father also visited the patient with us. The pulse came down to 120, and was evidently fuller since the bleeding. The *crassamentum was as firm* as before. Saline draught; blister to head; cold vinegar and water to temples.

"25th, 8 o'c. A.M. No sleep in night; head rather more composed; free from heat. Pulse 116. Some indications of a *paralytic* affection now apparent; faltered in speech,—tongue, when put out, drawn to one side. At noon, pulse up to 140; took little notice; and though she sometimes spoke correctly, an answer to any questions could scarcely be obtained from her. Her mind also appeared much agitated.

"At 4 o'c. P.M. The physicians met us. It was agreed that a little wine should be given frequently, and the following medicine was prescribed:

℞ Spt. æther. comp. gutt. XXX.
Spt. ammon. comp. gutt. X.
Aquæ pur.—℥ iss. M.
Fiat haustus tertia quaque hora sumendus.
At bed time, tinct. opii gut. XV.

"26th. Night again without sleep. Head free from pain, confusion, and sense of ringing. Pulse 116.

2 o'c. P.M. After three hours of comfortable sleep, the head was not so well. Bowels open; stools natural. Pulse 120.

"27th. No minutes taken.

"28th. No sleep in night: very restless; some delirium. In-

cessantly talking, but could procure no answer from her to any question that was proposed. Refused all medicine. Pulse 120.

"In the course of the day, the abdomen became tumid from flatus confined to the bowels. The tumefaction was unattended by pain or soreness; and entirely subsided as soon as evacuations were procured by an injection.

"10 o'c. P.M. In all respects worse. Urine came away involuntarily. Some rattling in her breathing, and appeared to be sinking. Pulse 132. Spt. æther. sulph. gutt. XXX. now and then.

"29th. Much better. Urine retained during night, and copious. Quite sensible, and more composed. Had regained the power of putting out her tongue. Pulse 106; tongue continued clean. A draught of infusion of roses with decoction of cinchona, and to have occasionally a little Madeira wine.

"These favourable symptoms did not long continue. In the evening the pulse had got up to 120, and the heat had increased.

"From this time, the patient became gradually weaker, her pulse was accelerated more and more, and her urine was again discharged involuntarily. She lived two days in a state of great anxiety and increasing restlessness, and died on Sunday night, the 1st of July." (1)

The foregoing case is worthy a particular examination, on account of its intrinsic merit; but especially so, as it is offered to us by Dr. Hall, "as a complete illustration of the effects of the extreme loss of blood upon the brain, lungs, and other organs of the body." It will be seen that we have supplied some interesting details relating to the first days of the case; and as Mr. Hey's comments are also suppressed, we shall add them.

"This," he says, "appears to me an instance of remarkable metastasis of the puerperal fever; and had the disease been transferred to a less vital organ than the brain, a more happy crisis would probably have been the result. I have before mentioned that, at Aberdeen, the disease was not unfrequently transferred to the surface of the body, producing *an erysipelas* of the extremities, which proved a certain sign of a salutary crisis. And the *transition of inflammatory affections* of various kinds from one part of the body to another is a fact well known in the practice of physic. In the case just related, it is observable, that,

(1) Hey's Treatise on Puerperal Fever. Case 9.

while the inflammation of the abdomen subsisted, the head was free from all complaint; and that, as soon as the inflammation was completely removed from the abdomen, to which it never in any degree returned, the head became affected with symptoms of inflammation, accompanied with evident marks of *compression* of the brain." (1)

Dr. Hall having exercised his right of putting Mr. Hey in the wrong, we have the less hesitation in endeavouring to establish Mr. Hey in the right. And this we are the more impelled to do, since by this case Dr. Hall let us into a knowledge of what he regards as irritation from "extreme loss of blood," and what he understands by symptoms that simulate inflammation, whilst the affection is affirmed to be something very different, and even opposed to that disease.

In the first place, all that we know about the loss of blood, at the time of delivery, is conveyed by Mr. Hey's expression, that "the uterus, not contracting well afterwards, occasioned too great an effusion of blood," and the patient had ultimately fainted. But this was of short duration, and may have arisen from the shock of the system occasioned by labour. It is manifest, however, from the subsequent symptoms, that this loss had not the least injurious effect upon the system; and it was not till the evening of the *fifth* day that any cerebral symptoms came on. It is also important to remark, that these symptoms supervened upon most palpable abdominal inflammation, and that the latter then disappeared. This succession of local phenomena, so remarkably characteristic, too, of inflammation, appears to us conclusive.

But Dr. Hall manifestly does not ascribe a very great importance to the uterine hemorrhage; for he only says of it, "there was hemorrhage after delivery." The "extreme loss of blood," therefore, which is supposed to have occasioned all the foregoing symptoms, must have consisted greatly of what was abstracted by Mr. Hey, the whole amount of which was twenty-one and a

(1) Dr. Hall has brought forward this case on other occasions, for the advancement of his hypothesis; and since its relation by Mr. Hey, as well as the other fatal cases, whose unfortunate issue Mr. Hey ascribed to his timid use of the lancet, was a great sacrifice of professional pride in the cause of humanity, it was due to humanity, that Mr. Hey's views should have accompanied this unexpected appropriation of his case.

half ounces, at five distinct bleedings, — or a little more than four ounces, on an average, at each evacuation.

Dr. Hall repeatedly admonishes us to beware of bloodletting in all cases where symptoms resembling inflammation spring up after the loss of blood; except, indeed, by the cautious application of leeches, — and he subsequently admits that the three ounces which were taken from the head by Mr. Hey in the foregoing case, at the last two bleedings, were beneficial. If this case, therefore, is to be taken as a standard of our author's views of "exhaustion and excessive reaction from extreme loss of blood," we think it of special importance that the case should be well examined and well considered. To us, it appears to present a most formidable array of symptoms of cerebral inflammation terminating in effusion; and had Mr. Hey promptly carried his bloodletting to a far greater extent, and avoided the bark and wine, there can be no doubt that effusion would have been prevented, and the relief would have been as complete as in those subsequent and analogous cases, in which his former unfortunate want of decision had induced him to adopt a vigorous abstraction of blood.(1)

(1) We might go far back into the records of medicine to show that neither Dr. Hall's views as to irritation from excessive loss of blood, nor our own as to its inducing inflammation, are in the least original.

When we spoke of Mr. Hunter's opinion on this subject, we ought to have said, that we have seen it stated by Zacutus, (*a*) that the constitutional injuries and local inflammations which follow the excessive loss of blood were well known to Galen and Avicenna, especially as it regards its pernicious effect on the brain.

Avicenna has many remarks, as well as Galen. The former says of bloodletting, when it is not necessary, "et phlebotomia quidem cum non est necessaria, choleram rubeam ebullire facit, et postea advenit linguæ siccitas, et quæ sibi sunt similia." "Fit causa commotionis illorum humorum," etc. (*b*) He well defines the subjects that bear, and such as will not bear bloodletting.

Kirkland says that bloodletting in excess "increases the irritability in fever," and brings on inflammation. He exemplifies his doctrine by the following case. A midwife extracted the placenta with violence, "and the woman, in consequence, was near bleeding to death. She was so pale, cold, and lifeless, that it was imagined she could not have survived half an hour." But she gradually improved; "and in a few days an anasarca came on, and yet an inflammation of the womb, (in which the pulse was quick and small,) and puerperal fever succeeded, and seemed to be the immediate cause of her death." (*c*)

So, also, Dr. Davis: "Phlegmaria dolens frequently occurs in exhausted states of the constitution, from excessive hemorrhages." (*d*) It is not improbable, that in these

(*a*) Med. Prac. Hist. l. 1. hist. 5.
(*b*) L. 1. Fen. 4. c. 20. (*c*) On Child-bed Fevers, p. 97.
(*d*) Davis, in Med. Chir. Trans. Lon. vol. 12. p. 455.

Indeed, it would have been but just to Mr. Hey, to science, and to mankind, had our author quoted a part of the next following case, which is exactly parallel with the foregoing, as to flooding and abdominal inflammation. The flooding was "copious."

"Though the attack," says Mr. Hey, "was less distinctly marked than in most of the cases which I have seen, my later experience warrants me in concluding, that the disease would have soon proved fatal, had not vigorous means been employed to check its progress. As night was approaching, I feared to wait till the symptoms became more urgent; and therefore, notwithstanding my reluctance to copious bleeding was not quite overcome, I immediately took from the arm a large basin full, (about twenty ounces) of blood," &c. The patient soon recovered, "without further complaint." "Thus," he adds, "was an immediate stop put to the disease, which, had the bleeding been omitted, or deferred till morning, would, in all probability, have been irremediable." (1)

Too much praise cannot be awarded to Mr. Hey, for the generous candour with which he admits the insufficiency of bloodletting in the *ninth* and preceding cases. He concludes the relation of the case which we have just quoted by saying that, "the next that occurred was the first of three, which were all that proved unsuccessful in my practice, out of a great number, after the change of treatment adopted in the foregoing case. And I trust I shall be able to show, that the method of cure which we now employ *had not a fair trial in any of them;* and consequently, that they *cannot be justly considered as instances of its failure.*" (2)

In case seventeen, which was of "a lady of remarkably delicate habit," the flooding was so great after labour, that Mr. Hey had "peculiar fears for the safety of his patient, in case she

cases, there had been, antecedently to the hemorrhage, a predisposition to inflammation. If the hemorrhage have been very excessive, it should be suspected as an exciting cause; and unless excessive, it should never, *per se*, restrain the lancet.

(1) Mr. Hey, ibid. Case 10.

(2) Denman makes the same admissions as to bleeding in puerperal fever. "I am now convinced," he says, "by manifold experience, that my reasoning was fallacious, and my fears groundless." (*a*)

(*a*) Introduction to Midwifery, vol. 2. p. 480. See his opposite opinions in Essays on Puerperal Fever, p. 18. Ed. 2.

should be attacked with puerperal fever." Next day, however, the same abdominal symptoms came on as in the celebrated *ninth* case, when Mr. Hey would only hazard a bleeding of fourteen ounces. This produced *syncope of two hours duration*, and "the recovery of the patient proceeded without interruption."

In case twenty-one the flooding was "copious." Next day there "*suddenly*" (1) came on the same symptoms that distinguished the abdominal affection in case nine, though less alarming. Mr. Hey immediately abstracted forty ounces of blood at 5 o'c. P.M. when the patient fainted. Indeed, she was faint when thirty-four ounces had been removed; but Mr. Hey most judiciously "put his finger on the orifice, and waited a while; when he took six ounces more. It appears, also, from the case, as we have formerly shown as to other inflammations, that these last six ounces were very important; thereby affording another demonstration of our accuracy as to what we have said of Dr. Hall's rules for bloodletting.

At half past 10 o'c. P. M. it was ascertained that "the pain returned soon after the bleeding, and with as much severity as before it." The patient had, also, much intestinal irritation, "small watery stools," and the pulse was 120, hard. But Mr. Hey, not having before him, as in case nine, and others that had unfortunately preceded it, the fear of "exhaustion and excessive reaction from extreme loss of blood," "tied up the arm, and took eight ounces from the same orifice; when, the patient growing faint, he desisted." She now became convalescent, and recovered.

Finally, whether the patient have flooded or not, after parturition, (2) when such symptoms came on as in the foregoing ninth case, Mr. Hey, on reforming his practice, adopted the following rule. "If the disease is clearly ascertained, no other consideration is of much importance. The state of the pulse affords little

(1) Dr. Hall says, that symptoms of inflammation come on gradually; those from loss of blood, suddenly. This is an important mistake as it respects the former disease, when considered in its relation to Dr. Hall's diagnosis.

(2) In cases of great flooding, "bleeding was found," says Mr. Hey, "even under these circumstances, a suitable and effectual remedy. I have related two cases, in which this happened to women of constitutions remarkably delicate, and unfavourable to evacuations; yet such are the cases in which puerperal fever has been considered most dangerous, and bleeding as peculiarly improper. We may therefore regard them as, of all others, the most decisive test of the nature of the disease, and of its appropriate remedy." (Ibid. p. 191.)

information, either as to the propriety of bleeding, or the quantity of blood proper to be taken away; and if we are deterred either by the *apparent weakness* of the patient, by the *feebleness* and *frequency of the pulse*, or by any other symptom, from *bleeding copiously*, we shall generally fail to cure the patient." (1)

We have one word more as to "case nine." It was the last in which Mr. Hey ever prescribed "bark and wine."

The manner in which he speaks of his obligations to Dr. Gordon for having dissipated from his mind the illusions of debility, and "excessive reaction from loss of blood," is beyond all praise, as we certainly think, also, is his entire work on Puerperal Fever.

But after all there is nothing very new in this *doctrine* of "excessive reaction" even as applied to the flooding of puerperal women. Denman says that "those women who have lost much blood at the time of delivery, are more liable to this disease than others, and that it is much more fatal to them." (2) But he altered his views, and bled these patients.

So, also, White; who says, "it is allowed that these fevers sometimes arise even after large uterine effusions." He then proceeds to argue according to the philosophy of Dr. Hall; "ought we, therefore, to expect to cure a disorder by bleeding which bleeding would not prevent. It is a maxim in physic, that whatever remedy will cure, will prevent a disorder;" (3) or, he should have rather said, in homœpathy.

We have thus thought it important to dwell upon Dr. Hall's "complete illustration of the effects of the extreme loss of blood upon the brain, lungs, and other organs of the body," before we came to the fundamental principles of our author upon this subject; since, in so doing, the preceding example will serve as a commentary on all that follows.

Our author, as we have said, regards the local developments which follow the loss of blood, as something wholly different from inflammation, however much the symptoms may resemble those of the latter disease. We have fully admitted, and have endeavoured to prove, that such symptoms may be brought on

(1) Op. Cit. p. 166.
(2) Denman's Essay on Puerp. Fever, p. 18. Ed. 2.
(3) White, on the Management of Preg. and Lying-in Women, p. 217.

by excessive loss of blood; but we do not admit that they are indicative of any thing but inflammation.

We fully agree with Dr. Davis, that "many diseases, loosely attributed to irritation, are often characterized by symptoms which a more accurate diagnosis would enable us at once to ascribe to actual inflammation? (1)

Our author, however, thinks otherwise.

"A state of inflammation and of sinking, and I think I might say, of reaction from exhaustion, appear to be in a certain degree incompatible with each other." (2)

"I have found that in *every case* in which *early syncope* occurs from bloodletting, the more remote effects of loss of blood, as reaction, or sinking, are also very liable to occur. There is, in every point of view, intolerance of loss of blood. The reverse of all this obtains in inflammation;" &c.

At this last quotation we must pause for a moment, calling the attention of the reader to the unqualified expressions of "every case," and "early syncope," and "in every point of view, intolerance of loss of blood;" and then refer him to what we have stated, and shall yet present, respecting that vast class of congestive fevers, in which "early syncope" is very apt to supervene on the first or second bloodletting; but in which this remedy, to a liberal extent, is commonly indispensable to life.

Again, "In doubtful cases, if little blood have flowed before syncope occurs, we must suspect *however similar the symptoms*, the case is, in fact, of a different nature,—perhaps irritation, perhaps exhaustion."

"It would be perhaps, difficult to offer any observations on the nature and causes of excessive reaction; but it is plain that the state of sinking induces a greatly impaired state of the functions of all the vital organs, and especially of the brain, *from defective stimulus*. The state of sinking may, indeed, in certain points of view, be compared with the state of the functions in *apoplexy*."

"Some of the more obvious and striking effects of loss of blood, or those of reaction, are such as to suggest the idea of *increased power* and energy of the system, and of *increased action* in *some* of its organs, and to lead to an erroneous and dan-

(1) Principles and Practice of Obstetric Medicine, p. 20.
(2) Hall on Effects of Loss of Blood, p. 238.

gerous employment or repetition of the lancet, when a *directly opposite* mode of treatment is required; while the state of actual protracted sinking frequently *resembles* a state of oppression of the brain, or of congestion of the lungs, *so accurately* as to prompt the unwary practitioner to a still more suddenly fatal use of the lancet."

"I have already stated, that the symptoms of reaction from loss of blood *accurately resemble those of power* in the system, and of *morbidly increased action* of the *encephalon*, and that from these causes the case is very apt to be mistaken, and mistreated by the further abstraction of blood." (1)

Again, in supposed irritation from other causes, "the cases resemble, in their symptoms, the *most acute forms of arachnitis, pleuritis* and *peritonitis*, but especially *arachnitis*." "These cases are very different in their nature, (from those diseases,) and especially in their reference to the effects of loss of blood."

Again, "the preceding cases are sufficient to establish the fact that there are attacks which resemble inflammation of the head, chest, or abdomen, and yet are *totally different* in their nature." In a particular case, "the patient was a stouter person, and the symptoms far more violent, and *so accurately resembled those of arachnitis* as to lead to the conclusion that that was indeed the disease." This patient was bled to ten ounces, when "complete syncope followed." She got well, though slowly, "on pursuing the usual remedies of irritation," which consist mainly of "rest" and "hyosciamus."

Upon this case our author remarks, that "these simple facts prove the existence of two kinds of cases, *similar in their symptoms, but altogether dissimilar in their nature*, and in their relation to the effects of loss of blood. Similar remarks may be made in regard to pleuritis and peritonitis, and affections resembling them, but dependent upon irritation." (2)

Now we have no doubt, as we have endeavoured to show, that our author is wrong in supposing that all cases of arachnitis bear nearly the same "relation to the effects of loss of blood," and that he has promulgated injurious doctrines as to the universal tolerance of this remedy in cases of inflammation, and the ability of the system to bear it, at all times, "in proportion to

(1) Hall, Ibid. pp. 154, 51, 11. (2) Ibid. pp. 178, 195, 213.

the exigencies of the cure." We do believe, however, that the foregoing case was one of cerebral inflammation, or of the membranes of the brain; and that early syncope was induced in part by the "intestinal irritation" that had antecedently existed. We believe it was so, because "the symptoms so accurately resembled those of arachnitis as to lead to the conclusion that that was indeed the disease." It is apparent to us, also, that the loss of the ten ounces of blood laid the foundation of the cure; and this especially as the cerebral influence was strongly obtained in the case by the "complete syncope" which followed the loss of blood. We have already shown many similar instances, where it was abundantly clear that inflammation was the pathological condition.

If our author administered hyosciamus after the bloodletting, in the foregoing case, so much the better. It was admirably calculated to quiet the constitutional irritation, and thus to carry on the subsidence of local disease. The advantages of this practice, and even of opium, in acute pneumonia, when conjoined with bloodletting, are well known, and well demonstrated by Dr. Armstrong. It argues nothing against inflammation; and even in those cases that are induced by the excessive loss of blood, or where inflammation comes on in certain chlorotic and hysterical constitutions, and some other analogous conditions, narcotic remedies, (with alteratives, &c.) by overcoming constitutional irritation, may succeed better without bloodletting than with. And here, again, we have other instances, in opposition to our author's uncompromising rule as to the ability of the system to bear the loss of blood in all inflammatory diseases.

We have variously shown, also, not only practically, but by the principles on which bloodletting operates, that it is a wholly mistaken view of the subject, to suppose that one case of arachnitis, another of pneumonia, &c., will respectively bear, in a general sense, the loss of nearly equal quantities of blood.(1) We have never seen two cases alike in this respect; nor are we ever governed by the quantity of blood taken at the first bleeding in the subsequent treatment of a case of any disease. We constantly depend upon the whole history of the case, and the symptoms which happen to be present, when our prescrip-

(1) Dr. Hall. Ibid. pp. 177, 226, etc.

"In arachnitis," says Dr. H., "an early bloodletting to syncope, also *always a large one*, is frequently efficacious."

tion is made; and we believe this to be the only successful or philosophical mode of practising the healing art. The plan of our author appears to us to be too mechanical and mathematical.

But let us not be misunderstood in regard to these reputed cases of "reaction from loss of blood," "irritation," &c., especially where symptoms arise exactly resembling those of arachnitis. In common with most others, we believe that violent headaches and throbbing of the carotids may arise from simple constitutional irritation, induced by gastric disease, or other causes. And we believe, too, that these conditions may be mistaken by the ignorant or careless for inflammation of the brain. But we farther believe, that all those "cases which resemble, in their symptoms, the most acute forms of arachnitis, pleuritis, and peritonitis;" and especially when "the symptoms *so accurately resemble* those of arachnitis, as to lead our author to the conclusion that that was indeed the disease,"—we then believe that they are indeed the diseases which they so exactly resemble. Any other conclusion appears to us to be contradicted by all experience, and all principle; and to be fraught with the highest danger. We have, in any event, in all these coincident cases, but one course to pursue,—to treat them all, at the beginning at least, upon a common principle. If, in "arachnitis," the patient faint on the loss of "ten ounces" of blood, this does not prove it was not a case of "arachnitis;" it only proves our author wrong as to the principles on which bloodletting operates; and especially wrong in his almost exclusive rules for the application of bloodletting. The differences in the quantity of blood abstracted before syncope is induced, and in the effects upon disease and upon the system, may depend, as we have endeavoured to indicate, upon a variety of causes. The manner, too, in which those causes may variously affect the operation of the remedy, we have also endeavoured to point out.

Nor do we think that a partial return of the symptoms of "arachnitis," when complicated with constitutional irritation, after syncope from loss of blood, and where the case afterwards yields to rest, hyosciamus, &c., is any proof against the existence of inflammation,—and this for the reasons just stated.

Our author has clearly made up his mind upon these questions from the exclusive view which he has taken of the effects of bloodletting, without that paramount regard to the whole con-

dition of the case which is indispensable to accurate diagnosis and to successful results.

But even in our author's cases of "irritation," and "reaction from loss of blood," we are told, notwithstanding the general exclusion of the remedy, that "it may be necessary to subdue the throbbing action of the head, by local bloodletting even; and it is *most remarkable* how small a quantity of blood being taken will relieve." (1) "In the case of exhaustion from loss of blood itself, it may still be necessary to apply the topical remedy." "Two or three leeches are sometimes sufficient; half a dozen are an ample number." (2) And again, "it is not less certain that the application of leeches to the temples, or of the cupping-glass to the back of the neck, relieves the symptoms of affection of the head, arising from loss of blood, in a remarkable manner." (3)

We have no doubt that this is sometimes ample, especially in those inflammations that are brought on by excessive hemorrhages, where the system had been antecedently in a state of health. Freer depletion will be necessary where disease, requiring the loss of blood, had preceded the injurious effect of bloodletting. The reasons for this we have already endeavoured to explain. More blood, therefore, as a general rule, must be taken in epidemic puerperal fever which may follow copious flooding, than when inflammation follows hemorrhage from a wound in a healthy system; and more, under the foregoing circumstances, in epidemic puerperal, than sporadic puerperal fever. If the system have been antecedently in a state of health, the vital forces are brought by the injurious loss of blood into a more susceptible state; and when inflammation supervenes, they still feel the influence of the remedy more sensibly. On the contrary, if inflammation have preceded the pernicious effects of bloodletting, and be either increased, or new developments established, the *vires vitæ* of the system, having been exalted, or otherwise altered so as to sustain a greater loss of blood than in health, the increase or new developments of inflammation will more fully maintain them in that condition, than a like condition will be established, in the former case, by an inflammation which grows wholly out of an injury inflicted upon the vital powers by an excessive loss of blood. In this case, the powers are artificially altered by the remedy for inflammation, when they had

(1) Op. Cit. p. 98. (2) Ibid. p. 241. (3) Dr. Hall on Puerperal Diseases.

none of the sustaining property which results in the other case from inflammation that grows out of other causes than the loss of blood.

The same principles apply, also, to cases where a strong predisposition to inflammation had existed antecedently,—as in epidemic puerperal fever. Here the vital forces of the whole system are so altered that inflammations are liable to spring up in many parts; and though excessive hemorrhage occur, it does not bring the forces out of that state of predisposition. But, allowing that it act as an existing cause of the disease, the original predisposition remains, and with it, a greater sustaining influence under a farther loss of blood, than in sporadic cases, or where a similar predisposition may not have existed.

But we have no doubt, as we have endeavoured to show in our treatise on venous congestion, that this mode of disease is present in epidemic puerperal fever, and contributes to the subsequent development of inflammation; and the existence of such congestion will at least explain the reason why the first bloodletting after excessive hemorrhage, may be more imperfectly borne than in sporadic puerperal fever.

We may also say, that whenever excessive loss of blood has been the exciting cause of inflammation in a healthy system, if bloodletting be required for its removal, it should follow, in conformity with our principles, that a moderate loss should be all that is necessary. Indeed, these cases, according to our views of the operation of leeching, will be best treated by that remedy.

So, also, more blood will be required in the ordinary forms of phrenitis, than when consequent on concussion of the brain. So, spontaneous pneumonia, peritonitis, &c., require greater sanguineous depletion than the analogous affections which supervene on injuries. In the former case, a greater persistence of disease grows out of the predisposition, the sympathies with other organs, &c.; and the general circulation is more durably maintained in consequence. We are aware that an opposite opinion is entertained by some others. M. Lisfranc, at a late concours, laid down a general principle, that "we may push our sanguineous evacuations much farther when inflammation has been produced by an external, than when it has arisen from an internal cause." But is this sustained by observation?

In many instances of phlegmasia dolens, a disease of acknowledged violent inflammation, "which arises from exhausted states

of the constitution, from hemorrhages, and long protracted fevers, general bleeding cannot be had recourse to without incurring extreme hazard." And yet the writer whom we quote, as well as Lee, Arnott, and others, considers "a dozen or a dozen and a half of leeches to the region of the affected parts, as the only remedy to be depended upon in these cases."(1) Admitting, however, that in most of the cases, general bloodletting may be practised; we shall still have some that are entirely parallel as to the effects of bloodletting, with the supposed cases simulating phrenitis, &c.

But let us again try these cases which are said by our author to resemble, exactly, phrenitis, pneumonia, &c., but which are yet "totally different in their nature;"(2) let us try them still farther by that great rule of our author which consigns to the latter class all cases which do not bear the loss of blood; and ranks in the former such as admit of bloodletting; especially where, in either instance, the characteristic symptoms of inflammation are present.

We will take a case from our author, which he had quoted for the purpose of showing that the able contributor had mistaken the diagnosis, and blundered in the treatment. He "considers it quite certain that this case was not one of inflammation, but of intestinal irritation, and of its *effects* on several organs in succession."

We quite agree with our author, who repeatedly says that if he "should fail in giving sufficient diagnostic marks of these morbid affections, it will still be of the utmost moment to know, that the distinction is absolutely essential to the adoption of an appropriate mode of treatment."(3)

It is, indeed, wholly by the foregoing considerations that we are actuated; whilst we have thought that this important subject might be best investigated by considering the facts and opinions of some able and distinguished writers which are opposed to our own. It is upon this principle that we have proceeded throughout our work, and not from any disposition to find fault.

The case which we are about to present was taken by our author from the Edinburgh Medical Journal. We shall confine ourselves to an abstract from it.

(1) Dr. Davis, in Med. Chir. Trans. Vol. 12, p. 455.
(2) Ibid. pp. 178, 195. (3) Ibid. pp. 193, 194, 196.

"Her medical attendant Mr. F. had bled her *copiously* the preceding evening, apprehending symptoms of *enteritis*."

Now agreeably to our author's own rule, inflammation should have been present to have enabled the patient to bear the "copious" abstraction of blood.

But there was nothing equivocal in the symptoms; for at the next visit, her attendant "found *enteritis* established in its *most acute* form, nor had the previous bleeding produced any mitigation of her sufferings." Again, "a vein was immediately opened, and she was bled *ad deliquium*." Calomel and other remedies were ordered.

But this was not the last "tolerance of loss of blood;" for, "next day the symptoms had remitted, but it was necessary to *repeat* the bleeding, and to apply *many leeches* to the abdomen." "Evacuations black and offensive to a degree I have seldom witnessed."

"Third day, abdomen free from pain, all the symptoms favourable, except a quickness of pulse." Had this case, however, been one of "irritation," or "exhaustion," and not of inflammation, as imagined by our critic, the symptoms should have been all increased by the large abstraction of blood.

The reader will now be forcibly reminded of the "ninth case" of Mr. Hey, when we tell him that the physician, two days afterwards, "was surprised to find *very marked symptoms of inflammation of the brain*." "The bowels had become *constipated*." The symptoms as described were strongly characteristic of phrenitis. "Tongue coated; pulse *full* and *labouring*; skin *hot*, &c." "*no pain upon pressure upon any part of the abdomen*."

But how was it now after all the foregoing "copious" bloodletting, as to the farther "tolerance of loss of blood?"

"*Copious* depletion was again had recourse to;" blister to neck; cold water to head, — calomel, &c. Another physician was in attendance, — aiding and abetting in this sanguinary practice.

The symptoms *yielded* gradually; but there still remained our author's characteristic "tolerance of loss of blood," as long as the symptoms lasted.

"At every visit, (for several days) I was obliged to bleed *ad deliquium*, or until sensible *relief* was obtained, besides applying *numerous leeches every day*." "The crassamentum was re-

markably *firm* and *fibrous*. After the second bleeding (for the phrenitis) the pain was confined to the right hemisphere." "At my fourth visit, the pain in the head was sensibly abated."

But, some eight or ten days after inflammation had supervened in the brain, and when it had been mainly subdued, we are again presented with another translation, as it is called, of inflammation to the abdomen. Bleeding was again adopted. But did it, according to our author's views, increase the symptoms "simulating inflammation?" On the contrary, "they were *conquered* by the bleeding." "The bowels, notwithstanding the *large doses* of purgatives, were with difficulty kept soluble."

The reporter of the case, who was rather a consulting physician, finally withdrew, "with sanguine hope of her permanent recovery." Does this, again, appear like "irritation," or "excessive reaction from immoderate loss of blood?"

But again we have another return of inflammation in this highly predisposed individual. "In a week after I left, I was again called to her, in consequence of a severe attack of *pleuritis*." The plan of treatment had, in the meantime, been continued "in a modified degree."

"Almost in despair," says the reporter, "I again began the evacuating system, and at length had the pleasure of subduing this last attack, but not without *repeated bleeding, leeching, cupping, and blistering*." "A few weeks in the country soon restored her to her usual appearance?"

Now, as to "intestinal *irritation*," in its ordinary acceptation, there appears to have been no such thing in the foregoing case, from its beginning to its ending; but exactly an opposite condition existed, as clearly shown by the constipation, &c. The whole treatment of the case evinced the hand of a master. "The preceding cases," says Dr. Hall, "are sufficient to establish the fact that there are attacks which *resemble inflammation* of the head, chest, or abdomen, and yet are *totally different* in their nature."

We have thus been guided by no other sentiment than that which actuated our author in his commentary, who says, "all of us, I think, have drawn similar conclusions from similar premises, and if those conclusions be in fact erroneous, it is of the utmost consequence that they should be corrected." (1)

(1) Dr. Hall, Ibid. p. 190 — 195.

To show as fully as possible, that we may be right in our criticism, we shall again offer a proof that Dr. Hall has confounded the most formidable inflammations with simple irritation; and that, in so doing, he has, by the influence of his high reputation, put in jeopardy the most vital interests of society. This we shall do, in the note below, in the language of Dr. Billing, in whose sentiments we fully agree. (1)

If Dr. Hall's supposed cases of "irritation," of "excessive reaction," &c. be of an inflammatory nature, we should expect to find them occurring at certain ages, and in certain constitutions, in middle life, amongst the most sanguine and robust; in such as are most susceptible of inflammation. Accordingly, this class

(1) "I consider the opinion wrong," says Dr. Billing, "which Dr. Hall gives in allusion to certain cases of inflammation from accidents which terminated fatally, under the treatment of other practitioners. He says that had they been real inflammation they would have borne the depletion. Now I think they would not; for I am convinced that where cases of inflammation, whether idiopathic or from accident, will not yield to bleeding within rational bounds, assisted by antiphlogistic medicines, they must terminate fatally; either by the violence of the disease, or by the unavoidable extent of the depletion.

"Dr. Hall says his medical friend (one of the cases he there alludes to, pleuritic inflammation produced by fractured ribs,) lost about eight pints of blood within four days; and yet he declares that had it been pleurisy, he might have lost twice as much with impunity and safety. I think, however, no man could lose fifteen or sixteen pints, (two gallons,) of blood in three or four days with "impunity and safety." This is what he calls establishing a distinction between irritation and inflammation; truly it is a strongly marked distinction — without a difference; for it was in fact a mixed case. With respect to the man who died of fractured ribs with wounded lung, in Bartholomew's Hospital, quoted by him, I must express an opinion contrary to that of Dr. Hall, who brings forward the case as one of irritation, in which the man died of exhaustion from a degree of bleeding which he could have borne had the case been inflammation. I cannot acknowledge the distinction Dr. Hall has here made between inflammation and irritation, as I consider inflammation of the pleura or lungs, though produced by broken ribs, still genuine inflammation." "Indeed,' he says 'cases of fractured ribs do not bear the loss of blood like those of inflammation.' Of course the mere fractured ribs do not require it;" &c.

"I cannot tolerate his perversion of the term 'diagnosis' 'by bloodletting;' for, notwithstanding the one sentence in italics placed to meet anticipated objections, that expression is calculated to puzzle, if not mislead, his junior readers. Diagnosis has always been understood to mean the distinction made between diseases for the very necessary purpose of deciding the treatment. In my opinion before such a decided step as bleeding is adopted, the physician ought to have made up his mind as to what is the nature ofthe disease." (a)

(a) Billing's First Principles of Medicine, p. 62. *Third Ed.*

of subjects is particularly indicated by Dr. Hall as being most liable to diseases of irritation and excessive reaction from loss of blood. On the contrary, "in feeble persons," he says, "and in rather advanced years, reaction after excessive loss of blood is, for the most part, defective. In this case, the patient long remains pale, thin, and feeble, and becomes faint on the slightest occasions; the pulse is frequent, but feeble, and perhaps irregular, and we look in vain for the throbbing and palpitation observed in the young and robust." (1) May it not be, however, that even in these cases, the foregoing symptoms are rather owing to some present inflammation, or to a neglect of sufficient bloodletting when inflammation had existed?

Again, as to the indications from morbid anatomy, "the next point for our consideration in the inquiry into the morbid effects of loss of blood," says our author, "will be that of the organic changes induced during the state of sinking. These are chiefly observed in the brain, in the cavities of the serous membranes, in the bronchia, in the lungs, and in the track of the alimentary canal, under the forms of *effusion*, *œdema*, and tympantis." (2)

At other times our author admits of "morbid redness," and absolute disorganization as a rapid consequence of this affection so opposite to inflammation. In the case, where "a stout muscular man" had a fall from a scaffold, and broke three of his ribs, which "were pushed into the substance of the lungs," and which our author considers an instance of "irritation," and of "sudden dissolution" from the effects of bloodletting, "the left lung sank on being placed in water; and it appeared going into a state of hepatization." (3) The treatment, by bloodletting, was the only rational practice; and if the gentlemen who last bled this patient, in a state of highly disorganizing pneumonia, ever recur to the fact, it must be with emotions of that pure happiness which never fail to attend an exercise of moral firmness, that gives to suffering humanity the last chance of relief; but the failure of which may bring down the censure of the ignorant, or the jealous, or the hypothetical.

That inflammation may be produced by an excessive loss of blood, we have endeavoured to show upon the principles which

(1) Dr. Hall in Medico. Chir. Trans. Lon. vol. 13, p. 129.
(2) On Loss of Blood, pp. 12, 49. (3) Ibid. p. 26.

govern the operation of that remedy. The remarkable instantaneousness, also, with which an excessive abstraction of blood modifies the forces of the capillary vessels, has been fully shown by Mr. Seeds; who, on bleeding animals to death, found that the *membranous tissues* presented appearances "that might be easily mistaken for inflammation." The animal living, such a state of vascular plethora, with the attendant high degree of irritability and augmented force of the circulation, must advance rapidly to absolute inflammation. But whilst the foregoing result is exactly the reverse of moderate abstractions of blood, it equally denotes the principle upon which bloodletting operates in the cure of inflammations and other diseases. The same affirmation, indeed, may be made of the inflammatory action which is established by the excessive loss of blood.

The following case shows that this new doctrine of resemblances is passing its original limit. A gentleman fell upon the ice, received a contusion upon the lumbar region, sickened, and died. His case had been treated as one of "spinal irritation,"—that comprehensive genus which appears to have taken the place of "worms." "On a post mortem examination," says Marshall, "the whole viscera were found in a healthy state, except the *kidneys*, which were gorged with blood, and several small abscesses formed in them. The spinal marrow, being found to all appearances perfectly healthy, proves," says my correspondent, "that there could be no *real* morbid action of the spinal nerves. Therefore I conceive that this forms a very curious example of *diseased kidneys simulating spinal irritation*"! (1)

Such is the absolute coincidence of our author's symptoms of irritation and those of inflammation, that we are told there is often no other test of their distinction than the treatment which is adapted to inflammation. We must bleed; and if the patient bear it well,—*well*;—if otherwise, we must then endeavour to repair the wrong. This after-knowledge,—this dependence of the diagnosis upon the effects of treatment may help the understanding; but will it be likely to help the patient, or to improve the science? And how is the treatment improved by this species of intelligence? The greatest zealot would abandon the diagnostic test, if he found it pernicious. But the patient "may be

(1) Marshall on Diseases of the Heart, Lungs, &c., p. 18.

greatly relieved by the loss of blood;"(1) "the temporary relief which follows general bloodletting may be so uniform "as to impose on the inexperienced." (2) Then we maintain the loss is useful; and if the patient be killed by the last bleeding, (3) there is no help for a practitioner of such unbridled rashness and ignorance than to send him to the penitentiary.

The rules of our author, and all others of a like nature, are captivating by their simplicity, and the exemption they bestow from all thought, or even professional acquirement. This was the secret of the popularity of Brown's Elements of Medicine. We say this in sorrow, and from a deep sense of the responsibilities of our profession.

Many have imbibed the impression, that Dr. Hall is a warm advocate for the use of the lancet. He is only so, as we have fully shown, in certain affections where few would deny the propriety of the remedy. What he has so far said with emphasis has only the effect of dissuading from the remedy, not only in the vast class of congestive fevers, and depressing inflammations, but even in many of the cases to which, on general principles, he admits its applicability.

But it is not alone by his exclusive precepts that his purpose is attempted. On more than one occasion he broadly affirms, that "it is difficult to say whether more injury has been done by an undue, or by an inefficient use of the lancet." (4) Does not the whole class of congestive diseases testify as to the inefficient use of this remedy? How constantly is it neglected in inflammations of the serous and mucous tissues of the alimentary canal, in pneumonia, in erysipelas, angina, scarlatina, measles, &c.? Is not a neglect of it consigning to the grave incalculable numbers affected with what are called "malignant," or "putrid," or "adynamic," fevers? And when is it adopted in excess? In cases of "irritation,"—affections of such comparatively rare occurrence, that, according to our author, "the profession generally appear to be totally unacquainted with them." (5)

We are, therefore, more inclined to the opinion of Botalli, that "one hundred thousand men perish from the want of bloodletting, or from its not being timely employed, where one

(1) On Loss of Blood, pp. 11, 66.
(2) Hall in Med. Chir. Trans, vol. 13, p. 140.
(3) On Loss of Blood, &c. p. 12.
(4) Ibid. p. 153. &c. (5) Ibid. p. 178.

perishes from excessive bleeding, when prescribed by a *physician.*" (1)

But allowing, that amongst our author's cases, there are some examples of simple irritation, or of mere exhaustion from loss of blood; these have been incident upon a loss sustained under circumstances of health, and must, therefore, be admitted to have no bearing upon cases where bloodletting may have been demanded, and to be worse than useless for illustrating the effects of bloodletting as a remedy. As well might we say, that cathartics shall not be given in disease, or only so with fear and trembling, because they may be pernicious in health. And he, being well, who should physic himself in order to be better, would be mad indeed, should he attempt to remove the evils of his mistake by swallowing one dose after another. Just so it is, in respect to bloodletting, or accidental hemorrhage, in health. What would cure a sick man, might develope disease in those that are well. If inflammation follow in the latter instance, it will be important to ascertain whether excessive bloodletting have been the exciting cause, or whether it did not spring from a previous disposition to the disease. The former case is rare, as known by the accidental and profuse hemorrhages that are daily occurring. Nor is simple irritation a result, the injury consisting mainly in debility. If inflammation, however, be actually induced, the remedies must be applied with circumspection.

But in very numerous cases, inflammation follows the loss of blood from a pre-existing tendency to the disease; or, more frequently, from a morbid condition already established, but not fully developed. Thus, it is not an unusual event, that the physician bleeds for pain in the head, or a stitch in the side, or for some uneasiness of breathing. The abstraction of blood is judiciously moderate. But there has been an accumulating tendency to inflammation; and the blood thus abstracted proves not to have been commensurate with the demands of the case. It releases and gives force to the general circulation, and increases the irritability of the extreme vessels. Phrenitis, pneumonia, or pleuritis, is the consequence. The physician is alarmed by the unexpected event; yet so like inflammation are the consecutive symptoms, that he ventures upon the lancet for their relief.

(1) Botallus, de Cur. per. Sang. Miss. c. 36.

But they had followed upon the loss of blood, and his decision is restrained. He therefore stops at the very point of mischief, and adds another impulse to disease. He may yet bleed again and again, as the malady resists all other agents; but the same caution prevails, and the evil increases at every partial outlet of blood.

These cases accumulate rapidly upon the hands of the unskilful or timid. Records are examined, and parallel examples are found to abound. Dissections are made, and found to coincide with the common results of inflammation. The conclusion, therefore, comes up, that there is a disease "exactly like inflammation, but totally different from it."

Just so it is with childbed women. There is often a great tendency to local inflammations, — and these may be the more speedily developed by moderate flooding, — especially if there have been previous venous congestion, as is very frequently the case. Dr. Hall has many of these examples, and were they really cases of simple irritation or simple exhaustion from loss of blood, they could not be adduced to illustrate the effects of bloodletting in disease. They should then form a class by themselves, — designated as cases of the morbific effects of "excessive loss of blood" upon the healthy system.

It is a remarkable fact, that the effects described by our author as incident to lying-in women rarely supervene upon excessive flooding; thus showing that in the cases of disease the affection already existed, or was about taking place.

The more we interrogate nature as to the loss of blood, the more we shall find her proclaiming that this is her expedient, beyond any other, by which she attempts the removal of fearful diseases. Hæmoptysis, hæmatemesis, the hemorrhoidal flux, intestinal hemorrhage, are all instituted for this purpose. We constantly witness the spontaneous effort where the vital forces are so prostrated that art looks on with dread and amazement; and what nature had thus wisely begun is often declared to be the effect of a putrid disruption of the living body, and calls for every counteracting means. Fortunately, these means sometimes consist of the lancet and depletive system. But this, with many, is only where there is vigorous strength, and where it is feared that unrestrained nature may possibly reduce it. Examples of this kind are common in pulmonary hemorrhage;

and, although in these instances the blood be taken with a view of astringing a suspected rupture of a blood-vessel, — the induction, nevertheless, does not affect the true philosophy of the case; and where nature may be too "sparing of the vital fluid" to overcome the real condition of the lungs, a singular illustration will be obtained of a co-operation of the means, towards a salutary result, which may have been employed to defeat the curative effort of nature.

But error, as it appears to us, is often committed in the latter cases. If hemorrhage be *profuse*, it should be allowed to go on, within the limit of safety, — since the depletion proceeds from the instruments of disease. A rapid abstraction of blood from the arm, superadded to the hemorrhage, may arrest the spontaneous discharge too speedily; whilst that which is artificially taken, should have been left to the natural process. And again, where the spontaneous discharge is small, the lancet may be imperatively demanded; whilst it is employed with greater caution than in the former cases.

If we regard, therefore, the effects of bloodletting where it is required as a remedy for disease, and look far back into the annals of experience, we shall find that it has rarely produced the consequences which have attended the accidental hemorrhages of the healthy system, or the sanguinary experiments upon healthy dogs, — a condition of circumstances as wide from the sick man, as health and the constitution of dogs can distinguish them. (1)

If we contrast even the scanty cases of injury from uterine hemorrhage, and other accidental losses of blood that may be sustained in health, (or try our best at the records of excessive bloodletting, as preserved by the most watchful Brunonian,) with the terrible and wide spread effects of procrastination, or timidity, in the use of the remedy where it has been demanded by disease, and more than all, with "the bark and wine treatment," we shall have little to fear from the possible abuses of the lancet. A few may be rash from ignorance, — perhaps from the encouragement of others; but will not this encouragement stimulate a host to lay aside their fears, and to moderate their Brunonian

(1) Mr. Piorry says, that "syncope cannot be produced in dogs without the loss of an enormous quantity of blood; (lorsque la saignée est énorme." (a)

(a) Collection des Mémoires, p. 227.

practice? Where then, according to the "numerical method," will be the balance?

Where conditions of disease are comparatively mild, their mildness will naturally restrain every practitioner; and when existing in severity, there will be little or nothing to fear from the liberal abstraction of blood, so long as the symptoms resist this principal remedy, and its proper auxiliaries. At most, there can be only now and then a disastrous result; whilst timid caution has its myriads of victims. Defective judgement there must always be, — and it is better, therefore, that it should lean to the side of safety. If going wrong, the error, in respect to *excess* of bloodletting, will be very soon discovered. The timidity of man needs no encouragement, when the question relates to "debility," and "the precious fluid." But come to cathartics and emetics, he is bold and indiscriminate. Here is opened an inexhaustible field of inquiry, — far more abstruse and difficult than the management of bloodletting. You may bleed in intestinal inflammation, perhaps to a vast extent, and speedily surmount the disease; when, had an irritating cathartic been exhibited, the scale might have been as speedily turned in the other direction. Who has not lamented his persevering use of cathartics in *scarlatina*, — or who cannot advert to cases in which he has not equally regretted his neglect of the lancet?

Here, books of protestation against abuse would come with a benign effect. There is nothing in the whole treatment of disease that requires a more careful analysis than cathartics alone; for, where one is now injured by an excessive loss of blood, ten thousand are destroyed by their indiscriminate application. And when we add to the excessive, or ill directed use of purgatives, the fearless manner in which the whole *materia medica* is poured into the stomach, who will not say that bloodletting has had more than its fair proportion of censure?

It appears to us that much of the antipathy to bloodletting has grown out of an illusion, natural to the fears of man. It is not wholly predicated of debility; for we constantly meet with admonitions against its use in high inflammations, which are not remarkable for their prostrating effect. But there is nothing more deeply implanted, than the knowledge of the immediate importance of the "vital fluid" to the life of every animal; and this conviction has been farther roused into operation by citing the authority of Holy Writ, that "in the blood is the life thereof."

We are dead in a few seconds from the division of a large artery; and we see scarcely a difference in the rapidity of the result when this method, or a division of the medulla oblongata, is employed for the destruction of life. Hence, many come to associate bloodletting, as practised for the relief of disease, with the extreme method of effecting death. We shall not dwell upon this want of philosophy, nor indicate farther the various causes which modify the necessities of the system in relation to its ordinary supply of blood, or how it sustains the privation by the same contingent influences. And yet the objectors to bloodletting in disease have been in the constant habit of depleting pregnant women, and others dying of apoplexy, or from drinking cold water, or from the shock of a fall, where there may have been no other inducement for the practice than a capricious desire of the subject, or the prejudice of society.

We had prepared some remarks on the controverted subject of "irritable uterus;" but our limits do not allow their insertion. We believe the disease to be inflammatory. Dr. Channing, who is opposed to us upon this question, remarks well, that "we have only to look to one great cause in our philosophizing, which is in conformity with one of the best established principles of philosophy, and may at once arrange all contingent phenomena under natural classes. In place, then, of multiplied treatises on particular and novel affections, our generalizations will be at the same time short, simple, and practical. The authority of fashion will cease to embarrass the profession in this regard, at least,—and doctrines once wisely established will not be lost sight of in the supremacy of some ingenious novelty." (1)

But who has departed from the plain laws of analogy? Who has disregarded "the doctrines once wisely established?" Who "has lost sight of them in the supremacy of some ingenious novelty?" Surely not those who contend for the inflammatory nature of the affections of which we are speaking. We reduce them all to one general class. We recognise among them a strong family resemblance. We erect no new hypothesis on their discrepancies; but reconcile ambiguities, and even what some may regard as apparent exceptions, by an appeal to a general principle, that there are no great laws that are not embar-

(1) Prof. Channing on Irritable Uterus, p. 7. Boston, 1837.

rassed by some contradictory phenomena, especially in their relation to the animal system. We believe that "all practical researches ought to be built on an induction of facts, and that single objects or events are of little value but in so far as they stand related to others; and when numerous objects and events present themselves in uniform combination, it is only by varying them and comparing them with others, that useful inferences can be drawn from them, and that the relation of cause and effect can be distinguished from casual coincidence or simple succession. Those physical agencies, on the discovery of which all practical knowledge is built, and those analogies in which all suggestions and rational conjectures originate, can only be ascertained by an enlarged view of nature, which, by enabling us both to elicit new truths, and to adapt means to ends, may be considered as at once the instruments and the light by which we work." (1)

"Subjectum istud medicinæ (corpus nimirum humanum) ex omnibus, quæ natura procreavit, est maxime capax remedii, sed vicissim illud remedium maxime est obnoxium errori. Eadem namque subjecti subtilitas et veritas, ut magnam medendi facultatem præbet, sic magnam etiam aberrandi facultatem." (2)

The records of medicine abound with astonishing examples of extraordinary hemorrhage, instituted by nature for the relief of disease; and art has not been deficient in illustrating the great law of the living system which enables it to bear excessive losses of blood under many of its inflammatory conditions. When we come to the consideration of the practical application of bloodletting, we shall see an irresistible weight of evidence to this effect.

Lancisi relates the case of a septuagenarian, who suddenly lost from his nose eleven pounds of blood, and four more in fifteen days after, without even any sensible loss of strength. (3) Here was a threatened attack of apoplexy, and the cerebral influence, which was determined by the congestion of the brain upon the vital forces of the system, sustained them under this immense loss of blood. J. Cheyne states, that "a person, who

(1) Sir G. Blane, in Med. Chir. Trans. vol. iv. p. 89.
(2) Bacon de Argumentis Scientiarum, l. 4, c. 2.
(3) Lancisi de Sub. Mort. l. 2, c. 5, Sec. 8.

had every symptom of impending apoplexy, was twice relieved by hemorrhage from the nose to the amount of several quarts." (1) And so of art. "Dr. Physick drew ninety ounces, by weight, from Dr. Dewees, in a sudden attack of the apoplectic state of fever, at one bleeding, and thereby speedily so restored him to health, that he was able to attend to his business in three days afterwards. (2)

Mr. Hunter "has seen several quarts of blood thrown up from the stomach in a few hours, even by a very thin puny person. (3) We have witnessed the same from the lungs as well as the stomach,—breaking up formidable congestions and chronic inflammations which had existed for years. This experience is not uncommon in the profession.

Schenck records cases of hemorrhage in which twenty, twenty-five, and forty pounds were lost in a short space of time without having endangered life. (4)

In the Miscellanea Curiosa is the case of a woman who repeatedly lost, for years, great quantities of blood; and, at last, a most profuse hemorrhage reduced her blood almost to the state of serum,—"ita ut non amplius sanguis, sed serum." (5)

In another instance of excessive uterine hemorrhage, occurring in a female at seventy-two years of age, who was threatened with apoplexy, the writer remarks, "admiranda hic erat naturæ sagacitas in conservando individuo." (6)

Bartholin records an instance of forty-eight pounds, within a few days; (7) and a case occurs in the Acta Nov. Acad. Philex Brixiæ, of a young man twenty-five years, who lost seventy-five pounds in the space of ten days. But we put the quantity down one half. He had been previously sick; but after the hemorrhage, "vivit ab omni corporis ægritudine immunis." (8) Similar cases are related by Rhodius, (9) Littre, &c.

Boerhaäve says "he has known almost the entire blood of the body to have been lost by hemorrhage, and the subject recover." (10)

Haller relates many examples similar to the foregoing; in one

(1) Cases in Apoplexy, p. 60.
(2) Rush's Inquiries, &c., vol. iv. p. 337.
(3) On the Blood, &c. p. 70.
(4) Obs. Med. Rar. L. 1 and 4.
(5) An. 3, Obs. 143.
(6) Miscell. Curios. An. 1678, p. 365.
(7) Anat. ren. L. 3. c. 6.
(8) An. 1687, no. 43.
(9) Obs. Anatom. Med. Cent. 1. Obs. 90.
(10) De Morbis Nervorum.

of which one hundred and twenty-five ounces were lost at each menstruation, for several years; besides a daily abstraction of blood from the arm for fourteen months. Whilst such practice cannot be tolerated, it nevertheless illustrates our principles. In another case, a thousand pounds were lost in one year; and in another, there was a hemorrhoidal flux of five pounds daily, for sixty-two consecutive days, — or a total of three hundred and ten pounds; being, probably, twice the weight of the whole body. (1) One man lost one hundred and ninety-two ounces from his stomach in a single night, and another three hundred and fifty-two ounces from the lungs in the same time. (2) Haller, himself, lost one hundred and twenty-eight ounces of blood within twenty-four hours. (3)

Moriceau relates two cases of pregnant females, one of whom was bled forty-eight times; the other ninety times, — "et qui accouchèrent heureusement." (4)

Bush has collected several extraordinary cases from piles, which terminated favourably. (5) Similar facts might be cited from other modern observers; but we have referred to past experience, to show that they have been always common.

"Ex his, itaque, clarrissime perspicimus, venæ sectionem maximum fundamentum in *ipsa natura* habere, cujus *ars* tantummodo est imitatrix." (6) "*Honest* Brunonians," says Dr. Beddoes, "have of late minutely recorded cases, to them incomprehensible, where immense discharges of blood have suddenly stopped protracted fever, and left the patient improved in strength." (7)

(1) Mr. Hunter says, "one is to calculate how much blood may be in an animal from the quantity it will lose in a short time."

"Bleedings," says M. Piorry, "equalling the thirtieth or fortieth part of the weight of the body may be repeated many times, although the animal (dogs) be upon low diet. We may thus draw successively to the tenth or eighth of the total weight.

"If some more nourishing food be allowed, bleeding may be carried to a much greater extent. If the animal continue to eat and to drink, his weight varies but little, notwithstanding the evacuations of blood. (a)

(2) Element. Physiol. T. 2. p. 4.

(3) Zimmerman on Experience in Med. vol. i. p. 304.

(4) Obs. sur les Malad. des femmes grosses.

(5) On Diseases of the Rectum, &c. *note.*

(6) F. Hoffman, De Mag. Ven. Sect. T. 3. p. 265.

(7) Beddoes, on Fever and Inflammation, p. 189.

(a) Collection des Mémoires, p. 225.

"There seems always," says Dr. Wardrop, "to be a disposition, in patients as well as in medical men, to economize blood," (1) — a fault from which, as we have seen, the doctor is not altogether free.

Independently of the general precepts which we have examined, there are a multitude of authors who advocate bloodletting in most inflammations, but restrict it in all to small and repeated operations. The quantity, therefore, being constantly short of what is necessary to change the action in the instruments of disease, does but confirm the fears of the timid, and lays the foundation of new hypotheses and the most mischievous errors.

To the English nation the whole world continues to look for enlightened instruction. It has been long in the habit of receiving from her the best gifts of science. But, as dependence is naturally attended by confidence, errors may at last come to exert the influence of truth. Sydenham has done more for America, than he did for England; but we have reason to fear that the trans-atlantic disciples of Hall, of Graves, of Louis, and other able and eminent men, are raising up followers in this hemisphere. Brown had no quarter shown him in America. The arrival of the great work of Armstrong found our physicians familiar with the advantages and the necessity of bloodletting in congestive typhus, and all analogous affections. We looked upon the practice of Broussais as inadequate to, at least, our diseases; and where he has prescribed leeches, we had first employed the lancet, and leeches afterwards.

Such being the prevailing habits of American physicians, they are so far qualified to offer an opinion on the merits of the great question in respect to bloodletting which now occupies so many enlightened minds in the country of Sydenham.

In respect, however, to certain diseases, the practice of some American physicians has been more fluctuating than can be justified by any modifications that may arise from atmospheric influence or other causes. This is particularly true of yellow fever, — bloodletting, at one time, being the principal remedy, and tonics at another. But this has been far from being general, — the faculty, for the most part, adhering more or less to the depletive system. This is partly owing to ancient prejudices, which

(1) On Bloodletting, p. 24.

are thus accounted for by Dr. Rush; although the period to which he alludes was not of long duration.

"The English physicians," he says, "forbid bloodletting in most fevers, and substitute sweating in the room of it. Here, then, we discover the source of the former prejudices and errors of our countrymen, upon the subject of bloodletting. They are of British origin. They have been inculcated in British universities and in British books; and they accord as ill with our climate and state of society, as the Dutch foot-stoves did with the temperate climate of the Cape of Good Hope." (1) Such was not less "our pride," than "to indulge in the fashions and manufactures of Great Britain." (2) The generous influence of old recollections remains. The medical literature of Great Britain is read with increasing avidity. We look to her schools for the best lights of science. This is, also, natural and just; — for what might philosophy have been without her Bacons, her Newtons, and Halleys, and Herschels, her Lockes and her Reeds, her Harveys, and Hunters, and Sydenhams, her Daltons, and Davys, and Farridays? Whilst, therefore, we are ever paying the tribute of admiration and gratitude to the lofty genius of our ancestor, let us not forget that we should at least inherit her independence; and whilst, also, we consecrate her as the parent of modern improvements in philosophy, which, in many of its branches, "like the fabled Minerva, seemed to spring at once into perfect proportions," let us not mistake the aberrations of age for the suggestions of a vigorous mind.

We have said that Dr. Rush should have limited his strictures upon Great Britain to a special era. We are told by Mr. Hunter, that "it is now seldom that in fevers physicians are obliged to have recourse to the lancet, at least to that excess which is described by authors in former times; — that they are now more obliged to have recourse to cordials than evacuations." (3) So, also, Robert Jackson. "The opinion that the cause of typhus fever was of a sedative nature led to the use of stimulants. The practice prevailed almost universally in Great Britain prior to the close of the last century." (4) These statements are confirm-

(1) Medical Inquiries, &c. vol. 4. p. 359.
(2) Franklin's Examination before the British House of Commons.
(3) Hunter on the Blood, &c. p. 227.
(4) On Contagious Fever, p. 222.

ed by Beddoes, Armstrong, and others. This change, however, from bloodletting to tonics appears to have been abrupt, and of short duration. Even Mr. Hunter "remembered when practitioners uniformly bled in putrid fevers." (1) During the sixteenth, seventeenth, and a part of the eighteenth century, almost all writers of any renown depended on bloodletting in the plague, as the "remedium principale."

Since Mr. Hunter's day, the ancient practice has been extensively revived;—bloodletting had become again the remedy for "Fothergill's sore throat," the erysipelas, typhus fever, &c. "Abstraction of blood in contagious fever," says Dr. Jackson, "which, but a few years since, was viewed with abhorrence, even branded with the epithet of murder, is now considered as the main engine in successful treatment." (2)

But again there are portents of another and a greater revolution, — beginning simultaneously in the two great empires of medical science. "Debility" is not only again becoming the watch-word for diseases of "a putrescent tendency," but we are told that "we must husband the strength and spare the lancet in acute pneumonia." How far all this may be owing to the spirit in which pathological anatomy is now cultivated, and to the reviving genius of the humoral pathology, we have considered in other places.

Whatever may be the causes for such vacillations in practice, it is important to ascertain them. Should it result that they have a real foundation in the instability of vital laws, or the influence of climate and of seasons, it appears to us that we must abandon all principles in medicine. New diseases may spring up; but when the old ones recur, it must be in consequence of nearly similar constitutions of the air; and although they may vary in respect to the intensity and complexities of morbid action, and require corresponding degrees of remedial agents, — nevertheless, the pathology is the same, and the same general principles of treatment should prevail in any given disease.

Disease, however, as we have said, may be so modified by climate, that a milder treatment may be required in one than in another. This influence of climate, therefore, renders it indispensable that Americans should have an experience of their own. Much of our country is still a forest, and the first stumps are

(1) Ut supra. (2) On Contagious Fever, p. 232.

scarcely cleared away. It is every where covered with the vegetable soil which has been accumulating since the flood, and the primitive marshes remain without change. Long will this be the condition of our western territory. These, then, alone are causes which must satisfy every impartial mind that our diseases are characterized by the greatest intensity of the pathological cause, and demand a corresponding treatment. The English or the French practitioner, who may seek a livelihood upon our soil, must come prepared for a new scene, if their own have so changed since morbid anatomy became the basis of practice, and disposed to listen to the suggestions of a new experience. And yet, if we may credit the writings of Sydenham, and Pringle, and Jackson, and of the French, and Germans, and Italians, of the 17th and 18th centuries, and some other Europeans of a later day, the difference in treatment should be scarcely commensurate with the distance of places. But certain it is, if the lancet were sparingly used in our congestive and inflammatory diseases, our grave-yards would be filled with the monuments of ignorance. "Vivo et scribo in aëre" Americano.

SECTION XII.

PRACTICAL APPLICATION OF BLOODLETTING.

Nor does it appear to us that the general precaution laid down by the distinguished Liston is more defensible than the precepts which we have examined in the preceding section,—namely, that "every practitioner ought to think twice of the probable and possible effects in every case of disease or injury, before he determines upon and proceeds to open a vein for the purpose of draining off the vital fluid."(1)

We will not dispute the point with this able surgeon where he advises us in relation to the subjects of his peculiar province. Indeed, we are very sensible that there have been many victims to an indiscriminate practice of bloodletting after severe injuries;

(1) Practical Surgery, p. 156.

and it is therefore natural, perhaps, that they who are accustomed to witness these results, and, moreover are too much employed about them, and the department to which they belong, to grapple with the profound art of medicine,—it is natural, we say, that such, however able as surgeons, should "think twice of the probable and possible effects in every case of *disease*, before they determine upon, and proceed to open a vein for the purpose of draining off the vital fluid."

But shall physicians deliberate when inflammation is careering in the great organs of life? Can there be a question of the applicability of bloodletting to phrenitis, pleuritis, peritonitis, pneumonia, and to many other grave inflammations, under their ordinary circumstances? It is true, we have lately seen practitioners boasting of their success without having "ever wetted a lancet." (1) But we do not believe that this exclusive practice has many open advocates; and to admit its imputed results would be to renounce the dictates of our own observation, and of rational philosophy.

A more limited opposition, however, to bloodletting in grave inflammations is making an inroad upon former experience; nor is it the least remarkable circumstance that it enlists the most able disciples of the anatomical school. And although they may often admit the utility of the remedy in a general sense; when they come to its practical application to particular diseases, we are told that it is either useless, or prejudicial.

In considering more specifically the advantages of bloodletting, we shall limit ourselves mainly to its effects in the diversified aspects of pneumonia, as deduced from the best observation. If it be found inefficient or detrimental in the simple or complex forms of this disease, we may rationally conclude, that the remedy can be rarely demanded by any other. But if, on the other hand, it prove indispensable in the most prostrating conditions of pneumonic inflammation, a general principle as to the utility of this agent in all analogous affections will have been sufficiently established.

We shall see that M. Louis, and his followers, have little or no confidence in the curative effects of bloodletting in pneumonia, and some other equally severe inflammations. Laennec also adopts the practice of Rasori,—urging our chief de-

(1) See, particularly, Dr. Dickson on the Fallacy of the Art of Physic, p. 53. 1836.

pendence on large doses of tartarized antimony, (1) and that we should seldom carry the abstraction of blood beyond eight or sixteen ounces, in pneumonic inflammation. He "has sometimes effected very rapid cures of intense peripneumonies without bleeding at all." (2)

The tartar-emetic treatment is thus recommended by Dr. Graves: "We can cure," he says, "a pneumonic attack by moderate bleeding, and without injuriously weakening the strength of the patient." (3) Dr. Stokes is also "convinced, that in general, a single, or at most, two bleedings will be sufficient." "General bloodletting is not to be considered the chief means of removing the disease." "In the typhoid form, the best practice is to use *wine* in conjunction with *local* bleedings." "General bloodletting is to be used with extreme caution, and the vital forces are to be carefully supported." Hence the reason is obvious, why such frequent opportunities have occurred for adopting the advice "of applying leeches or scarifications day after day, and supporting the patient's strength by food, and even wine, if necessary." (4)

Dr. Stokes remarks farther, that "in two instances only has he seen pneumonia cut short by bleeding." This admission appears to us conclusive against the doctrine of "saving the vital fluid," and especially that of "supporting the vital forces by wine." But if more were necessary, we might add, that in these United States, where the best practitioners do not sheathe the [illegible] till the inflammation is overcome, we have constant opportunities of seeing the disease completely removed within twenty-four hours after its invasion,—in infancy, even earlier. (5)

(1) The "triumphal car of antimony" has been lately mounted as if it had never been driven before. Hartman pronounced antimony a specific for all fevers, except the hectic.

(2) On Diseases of the Chest, p. 255.

(3) Lon. Med. and Surg. Journ. vol. iii. p. 103.

(4) Dr. Stokes on Diseases of the Chest, pp. 226, 227. 231.

(5) Tartarized antimony has been long and almost universally employed with us as a principal auxiliary to bloodletting; but a repetition of the latter remedy to a third or fourth time, in quick succession, and often to a large extent, has been the successful practice of Americans in a majority of cases. Others have yielded to a milder treatment; so that we do not regard pneumonia, in private practice, as a difficult disease. We have only seen fainting produced by small abstractions of blood, when the inflammation has been complicated with idiopathic fever of a depressing nature, or with venous congestions of some portion of the lungs or of the liver,—the pulse being small and oppressed under these circumstances. And then we have almost

But Dr. Stokes has no fear of bloodletting in apoplexy. "There is no disease," he says, "in which the efficacy of free depletion is more remarkable than in apoplexy."(1) Can this be so? Does it not wholly fail in all but an extremely small minority of the cases? Is not the death of patients, in apoplexy, often owing to the indiscriminate use of "free and bold depletion?"

Dr. Osborne remarks, that "since what has been termed the tartar-emetic treatment has been introduced into this country, and the practice of bleeding has consequently been to some degree discouraged, it appears to me that the advanced stages and fatal terminations of pneumonia have been frequent; and in this judgement I am confirmed by records on the large scale."(2) And thus, Dr. Armstrong, on the "antimonial treatment" of pneumonia: "The testimony is so complicated, that I can derive but little information from it; but, upon the whole, the practice seems to have been unsuccessful."(3)

In severe inflammations and fevers, it was an aphorism with Gregory, that "the danger of a large bleeding is less than the danger of the disease."

It was also the opinion of Dr. Beddoes, that "numerous facts show that early in high inflammations, the lancet can scarce be used too freely. With gangrene, infarction, or even abscess, in prospect, transient syncope is a slight evil. The rule, that the constitution in such cases *recovers more kindly from debility by bloodletting than by disease*, affords great encouragement."(4)

But we may go farther back for aphorisms of the foregoing nature. Thus Hippocrates: "ad extremos morbos extrema re-

invariably observed that the force of the general circulation has risen soon after; the vital powers becoming more exalted, the inflammatory symptoms more developed, and bloodletting soon borne to a surprising extent.

As to tartarized antimony, the irritability of the stomach in this country does not yield to the influence of increased doses to the extent which is said to have been observed in the other hemisphere. It is rarely diminished by an emetic effect; but, on the contrary, gastritis is then apt to spring up. Our stomachs will rarely bear beyond the ½ or ¼ of a grain; especially south of 40°. Hillary denounced it as "murderous," in many diseases. (*a*)

(1) Lectures on the Theory and Prac. of Med. p. 265.

(2) On Dropsical Diseases, p. 78.

(3) Lectures on Acute and Chronic Diseases, vol. i. p. 406.

(4) Researches on Fever and Inflammation, p. 97.

(*a*) Inquiry into the Method of Improving Medical Knowledge, p. 223.

media exquisite optima," (1)—in which we shall find Aretæus, (2 and the most distinguished ancients fully agreeing,—especially as it regards bloodletting.

During the enlightened ages of the world, these precepts were commonly observed, and gathered strength as experience accumulated. If, however, some adventurous innovator, like Brown, now and then threw the world "from its discretion," this triumph of prejudice soon yielded to its own calamities.

We look back with no little astonishment at the practical advances of our art at the era of Hippocrates, and even at the stability which it maintained through many succeeding ages, when so little was known of natural, and, especially, of morbid anatomy. Debility was not then regarded as an early attendant of disease; but was supposed to be only incident to its protracted stages. Hence, as we shall see, the fathers of medicine paid no other respect to that prostration of strength which marks the invasion of malignant fevers, than to bleed their patients till the prostration was overcome; and we shall even find them employing the lancet to subdue that state of syncope which attends the worst forms of congestive fever. There was little or no "bark-and-wine-practice" in those days.

Different ages of the world, however, appear to have been distinguished either by different degrees of moral firmness, or practical habits; for certain it is, that knowledge had reached a high advance at the time of Hunter, when bloodletting had given place to the stimulating plan of treatment. The present age very justly assumes for itself a proud elevation in knowledge over all former times; and yet it must be admitted, that a numerous class are now sacrificing at the altar of debility, although "their practice is a continued struggle between the prejudices of education, and the staring conviction of opposing facts, which are continually forcing themselves upon their observation." (3)

But the ground is hotly contested by some of our most able observers, whose convictions and spirit we shall briefly state in the nervous language of Southwood Smith. Even "in the intense forms of congestive fever," says Dr. Smith, "I look upon the notion of debility to be an error not less palpable in its nature, than destructive in its consequences; and if the havoc it produces do not confer upon it a pre-eminence as bad as that of

(1) Aph. s. 1. 6. (2) De Cur. Morb. Acut. l. 2. c. 1.
(3) Bateman on Fever, p. 98.

the very disease of which it is supposed to constitute the essence, it at least entitles it, in comparison with every other error in medicine, to the distinction recognised in society between the hero and the murderer. The one destroys a single human being now and then; but the other numbers its victims by thousands." (1)

Our records abound with facts like the following: "In the late war," says Robert Jackson, "hospital practice occupies a conspicuous column in the statement of public expenditure. Hospital mortality accounts, principally, for the diminution of the army. In these two points, the late war is held to be unexampled." (2)

In another work Jackson gives a clue at the cause of all this mortality. When the typhus fever prevailed among the soldiers during their retreat through Holland, in 1794, so strong was the prejudice in favour of stimulants, and opposed to bloodletting, that Jackson was obliged to yield to its influence so far as the latter remedy was concerned, although against his own judgement. "The remedy," he says, "was in a manner interdicted at the beginning of the war of 1793." (3)

We shall see, in another place, that what is called "typhoid pneumonia" often grows out of a neglect of bloodletting in the ordinary form of pneumonic inflammation. But when pneumonia presents some of the symptoms of typhus from the beginning, the inflammation is complicated with venous congestion, as will be explained hereafter; and hence it is, for reasons which will be also stated, that Dr. Stokes and others describe "typhoid pneumonia" as being remarkable for the obscurity of its symptoms. But, although bloodletting be not always necessary to the same extent in this modification, the remedy appears to have been more indispensable, whilst stimulants are more fatal, than in the unmixed form of the disease.

But it is not alone in the simple forms of pneumonia, and the typhoid variety, that we are told that "we must be sparing of the vital fluid." "When," says Laennec, "pneumonia is complicated with a bilious affection, bleeding must, in like manner, be more sparingly had recourse to, than when the inflammation is simple." And then follows that most dangerous precept, that "in every

(1) Treatise on Fever, p. 97. (2) Medical Depart. of the Army, p. 159. 1803.
(3) On Contagious Fever, pp. 145, 232.

case whatsoever, the more feeble the pulse is, the less indication is there for venesection."(1) We need not say how far these doctrines have been carried out by many enlightened followers.

We have already seen that the quantity of blood, which we are permitted to abstract under any conditions of pneumonia, is exceedingly small; and we think that we shall be able to show that the fault belongs not to the remedy, but to its inadequate use. We might cite numerous examples to this effect, and demonstrate the error of depending upon prostrated strength, and a sunken pulse, unless as certain indications of a vast amount of disease which nothing can reach but decisive bloodletting; sparingly, perhaps, at first, but soon decisively.

We will confine ourselves, however, to a few imposing examples of "pneumonia complicated with a bilious affection," where M. Laennec and his school would have abstained from bloodletting; but where it is fully shown that the remedy has been brought into discredit by its timid use. We begin with an instance from a work of the ingenuous Cleghorn. It forcibly exemplifies the pernicious effects of moderate abstractions of blood, as well as the surprising utility of a greater application of the remedy, in severe and complex states of inflammation, venous congestion, and idiopathic fever, when attended by great prostration of strength.

The epidemic affection of the chest, of which he treats, was of a most malignant nature,—associated with constitutional fever, and attended by "insidious intervals and treacherous remissions," and great depression of the powers of life. Unless speedily mitigated, "its quick progress and uncommon mortality surprised him greatly." He "attempted to cure the patients by bleeding once or twice a day, except during the insidious remissions;" but they generally perished.

"This unforeseen event startled him greatly, and led him to review the whole progress of the disease," &c. He finally determined to adopt the advice of Duretus, and "to use the lancet with more caution." But his failures became still more frequent and mortifying.

At length, "he was convinced, that, instead of too much, too little blood had been taken away in the beginning, and that he had been misled by the *insidious intervals*. He then began to

(1) On Diseases of the Chest, &c. p. 249.

bleed more plentifully, — taking away 30 or 40 ounces within the three first days of the distemper. This method succeeded well in several of the cases."

Still he was not satisfied; — when, "at last, about the middle of March, when the disease raged with the utmost fury, having found that there was an absolute necessity for bleeding largely without delay, in order to preserve life, he began to put in practice the following method of cure, which *seldom or never failed;* not only in young robust people, but even in those of a more *advanced age*, provided he saw the sick before the end of the third day."

This consisted in abstracting blood in the horizontal posture, "till the pains abated or the patient began to faint, — taking from 18 to 27 ounces, avoirdupois. If the symptoms continued a few hours afterwards, the same quantity was again taken away, without regarding the state of the blood, &c. Next morning, if there were any remaining symptoms, the bleeding was repeated, and the blood carefully weighed. From 54 to 60 ounces were frequently taken away during the first 24 hours of attendance. If any symptoms returned, the patients were immediately bled again to the amount of 14 or 27 ounces."

This generally answered. We have stated the successive steps of the plan, that each may be compared with the others. The quick transition from wrong to right evinces the hand of a master. The record is full of the most important instruction; and whilst we hold it up to the present generation, we would that not only its practical instruction should be duly regarded, but, equally too, that its frankness should be emulated.

We should not fail, also, to observe the coincidence in another respect, betwixt the results of the practice of Cleghorn, and that of Robert Jackson in the inflammatory and congestive diseases of the West Indies, — to whose experience we shall refer, more particularly, hereafter. "It was remarkable," says Cleghorn, "to observe how rapidly the sick recovered their usual health and strength, notwithstanding the great loss of blood which they had sustained; while many, who had been bled more sparingly, continued in a languid infirm state, for *some* months." (1) Patients of the latter class were only partially cured. Congestion still remained about some of the great viscera, — especially of

(1) Cleghorn on the Epidemical Diseases of Minorca, c. 6. p. 164 — 168.

the liver, by which the powers of digestion were maintained in a prostrated state. We have often seen this exemplified in analogous diseases of our own climate, where bloodletting had been too sparingly employed. Under these circumstances, also, errors in diet, and mental or bodily fatigue, contribute to maintain the consecutive derangement; — till, at last, it frequently happens in our congestive fevers especially, that a slow increase of the local disturbances produces another explosion of the constitutional malady. Thus the patient not unfrequently goes on revolving, year after year, through mitigated and exasperated conditions of the disease, which more decisive treatment by bloodletting, at its onset, might have prevented.

The foregoing bilious pneumonia was a severe and complex form of disease; and, as a general principle, required a more energetic practice than is demanded by many cases of simple pneumonia. But it shows not only the perfect compatibility of copious bloodletting with great prostration of strength, in the early stages of disease; but proves that the strength may be best maintained by such treatment, where inflammation and congestive fever strike a deadly blow at the powers of life, at their very invasion. And so it has been from the earliest records of medicine; in all climates, in all constitutions, at all ages; and there in Minorca, and here in America, we witness the immutable principle laid down by the great father of the healing art, that "severe diseases require severe remedies." Constitution, habits, and age, certainly modify the details of treatment more or less,—climate comparatively little. But the great fundamental laws of the constitution remain without change, as do, also, the leading conditions of disease. We have all that Hippocrates described before our own eyes, and we are astonished at the identity. We think him, at one moment, a prophet; — and when, at the next, we realize a simple narrative of only what he observed, we are either amazed at his sagacity and philosophy, or that we should have been so slow to have discerned the truth ourselves.

Again, Dr. Boyd, in describing the malignant fever of Minorca, when not complicated with high inflammation, states that bloodletting must be carried to the extent of positive relief, without reference to *quantity*. He sometimes repeated the operation four times in a day.(1) Our inland practitioners, at the west and south of

(1) Thesis Inaug. de Febre Minorcæ, 1817.

New-York, will see in the *simple* congestive fever of Minorca, a simile of their own, whilst their practice more or less responds to that of Boyd and Cleghorn.

But before we shall have recovered from our astonishment at the success of Cleghorn in typhoid pneumonia, let us endeavour to ascertain the possible cause of the vast mortality from this disease in other hands. We shall probably find it in an opposite system of practice, — as indeed, was so candidly admitted by Cleghorn. And that we may have fully before us the nature of the most essential difference in treatment, we shall quote the language of one of our ablest and latest writers; who, we believe, exhibits the prevailing treatment in Europe, since its foundation has been established upon the developments of morbid anatomy, — upon nature in the midst of her ruins.

"In the typhoid form of pneumonia," says our author, "bloodletting is not only very ill borne, but it appears to have little influence on the disease. There is, in fact, no vascular tension or tonicity; and you would empty the great bloodvessels, and stop the heart's action, before you could relieve the congestion of the lungs, or enable their vessels to contract. The depressing influence of typhoid or adynamic diseases renders any loss of blood hazardous; and local depletion is the utmost that can be attempted. Considerable advantage may, under these circumstances, be sometimes obtained from *dry cupping* on the chest, which, for a time, tends more effectually than even bloodletting, to *draw* the fluids from the congested organs, whilst it does not *waste the blood* from the system."(1)

(1) Williams' Lectures on Diseases of the Chest, lec. 20.

Thus, also, we find the following in its appropriate place.

"*Machine for the Cure of Apoplexy suggested by Mr. Lewis Gompertz.* Provide a plank with a perpendicular axle at one end, and place the axle in a socket, so that the plank can turn around horizontally, like a turnpike-gate. Then let the patient lie on the plank, with his head near the axle, and let this plank be caused to revolve swiftly, so that the blood in the patient's head may, by means of centrifugal force, be forced towards his extremities." (*a*)

This suggestion was probably derived from our philosopher, Dr. Mitchill; who says, that "the power of gravity may be applied to aid in retarding or accelerating the velocity of the blood, by fixing a cot or bed upon a central axis, as a tilting cart." (*b*) &c.

(*a*) Mechanic's Journal. Extracted from the New-York American of Nov. 19, 1838.

(*b*) New-York Med. Repository, vol. 12. p. 47.

We have, perhaps, already said enough of this application of mechanical philosophy to the actions of living organization; and of the importance of regarding in their proper light the forces of life. But we shall feel it incumbent to advert to numerous examples of the foregoing nature, not only on account of their practical bearing, but for the purpose of repelling the charge of an inefficient treatment of disease, which has been alleged by the "anatomical school" against the anatomico-Hippocratists. The right of defence must not be denied, — and examples are necessary to produce conviction.

Again, we may find a cause of the comparative mortality of the worst forms of pneumonia in the hands of others, by contrasting with Cleghorn's, other modes of treatment which have been employed by the most illustrious anatomical physicians, and extensively propagated by the influence of high authority. Thus, the illustrious Laennec,—so justly immortalized by his genius and discoveries: "In the treatment of pernicious peripneumonia, we must ever keep in mind, that bloodletting cannot cure a disease, which will certainly return after a few hours with fresh violence; and of which experience has long since demonstrated bark to be the only effectual remedy."(1) By referring back to what Cleghorn has said of the "insidious" remissions attendant on the epidemic of Minorca, we shall find a true interpretation of the foregoing paragraph.

But it is not alone the early stages of pneumonia to which bloodletting is applicable. Eminent practitioners, even amongst the French in by-gone days, practised it at late periods of the disease. Professor Baumes, and Roucher, bled successfully on the 11th day. Guy Patin on the 13th, Husson on the 14th, and Huxham himself, and Triller, on the 18th, 19th, and 20th days.(2) Van Sweiten bled, also, as late as the 20th day, and adds, "cum *pulchro successu* me serius adhuc venæ sectionem aliquando instituisse me probe memini."(3)

The symptoms, not the period of the disease, must form the indication. The greater the disorganization, the greater our duty to give the sufferer his only remaining chance. "Prima praxeos fundamenta sunt indicationes, quibus detectis remedia

(1) On Diseases of the Chest, p. 248. Tr.
(2) See Polinière sur les Emissions Sang. t. 1. p. 129.
(3) Comment. t. 2. p. 146.

facillime depromuntur e medicinæ penu."(1) Therefore, too, the precept of Galen: "Quodcumque enim die mittendi sanguinis scopas in laborante inveneris, in eo auxilium illud adhibeto, *etiamsi* vel *vigessimus* is ab initio extiterit."(2) Failure, no doubt, in these prolonged, and generally neglected cases, must often discourage effort, and bring down the prescience of ignorance or prejudice; but there are always intelligent observers, who will appreciate the well directed skill, and reward its worth and independence. The applause of the crowd is something, to be sure; but an approving conscience more than supplies the loss.

Let us, however, step aside for a moment, from the grave diseases under consideration, to contemplate the effects of bloodletting upon constitutions broken down by prolonged suffering and wretchedness. Let us look at the minister of relief, who comes to rescue from a lingering death the subject of a mortified extremity, which had been advancing to its present state for a year. At this period the leg was removed above the knee. The patient had been crowded, for months, with tonics and stimulants, and "was reduced to bones." The stump put on an inflammatory action. The admirable surgeon, Kentish, saw nothing but death in prospect, unless he opposed the dictates of philosophy to the prejudices of the lookers-on. "What!" said they, "bleed a poor man who has been confined above a year, and is quite reduced to a skeleton? Oh, for shame." But philosophy triumphed, and ignorance stood rebuked. Blood was drawn, and nature began to rally. Still the system remained oppressed with the effects of former disease, and former practice. More blood was again, and again taken; and at each outlet, nature acquired fresh vigour;—the inflammation gave way, and the patient fully recovered. "Near a year afterwards," he remained a monument of the benefits of science, and of moral courage. The patient was 60 years of age.

Here is another case of a parallel nature,—only more illustrative of the safety and utility of extensive bloodletting in debilitated states of the constitution, when that debility is associated with disease for which bloodletting is a remedy in more robust

(1) Baglivi Prac. Med. l. 2. c. 10.
(2) Galen, de Curand. Rat. per venæ sect. c. 20.

habits. It is a case of diabetes by Dr. Barlow, (1) which had reduced a boy to a feeble and emaciated state. In this condition, he was bled to the extent of 209 ounces, within 51 days. The operation was repeated 12 times; so that each bleeding averaged upwards of 17 ounces. The result of this was, a rapid restoration of health and strength, and a return to his plough.

And here is another somewhat parallel case, by the able Wardrop: "A lady, in a state of pregnaney, had been greatly debilitated," &c. "She was emaciated, and so feeble that her recovery was, by those around her, considered hopeless. She had a distinct tenderness on pressure in the epigastrium, and her pulse, which gave at first the impression of great langour, on more minute examination was very contracted, feeling like a thread, and incompressible, while the heart's action was vigorous." Bloodletting "was immediately resorted to, though with hesitation by other medical attendants. No sooner had a few ounces of blood flowed from the vein than the pulse began to rise and acquire volume, and upwards of 20 ounces were abstracted before its vigour was subdued." "The patient's recovery was progressive." (2)

We shall merely add, that we have had many cases of constitutions broken down by local inflammations and congestions, in which we have employed the lancet, and often extensively, with the happiest results. But our method is, to prefer the cases of others to our own. Enough are already extant.

We shall now proceed to an examination of the treatment of *simple* pneumonia, and of some other inflammatory affections, as founded upon the "numerical method," in its connection with morbid anatomy as cultivated by the exclusive philosophers of the anatomical school.

M. Louis, in his "numerical" treatise on Bloodletting, endeavours to set aside the practical results of all other eminent men, whose observations were probably not less accurate, although not reduced to a tabular form. The latter observers had found themselves more usefully employed in giving their whole time to the study of nature, and in recording general facts, and general re-

(1) Cyclopædia of Practical Medicine, art. Plethora.
(2) Wardrop, on Bloodletting, p. 29.

sults, or, in presenting examples in detail, which should most clearly illustrate the ordinary conditions of disease; and thus form the luminous basis of general principles. Where these philosophers have delineated disease, a minuteness has been observed which gives us the advantage of knowing how much is to be ascribed to nature herself, and how much to art; how much to a particular remedy, and how much to the neglect of others; how much to the adaptation of bloodletting in respect to quantity, — the opportuneness of its applications, — the auxiliary means, — the stage of the disease and its progressive complications, — the constitution, habits, &c.; all of which have an important influence upon our conclusions as to the particular effect of any one agent.

It is a complaint of the "numerical school," that general affirmations cannot be trusted, without the tabular view before us. This is no better than a general distrust of the common honesty of mankind. A general statement being given implies an induction from a proper estimate of particulars, and is equally entitled to our assent, as if accompanied by the exact amount of observations upon which the conclusion is founded. No farther information can be derived from numerical figures, than their mathematical import; and if the calculations be properly made, by excluding from books the arithmetical process, a useless encumbrance is avoided, whilst the imposing exactness of mathematics will be divested of its injurious influence upon our practical and theoretical views.

But to what greater confidence are numerical tables otherwise entitled, than the general affirmations which, we must admit, have been equally founded upon an estimate of particulars? May we not, also, more safely deduce a general principle from a few well digested and unquestionable facts, delivered with all their attendant circumstances, and collected from a variety of sources, than from a greater number expressed in a tabular form, and reposing alone for their authority, as facts, upon the judgement of an individual?

We can see no difference betwixt the "numerical method" of specifying the particular number of cases, so far as aggregate results are concerned, and that of which M. Louis complains, where the observers have declared "their experience to have been large," but without stating specifically how large. It matters not, whether the number of cases have been 500 or 1000,

so long as authors declare that their experience has been *large*, and that it decidedly favours a certain remedy. Comparisons with the experience of others may be as readily instituted; and where the results of general experience coincide, principles may be as safely and as truly deduced, as when the comparisons assume the more exact form of precise numbers. We get nothing from the tables but a general average; for the detail of figures produces no greater conviction than an affirmation of large experience. It is only necessary to believe that what is stated is true. It cannot be admitted, as our author has it, that "if thousands of authors have proceeded in the above manner, it is as if there had been but one; and in many respects, as if there had been none."(1) Our author complains that Vieusseux, in speaking of epilepsy, remarks, that "he had *almost always* used leeches, and with success." Vieusseux implies that his experience had been large, and his practice successful. If we believe the statement of this author to be correct, it is entitled to an equal weight with the mathematical calculations of our author. But our author thinks differently. "If you are certain," he says, "that you have more frequently succeeded with leeches than without them, you must have counted the cases; and if so, why do you not state the number, that we may have demonstration instead of mere assertion."(2)

We might remove this objection by an *argumentum ad hominem*. We might ask whether the accuser has not rested his own claim to credibility on a mere numerical statement? Whether in his treatise on bloodletting, he has given us any proof that the diseases were such as he has represented? Whether some of the patients may not have been the victims of an erroneous diagnosis, and of malpractice? Whether he has not substituted "assertion for demonstration" in every important particular?

Figures may disguise the truth as well as language; and even more so, since they have no elements of exposure. Vieusseux, therefore, by having reported the number of his cases, would have been entitled to no greater credit for veracity, nor would it have rendered his experience in the slightest degree more valuable. All that is necessary in these cases of general, but positive affirmation, is a knowledge that the observer is a

(1) On Bloodletting, p. 69. (2) Ibid. p. 84.

man of truth, and interested in science. Reject this principle, and medicine will cease to be cultivated by men of any character or understanding. But what is especially liable to objection in the numerical method is the very evil of which M. Louis complains. This method, whilst it only offers the general results, supposes that the figures of one man are as full of accuracy as those of another; and we cannot better finish our intended illustration, than by quoting an objection that has been well made even to the statistics of mortality. "Tables of statistics," says the editor of the New-York Journal of Med. and Surg., "when constructed under the most favourable circumstances, can only *approximate* the truth; but, in a great city like our own, where the ignorant and careless of the profession abound, such tables may only serve to propagate error."(1)

Again, shall we discard the commentaries of Hebberden, and other analogous works, because they only narrate briefly the results of experience? Shall we set aside statements like the following, because we have no figures to calculate ourselves. Thus, Dr. Wardrop says, in speaking of the importance of bloodletting in erysipelas, that "we can be readily satisfied of the results of the two very opposite modes of treatment, by observing the practice in the different hospitals in this city, where the patients, affected with similar ailments, are bled profusely by one practitioner, and by another, get as much bark and wine as their stomachs will receive." Or, when he affirms, "that during a long attendance at a public hospital, a certain physician had never known bloodletting employed in erysipelas; the physician adding, that nearly all the cases which he had seen of that disease, affecting the face and head, had terminated fatally."(2) Who will deny that Armstrong spoke from accurate experience, when he said, that the "wine and bark system is of all the most fatal practice, generally, in erysipelas?" If numbers be required, the same author was not unmindful that they might be demanded. He therefore states, that "*five* individuals had erysipelas in *one* house, and were treated with bark and wine, and *all died.*" Shall we pay no heed to numerous declarations like the following, because numbers are not specified? "When," says Gordon, "I took away only ten or twelve ounces of blood in puerperal peritonitis, my patient *always died;* but when I

(1) July, 1839, p. 226. (2) Wardrop on Bloodletting, p. 27, 61.

had the courage to abstract twenty or twenty-four ounces at one bleeding, in the beginning of the disease, the patient *never failed* to recover." (1) Or, may not one's experience be advantageously expressed in this manner,—always provided he be a man of veracity and judgement? "In abstracting blood in pneumonia," says the ~~last-mentioned author~~, "be prompt,—bleed the patient to approaching syncope,—otherwise, instead of benefiting him, you will do him harm;" and especially, when long experience leads him to assert, that "in inflammation of the serous membranes or parenchymata, he bleeds more decidedly than he ever did?" Is not the foregoing fully as satisfactory, as when he says, with greater precision, "I have treated nearly three hundred cases of sero-enteritis, with bleeding, calomel, and opium, with a success far greater than I have heard of from any other plan. There is no success on record at all comparable with it." (2) Shall we question the general truth, because numerical tables are not paraded before us, that "it may be laid down as an established principle, that if venesection does not absolutely cure intermittent fevers, it paves the way for other remedies, and is, on that account, highly necessary?" (3) Shall we discard the experience of authors whom we shall soon mention as advocates of the *remedium principale*—every one of whom was as richly endowed with genius, and withal, as honest, as any individual of the numerical school,—because they have no tabular views, and studied nature in her living aspects? Do we reject even the opinions of a Coke, a Lyttleton, &c., in a science which is not less liable to doubts and difficulties? Or is medicine really capable of "mathematical certainty?"

But we want not the array of cases, either in detail, or in consolidated column, which our author is disposed to set forth as an indispensable basis for medical science; whilst, be it remembered, our author abounds with a greater number of generalizations in relation to specific subjects than are to be found in the whole annals of medicine. These generalizations are always founded, as we shall show in another place, upon results which

(1) On Epidemic Puerperal Fever, p. 80.

(2) Armstrong's Lectures on Acute and Chronic Diseases, vol. i. pp. 401, 406, 423; vol. ii. p. 101.

(3) Senac on Fevers, p. 171. "In an epidemic double tertian," he says, "the practitioners were obliged to repeat the bloodlettings five or six times." (p. 173.) This corresponds with Cleghorn's experience in Minorca. In those cases, too, there was great prostration of strength.

came under his own observation, without the slightest regard to that of others, which, indeed, he rejects as unworthy of record; and they are always predicated of a small number of cases,—often, of less than half a dozen, and sometimes of an isolated, anomalous fact.

But others, again, who have carried out M. Louis' "numerical method," even in Paris, have arrived at exactly opposite conclusions. Nor did the system begin with our author; to whom, however, belongs the merit of urging it into notoriety, and of establishing its reputed importance. Even as late as Mills, we were wont to regard his well digested tables, instituted for the purpose of showing the advantage of bloodletting in fever, as of no value. They have never attracted notice, and are now forgotten.

The mind, too, has been long wearied with examples, in detail, of familiar diseases; and however the "numerical method" may promise intellectual relief, and a saving of time, or administer to the ease of the indolent student, it certainly offers nothing but penalties to the misplaced confidence of mankind. Accurate observers, under the guidance of genius, have long since concluded that our materials were abundant for every important principle in medicine. They have only been embarrassed by their multiplicity in elaborating the genuine from the spurious, and arranging such as are valuable into systematic method. The principles, however, are only general in their nature; and every case must have its peculiarities of treatment.

Let us hear an illustrious Frenchman, of by-gone days, upon this subject,—who sacrificed to philosophy what is now considered the philosophy of the healing art. He thus magnanimously concludes his labours: "For the better illustration of the method of cure, I had prepared for publication a number of cases. But it appeared best, on reflection, that these should be omitted. There are not two cases of disease precisely alike; whence, there are not two in which the same remedies will produce precisely the same effects. It seems more advisable, therefore, to reduce, to certain general laws, all the facts which experience has brought to light, and from these, to deduce a mode of cure accommodated to the state and condition of each patient." (1)

(1) Senac de Recondita Febrium Natura et Curatione.

Does France now boast a brighter luminary,—a better practical man, with all the advantages of the necroscopic art? And shall such experience be rejected as "nothing?"

The more extended introduction of morbid anatomy, as an element of medical science, has properly given rise to histories of disease, which the morbid results were designed to illustrate. But whilst we would cherish this branch of the science under strict limitations, we are prepared to show that medicine has lost by its modern cultivation. Illustrious exceptions there are which greatly restrain this general induction, and which hold up our science as it was once sustained on the barrens of Arabia.

We need not say, that the conclusion at which M. Louis has arrived, by mathematical process, is not favourable to the abstraction of blood in pneumonia, erysipelas, "typhoid fever" or acute intestinal inflammation, with their complications of other local inflammations. Comparing our own experience with that of this illustrious man, we might be disposed to ascribe much to a difference of climate, and to believe that the foregoing affections require a different treatment in France from what they demand in America. The same conclusions might also arise in relation to Great Britain, from what we learn of some of her distinguished sons; or, we might possibly come to conclude that they are better observers,—more practical,—and better philosophers, than we of the "New World," were we not put right, in both instances, by others of equal ability, and by the results of practice.

In France, for instance, we are told by M. Bouillaud, that by *his* system of "copious bloodletting, practised in quick succession, *coup sur coup*," pneumonia is generally arrested on the third day. Indeed, so sanguine is he of his practice, that he says "cure is the rule, death the exception." He calculates that he loses one patient in eight,—whilst others lose one in four. (1)

(1) On Acute Articular Rheumatism, p. 56, &c. Tr.

M. Bouillaud carries this practice into the treatment of erysipelas, rheumatism, and typhus fever; and although very exclusive, he states, that out of 178 affected with typhus, only 22, or one in eight died; (*a*) whilst M. Louis admits, at one time, (*b*) he lost more than 1 in 3.

(*a*) Essai sur la Philosophie Medicale.

(*b*) Archives Générales, 1828. Essay on Typhus.

Chomel(1) makes the average mortality, from pneumonia, at the hospitals 1 in 4, and Louis 1 in 3. Legarde lost 1 in 3; and Leconteulx reports 12 out of 30, by the antimonial treatment. The last were treated by Laennec, though the latter states a more favourable result at another time.

In these United States, and in Canada, where bloodletting is thoroughly adopted, the loss does not probably exceed one in twenty. But here the treatment proceeds upon Hippocratic principles. There is no exclusive system, — no numerical method; but the symptoms, and various other circumstances attending the disease, regulate the practice. It is not all bloodletting, nor all antimony. Cathartics, calomel, blisters, &c. form as well a part of the treatment. Nor have we much knowledge of the effects of bloodletting in the advanced stages of the disease, — mainly for the reason that we adopt it early.

As to any modifying influences from climate in England, either in respect to pneumonia, or other inflammations, or all the varieties of fever, we have only to consult such authors as Armstrong, Johnson, Wardrop, Elliotson, Lawrence, Smith, Davies, Weatherhead, &c. to be convinced that these diseases are the same there as in America, and require the same general plan of treatment.

Looking back to the age of Sydenham, and along the intermediate periods, we still find that every thing, on this subject, has remained without any essential change. And if we cast our recollections through the vista of time, over various countries, till we reach the age of Hippocrates, we shall find that diseases have been still the same, and have ever required the same general treatment.

As it respects the control of well directed bloodletting over pneumonia, in the present climate of England, take the following example. It comes from Mr. Lawrence, who, it will be seen, belongs to the Hippocratic school.

It is the case of "a slender young girl, in a serious attack of inflammation in the chest. He bled her not very sparingly, and adopted other antiphlogistic treatment; but found it necessary to repeat the bleeding several times; but the inflammation was not satisfactorily reduced." "The patient was still in danger." He then treated her activ ly with digitalis, when she ultimately recovered. Two or three years afterwards, he treated

(1) Nouveau Dic. de Med. Art. Pneumonie.

her again for the same complaint, and again he resorted to bloodletting. "The blood spouted out most vigorously from the vein of the arm. I got a good large vessel; it ran into it famously; and really, in cases of inflammation, where the blood runs freely out of the vein, I generally let it run on till it stops, — for that seems to me the only way of doing good, and I fully intended to have gone on till this young woman fainted; but, to my astonishment, though I had taken away an enormous quantity, she did not faint, — the blood still running in a vigorous stream, to the last. In the end, I stopped it, because the quantity did seem to me so very great." "This single venesection cured her; she was well from that time." Three pints of blood were taken away. (1) The practice was decisive, as well as the language. It was enough for this patient; too much for some, and too little for others.

The advocates of bloodletting have sometimes affected its reputation by the mere language in which it is recommended. They are said to be rash, and bloodletting shares the odium. Thus Dr. Elliotson, in speaking of enteritis, remarks, that "the first thing one has to do is to bleed the patient well. You must set him upright as he can be, and bleed him from a large orifice without any mercy." The prejudiced, or unreflecting, look only at the language; but an upright posture, and a large orifice, render the operation safe, and comparatively mild, though it proceed, as it should, ad diliquium.

So far as age may be supposed to have a controlling influence, Hourman and Dechambre have investigated with great care the effects of bloodletting and the tartar-emetic practice, in pneumonia, upon the old women at the great hospital of Salpètrière. They approve the former and condemn the latter. (2) As to the period of infancy, we shall speak of that hereafter; but may now say that the best observation is as much in favour of bloodletting at that age, as at any other, in European climates.

Returning to M. Louis, we find, that after despatching 78 cases of pneumonia, 33 of erysipelas of the face, and 35 of angina tonsillaris, in a tabular form, he comes to the conclusion by an arithmetical result founded apparently upon inefficient practice, "that bloodletting has had very little effect upon either

(1) London Lancet, No. 325. p. 270.
(2) Archives Gen. de Med. 1837.

disease," (1) — and this is not only variously reiterated here, but in other works of our author. (2)

In making these remarks, we will not justify ourselves by the example of our author, who impugns all the experience which has been accumulating from the era of Hippocrates, and who often affirms that "men have for ages devoted themselves to medicine, and the science is still in its infancy; (3) — but we act upon the right of inquiring specifically into the published doctrines of an eminent man which may sensibly affect the destinies of the human race. For this reason, especially, have we bestowed a more critical examination upon our author's opinions in another place. Nor will we yet dismiss the subject, without saying, that the universal method is mainly the offspring of hospital practice. The original is well depicted by Zimmerman: "I know," he says, "a certain Esculapius who has 50 or 60 patients every morning in his anti-chamber. He just listens a moment to the complaints of each, and then arranges them in four divisions. To the first, he prescribes bloodletting; to the second, a purge; to the third, a clyster; and to the fourth change of air."

"The same vulgar prejudice leads people to have a great idea of the practice of large hospitals. I have seen, in my travels, some of the largest hospitals in Europe; and I have often said to myself, Heaven, surely, will have pity on these miserable victims." (4)

We all know the results of later experience in the hospitals of Europe; and our only motive for adverting to this subject is to protest against the assumption of this species of observation as a basis for great principles in medicine, or its application in private practice. When we come to our analysis of M. Louis on Typhoid Fever, we shall show ample ground for our objection. Independently of the necessary haste with which hospital cases are despatched by visiting physicians, and the various resulting evils, there are others of no less magnitude. The greatest of these applies to the artificial and broken down constitutions which always abound in these institutions. Intemperance and

(1) On Bloodletting, p. 22.
(2) See Researches on Typhoid Fever, vol. 2. p. 412.
(3) On Bloodletting, c. 3.
(4) On Experience in Medicine, vol. i. p. 167.

irregular habits, alter the whole nature of man; climate, upon which so much emphasis is laid, but little; not even modifying much the same diseases.

On every principle, it appears to us, that our practical facts should be taken from the private walks of our profession. We would neither trust to the vital phenomena, the lesions of organization, nor the results of practice, as observed in the shattered and devoted inmates of hospitals. Our remarks are warranted by all experience.

As to the "bark and wine practice," or, as otherwise denominated, the "incendiary treatment," which so unaccountably prevails in hospitals, where its destructive effects should lead, at least, to other experiments, we can scarcely account for the occasional statement of its success in typhoid fever, erysipelas, &c. In respect to the former disease, perhaps a satisfactory explanation may be given. Every body knows that the exclusive necroscopist has taken upon himself the business of constituting different species of typhoid fever, as the glands of Payer, &c. may happen or not to be altered in structure. This, or that, lesion makes up the disease. Prior to this interpolation upon the science of life, a variety of diseases, even pneumonia, &c. were confounded with typhus fever as soon as a dark tongue, or headache, &c. supervened; and nothing is more common than to designate remitting bilious fevers by the former appellation. We think, therefore, not only for this, but other sufficient reasons, that, in the cases of hospital success with "bark and wine," the fevers have been induced by those miasmata which generate intermittents, and of which, in Italy, we know that the most malignant are sometimes treated successfully by bark. (See p. 233.) But this does not show that they would not have been sooner, and better, and more generally cured by previous depletion; since all cases, like the Italian, suppose the existence of some local congestion or inflammation. As Hippocrates has it, "he, who would purge bodies, must first make them permeable." (1)

Whenever local developments were present, Baglivi affirms positively, that they could not be cured without previous bloodletting. (2) Sir John Pringle, with all his fear of "putrefaction," (and no author has more,) in treating fevers of this very class and

(1) Aph. s. 2. a. 9. And so Avicenna, l. 1. Fen. 4. Doct. 5.
(2) Prax. Med. l. 1. c. 13, and c. 15. Fernel the same, M. M. l. 3.

malignancy, occurring in "low and marshy countries," "found it necessary to begin with opening a vein, and to repeat the bleeding according to the urgency of the symptoms." "A person," he says, "unacquainted with the nature of this disorder and attending chiefly to the paroxysms and remissions, would be apt to omit this evacuation, and to give the bark prematurely." (1) Cragie says, that in Great Britain, remittents now require the loss of 25 or 30 ʒ of blood. (2) Cleghorn states, that "for his part, when called early enough, he used to take away some blood from all people, of all ages, when affected with tertians, unless there was a strong contraindication." (3) And so Senac, "the physicians bleeding five or six times in an epidemic tertian." (4)

It should be well considered, too, that there is something in the foregoing class of fevers to which the bark is *specifically* adapted; whilst we aver, that it has been the experience of the most enlightened of the profession, at all ages of the art, that bark and wine are pernicious, at least, in the early stages of all other fevers, and of erysipelas. (5)

(1) Pringle, on Diseases of the Army, Part 3. c. 4. p. 201. As to wine, in "Hospital fever," he says, "however it may be necessary in the *low state* of the fever, we are to remember, that it is to be administered only as an *antiseptic*, and supporter of the *vis vitæ*." (P. 318.) Bloodletting was his first and great remedy in all fevers, if attended by any inflammation or congestion. Though a most absolute humoralist, he had no respect for the doctrine in his practice. He took all his indications from an enlarged view of the vital phenomena.

(2) Practice of Physick, pp. 184, 186.

(3) On Diseases of Minorca, c. 3. p. 114. (4) On Fevers, p. 171, 173.

(5) Since the foregoing was written, we find in the July No. (1839) of the New-York Journal of Medicine and Surgery, all that we want to illustrate our text. The information is embraced in the Report of Hospital Cases. We have only to refer our readers to the proper distinction which is observed by Dr. Johnston, between our own intermitting and remitting fever, and the typhus as affecting Irish emigrants, just landed on our shores. The success, which attended the treatment of the former, leaves no room for comment, unless what we have said in our text as to the advantage of a more discriminating treatment in accelerating and perfecting the cure be applicable. We may say, also, that we think a perusal of the cases justifies the belief, that the cures were effected, wholly, by the early cathartics of calomel, &c. and very judicious doses of quinine; and that the patients, generally, would have done better without the diffusible stimuli, and with a non-stimulating diet. We think they would have been less liable to relapse hereafter. The consideration however, should not be overlooked, that hospital patients will often *bear* a mixed treatment which would be utterly fatal in private practice. Nor will we neglect saying, that we know that other surgeons have been eminently successful, at the New-York

We shall now cite an instance which illustrates the origin of the "numerical method," and other doings of certain philosophers. We will take for our purpose a report from the French Academy of Medicine, made by M. M. Andral, Louis, Bouillaud, and others, upon the purgative treatment of "typhoid fever," which had been so exclusively recommended by Larroque. The commission actually physicked, without mercy, one hundred patients, and without the slightest regard to symptoms, age, sex, or stage of the disease; and, by way of comparison, they bled another hundred upon the same plan;—but more merciful to another hundred they adopted "une méthode à peu près expectante." Who would not say with Zimmerman, "Heaven have mercy on these miserable victims."

The experiments, as might well be supposed, had a salutary influence; and when their discussion came up, Andral, Piorry, Castel, Rochoux, Brichteau, Bousquet, and Bouillaud, (1) raised their voice against the mathematical system.

M. Double remarks, that if the numerical method were to be adopted, "medicine would cease to be a science, an art, or even a profession. It would become as mechanical as the employment of a shoemaker." (2) He fully justifies this conclusion by an appeal to the science of statistics, the universal law of large numbers, the calculation of probabilities, &c.

It is no more a multitude of patients, than of recorded cases, that imparts the best knowledge, or forms the skilful practitioner. It is the limited number, as insisted by Celsus, whose diseases can be duly analyzed and considered. Hippocrates practised only in small towns, insufficient to support a single practitioner.

Hospital, in cases like the foregoing, where the antiphlogistic treatment was pursued.

Coming to the 20 cases of typhus, which were all supplied by two vessels, we find a luminous view of the pathology and treatment, the remedies being "strictly *antiphlogistic*."

In that excellent Hebdomadal, the Boston Medical and Surgical Journal for June 26, 1839, is a communication from Dr. Barbee, from the "Valley of the Wabash," which adds to the multitude of facts that establish the specific nature of the predisposing causes of intermittent fever, and its varieties that are known as the remittent. The cases communicated were associated with intermittent pleurisy, and intermittent dysentery; which were cured by depletion and quinine. They also go to prove, that our diseases and those of Europe are not remarkably modified by climate, nor by the lapse of time.

(1) See, also, Bouillaud on Articular Rheumatism, pp. 34, 35.

(2) Gaz. Méd. and London Med. Gaz. May 13, 1837.

On the whole, we do not believe that the "numerical method" will enjoy many advocates amongst that part of the profession who have been addicted to the practice of laborious investigation of individual cases. The system of counting belongs to abstract inquiries, and appears not more likely to ensure those just conclusions, at which its author aspires, than a similar practice, which sometimes obtains amongst jurors, determines a just decision. The plan is exclusively empyrical, and shuts out the understanding. It takes not into account, either pathology, the remote causes which so constantly modify diseases, the complications of disease, the combined influence of associated remedies, sex, age, constitution, habits, malpractice, &c. Particulars may be rendered of individual cases; but this is only the former philosophical method. It affects not the great numerical amount. The rest is an encumbrance, a canker. "Simple views," says a profound observer, "whether of health or disease, however ingenious, can seldom be just. They have their origin in the spirit of system, not in the careful study and faithful enumeration of the complicated circumstances which concur in the production of all vital phenomena." (1)

"Each individual malady is not a simple phenomenon that can be represented by unity. It is not certain and fixed, but constantly varying. Thus the pneumony of to-day is not the pneumony of yesterday; and the pneumony of Peter is not the pneumony of Paul." "Take any large collection of cases; consider the epidemics of Hippocrates, the constitutions of Baillou, the letters of Morgagni, the consultations of Hoffman, the Ratio Medendi of Stork, &c.; how many cases will you find alike?(2)

Every new case is a new and separate problem, however great the general resemblance. It has nothing fixed; but consists of a series of changing actions, which may require as constant variations in treatment. Even bloodletting and wine may be simultaneously useful. This doctrine was well understood and taught by Hippocrates. One body, he says, differs from another body, even when it is of the same temperament, and seemingly under similar circumstances. And again, of the modifications

(1) Thompson's Lectures on Inflammation, p. 174.
(2) M. Double in Gaz. Méd. and London Med. Gaz. May 13, 1837.

of season: "Ut quod methodo currente anno ægrotos liberavis, eadem ipsa anno jam vertente forsitan e medio tolles." (1)

Again, "there is one sort of small-pox in which the nurse cannot kill, and another in which the physician can never cure." (2)

Hence we go back to the origin of our art for that best advice, "diligenter considerare quid horum faciendum sit oportet, et ubi magis, aut minus ea adhibere convenit. Utrorumque enim usus opportunus, (ἡ χρῆσις εὐχαιρός,) itemque negligentia, magnam differentiam habet." (3) "Λεγε πρατικως, και πρατε λογικως. Reason as a practitioner, and practice with reason."

Again, says this remarkable man, "an important thing to be done in the study of medicine, is to consider the seasons of the year, their various changes, and the differences of their effects. Next the winds, particularly such as are common to all nations, and those which are peculiar to certain countries." "The knowledge of disease is to be obtained from the common nature of all things, and from the nature of every individual; from the disease, the patient, the things that are administered, and the person that administers them; for the case becomes easier or more difficult accordingly. We are also to consider the whole season in general, and the particular state of the weather, and of every country; the customs, the diet, the employments, the ages of every one, the conversations, the manners, the taciturnity, the imaginations, the sleeps, the watchings, and the dreams; and how far vellications, itchings, and tears are concerned; and what the paroxysms are; what the evacuations by stool, or urine, or spitting, or vomiting may be; and what changes may happen from one disease to another, and the various sequelæ. Sweat, cold, shivering, coughs, sneezings, sighings, breathings, belchings, flatus (secret and audible,) hemorrhages and hemorrhoids are also to be considered, together with their respective consequences." (4) Will you embrace these considerations in numerical tables? Will you deny that Hippocrates had a philosophical understanding of medicine? Do not the foregoing facts conduct us to the true philosophy of "prescription making," and shake the very foundation of those empyrical rules which we have hitherto considered in regard to bloodletting?

(1) Hippocrates, de Morb. Epidem. 1.
(2) Proverb. (3) Hip. de Medico, v. 43.
(4) De Aere, etc. s. 1. ver. 1 — 7; and de Morb. Epidem. l. 1. s. 3. ver. 1 — 10.

Again, as Celsus has it too strongly; "differe quoque, pro natura locorum, genera medicinæ; et aliud opus esse Romæ, aliud in Ægypto, aliud in Galliâ." (1)

Plato, ever prone to reason, says that every disease should be treated according to its particular and peculiar determinations.

And so, too, the very poet:—

> "Et quoniam variant morbi, variabimus artes;
> Mille mali species, mille salutis erunt."

Hence it was that Galen denominated the physician "an inventor of opportunity."

Thus might we go on, and bring up a host of our derided ancestors,—philosophers, physicians, and poets,—who would prove to us that they were as well informed as ourselves of the advantages of estimating disease by mathematical rules. Hillary has thus summed up their substance. "The empyric prescribes such a medicine, and such a *method*, because such an author, or such a physician, or himself, has known that medicine cure such a fever, or such a disease, without either knowing the cause, or the true intentions or indications of nature, in any of those diseases. If he chance to succeed in several cases, he grows so fond of it, that it becomes his favourite medicine, and he prescribes it in almost all cases and constitutions; often where it must be injurious, and may chance to kill; though he does not see this, because he prescribes it without truly reasoning from its causes to its effects, or its true indications, viz.: empyrically or by guess,—and so it soon becomes fashionable." (2)

Sir G. Blane, in speaking of this class of physicians, remarks, that "the convictions of their own sanguine minds are so irresistible as to betray them into errors against the plainest evidences of the senses; and we can in no other way account for the effect of remedies, and of cures performed and reported ***bonâ fide***, which could never be verified by the experience of others." (3)

Every body bleeds to death in apoplexy. Is this right? Is not, rather, Burserius right,—who says: "the physician should, while arranging the body of the patient, deliberate with himself, as far as possible, of what temperament, age, and habit, the patient is; how he used to formerly live; as, also, what causes preceded the complaint, and what ones excited it; and, lastly,

(1) Celsus, l. 1. p. 7.
(2) Inquiry into the Method of Improving Medical Knowledge, p. 374.
(3) Blane's Medical Logic, p. 247.

what the state of his strength formerly was, and now is; and in consequence of weighing all these things, — having ascertained the species and degree of violence of the complaint, he ought, without loss of time, to employ the treatment adapted to it." (1) Where are your *rules* in cases of this nature? Can they be found but in luminous physiological and pathological principles?

Finally, we have some reason to think, that the dangerous innovations of M. Louis and his school are taking root on American soil, though incomparably less so, than is indicated by one of our distinguished countrymen in his correspondence, when lately at Paris. "Among the physicians," he says, "of the Hôtel Dieu, there is none so well known with us, and none so highly esteemed, as M. Louis, the celebrated author of the application of the numerical method to observations on fever, with a view of ascertaining, with arithmetical precision, the phenomena which belong to this important disease." (2)

Although we shall again recur to the foregoing subject, we have thought it expedient to connect it, in part, with our remarks upon bloodletting; since it is here, especially, that its mischief has been displayed, and here it is most calculated to work the subversion of science, and the destruction of man. And having accomplished all that may be useful at present, we here take our leave of this master spirit of our age; and in his own language, when parting with Polinière, "enough has been said to give an idea of his method, and we shall refrain from any comment whatever on his later researches; so that, no one may be at liberty to suppose, that in any criticisms we have made, we have had any other design than to discharge a duty." (3)

We entirely agree with our author, that there is no pursuit in which independence of opinion is more important to mankind, than in the science of medicine; and whilst we endeavour to cultivate this principle, and would reprobate an imbecile submission to authority, we will be among the last to prove ungrateful to those sages, who have, through ages of toil, reared an edifice not less sublime, than beautiful in its proportions, and which, we doubt not, will prove impregnable to the "numerical method:" "Sit honos antiquitatis, sit ingentibus factis, sit fabulis quoque." (4)

(1) Burserius' Institutions of Med. vol. 4. p. 132. Tr.
(2) Boston Med and Surg. Journ. vol. xviii. p. 43.
(3) M. Louis on Bloodletting, p. 97. (4) Plin. l. 8. c. 24.

SECTION XIII.

FARTHER EXPERIENCE OF DISTINGUISHED OBSERVERS.

In the present section we shall present the opinions of some of the early medical philosophers, or such as have preceded the era of modern pathological anatomy, upon the advantages of bloodletting in inflammations and fevers. They are mainly such as formed their inductions from an observation of nature in her living aspects; so that, we shall accomplish the double purpose of showing the conclusions of minds that were not specially enlightened by the disorganizations of disease, and how far the imputation is tenable, that Hippocratists are inefficient in practice.

The world, however, has never been without its variety, nor genius and erudition without their obstacles. These are forever springing up in the shape of jealousy, a courtly deference to the crowd, or the prejudices of ignorance. The fearless and philosophical practitioner, therefore, has a triple conflict to sustain in his own peculiar walk, whilst the multitude at large may press hard from every quarter. Hence we find, that the greatest benefactors in medicine have been often traduced, and even driven to other occupations. It began with Hippocrates, who, at the close of his life, regarded the past with the lamentation that "he had been oftener censured for misconduct, than praised for success."

We have been led to these prefatory remarks, by the opposition which bloodletting has incurred from a class of physicians at former eras of our art; and what is now most extraordinary, they, who profess that science and skill are only to be found in the cadaverous body, are amongst the greatest drawbacks upon efficient practice.

We need not say that bloodletting had its advocates at a period long anterior to the time of Hippocrates, or that it has been commended as an important remedy by a great multitude of authors. These may be known by a reference to the histories of our art, especially those of Sprengell, Le Clerc, Carol, and more recently, Nopitsch; the last of whom has furnished a chronological account of bloodletting from its origin. He enumerates 740 writers in connection with this subject, down to the year 1830; thirty of whom appeared before the Christian era. It is

not our intention to give any thing like a history of this remedy in the remarks which follow; but merely to cite the experience of the most illustrious observers in relation to particular diseases.

Leonardi Bottalli thus philosophizes upon the merits of bloodletting, and the causes which have impaired its utility. "In short," he says, "I conceive there is no plague wherein bleeding may not prove more beneficial than all other remedies, provided it be seasonably used, and in due quantity. But I am of opinion it sometimes does no service, either, because persons have recourse to it too late, or use it too sparingly, or commit some error in both these particulars. But if our fears be so great, and we take away so small a quantity of blood, how is it possible to judge exactly what good or mischief bleeding may do in the plague? For if a disease, which requires the loss of four pounds of blood for its cure, and yet but one be taken away, destroy the patient, it does not, therefore, prove destructive because bleeding was used, but because it was employed in an improper manner. (1) But ill designing and indolent men always endeavour to lay the fault to the bloodletting; not because it did really do mischief, but because they desire to give every body an ill opinion of it. Or, suppose they do not do it from wickedness, they cannot be excused from ignorance and perverseness, — both which are, doubtless, pernicious, but the former much more so." And again, "one hundred thousand men perish from the want of bloodletting, or from its not being timely employed, where one perishes from excessive bleeding, when prescribed by a physician." (2)

"Nothing," says Sydenham, "is more frequently urged as a capital argument, by those who condemn bleeding in the plague, than the mischief which arises from bleeding in an improper manner." (3)

So, Hoffman to the same effect. "Nam sicuti inflammationibus sanandis nimia sanguinis quantitas nocet, ita etiam exigua ejus portis est iis adversa." (4)

Plater says, "though some wink at, disallow, or quite prohibit all bloodletting in *melancholy;* yet, by long experience I have found innumerable saved, after having been bled twenty, nay sixty times, and to enjoy a restoration to health. It was an ordinary thing of old to take at once from such men six pounds

(1) We have seen that Cleghorn, Hey, and Denman, made this admission of their own practice.

(2) De Cur. per. Sang. Mission. c. 36. (3) Works, p. 101.

(4) F. Hoffman de Magno Ven. Section. t. 3, p. 269.

of blood, which we now scarce dare take in ounces." (1) These were obviously our modern cases of chronic hepatic congestion, or chronic inflammation of the mucous coat of the stomach.

We have already quoted Gordon as saying, that he lost every patient in puerperal fever when he bled only to the extent of 10 or 12 ounces; but that all recovered "when he had the courage to abstract 20 or 24 ounces." (2) Armstrong, by "copious bloodletting," "lost only 5 out of 43 cases."

In the same disease, "after we have once established the utility of sanguineous effusions," says Baudelocque, "I am of opinion, that bleeding should be, generally, in very large quantity. It is, perhaps, for want of proper energy, that frequently we do not attain results which we had a right to expect from this remedy. The abstraction of a few ounces of blood will, at most, only produce a momentary diminution in the symptoms, and retard, somewhat, the progress of disease, which will soon recommence with increased violence." (3) Legouais, (4) Armstrong, (5) Hey, (6) Denman, (7) and many others, make the same comments on bloodletting in this affection.

"Nam quæ ex hisce rebus profuerunt, ob rectum usum profuerunt, quæ vero nocuerunt, ob id quod non recte usurpata sunt, nocuerunt. (8) Hence the reason why "every puerperal woman in Dr. Lowder's time, who was blooded, died," — "ten ounces being considered a large bloodletting;" (9) whilst Levret, accustomed to the timid practice of Lowder, affirms, that "he had never seen one woman escape after bleeding." (10)

(1) Prax. Med. (2) Gordon on Epidemic Puerp. Fever, p. 80.

(3) Baudelocque on Puerp. Peritonitis. p. 326. (4) Inaug. Dissertation.

(5) Account of the Epidemic at Sunderland, &c. (6) Ut Citat.

(7) Ut Citat. Dr. Denman says, "I am now convinced by manifold experience, that my reasoning was fallacious, and my fears groundless; and that what I had considered proofs of the insufficiency or the impropriety of bloodletting in the puerperal fever, ought, in reality, to have been attributed to the neglect of performing it in an *efficient* manner, at the very beginning of the disease."—*Denman's Introduction to Midwifery*, vol. 2, p. 480.

Those who have the curiosity to witness the results of bark and wine, and the neglect of bloodletting in puerperal fever, may consult Walsh on "Puerperal Fever," p. 40; Manning on "Female Diseases," p. 371; Millar on "Puerperal Fever;" Leake's "Practical Observations," and "Child-bed Fever;" Clarke's "Practical Essays," p. 158, &c.; Levret, "Aphorism," 995; Whitehead on M. Doulcet's "Memoir," p. 33; Hulme on "Puerperal Fever," pp. 74, 77; Denman's "Essays on Puerperal Fever," p. 18; Butter on "Puerperal Fevers," p. 26; Cooper's "Compendium of Midwifery," p. 219; White "On the Management of Lying-in Women," p. 217.

(8) Hippocrates de Arte, ver. 74.

(9) See Leake's Practical Observations, p. 147. (10) Aphorism 995.

We might go on with the same variety of experience, illustrating the conclusions which we had drawn upon philosophical grounds, of the pernicious tendency of bloodletting in severe visceral inflammations and venous congestions, when inadequately performed. The vital forces are roused into greater energy, the whole circulation is released from its embarrassment in the capillary system; and the heart, being thus, and in other ways, set at liberty and invigorated in force, propels the blood with increasing violence. This mechanical impulse, in itself, lights up the flame of disease, and kindles it in other parts already disposed to join in the work of havoc. But much is also due to an augmented irritability of the instruments of action. The partial impression upon their vital forces takes off the depression from such as were not immediately concerned in the morbid process, and an exciting influence is thus reflected upon the immediate agents of disease, which, in their turn, had equally sustained, in a more direct manner, an exalted state of action. The remedy, therefore, not only in the disease under consideration, but in all grave visceral inflammations, should be carried to the point of absolute depression. The forces of life are not only then subdued in energy, but the strength of the impression alters the modified condition of such as carry on the disordered actions, and which constitutes the essential pathology of disease.

"Non possum absque commiseratione ægrorum," says Riolan, "et detestatione medici, legere historias ægrorum, qui febribus ardentissimis torrefacti fuere, absque venæ sectione. Observavi in Anglia, Germania, Belgio, raras esse detractiones sanguinis, ub sinunt suos ægros febribus ardentibus exuire sine missione sanguinis, aut saltem parca, itaut a sanguine *præfocata* plurimi pereant, vel incidant in apoplexiam, vel paralysin, aut alios diuturnos affectus et immedicabiles." (1)

Who, of enlightened experience, will not recognize in the foregoing quotation a portrait of his own sensibilities, when he has seen one after another perishing from the neglect of an agent which might have slain the destroyer at the onset of the conflict; or, again, when he has seen that very agent, from unskilful or timid use, converted to the advantage of the foe; or yet again, if nature have partially triumphed over disease and malpractice, has he not witnessed a half-subdued conges-

(1) Opera. l. de Circ. Sang. p. 585.

tion, a conversion of acute into some chronic derangement, which again slowly gather strength to explode again into the original malady; or the subjects of which, in the language of Riolan, "vel incidant in apoplexiam, vel paralysin, aut alios diuturnos affectus et immedicabiles."

Mr. Hey helped the evil of a timid bloodletting by the auxiliary force of bark and wine in his early cases of puerperal fever; but he was surrounded by prejudice, and the record he has left atones for the error. "The truth is," he says, "bloodletting has seldom been fairly tried. Either the quantity of blood taken away has been too small, or the time when it was taken too late to be of any use; and thus the principal remedy of the disease has been brought into disrepute." (1)

But here, again, Mr. Hey was mistaken. Bloodletting, to the greatest admissible extent, had predominated as the only great remedial agent for all inflammatory affections and fevers of the most depressing character, ages before our author came to illustrate the truth by other martyrs; and this in England, as well as in France, Germany, Arabia, Italy,—the island of Cos. The "disrepute," even in Mr. Hey's day, was local,—not general; for, whilst the temporary reign of the "bark and wine treatment" crippled the best practitioners in Great Britain, reason maintained its unrestrained exercise in other countries.

That "ninth case" of puerperal fever, of which we have spoken particularly in a former section (pp. 249, 221,) and which forms a ground-work, as it were, of Dr. Hall's opinions on the bad effects of bloodletting, was the last mistaken act that divided the professional life of Mr. Hey into two distinct eras. The patient died, and with her death came Mr. Hey's conviction of his error, his manly atonement, and his subsequent success. The more malignant and prostrating the fever, the "measure of bloodletting was greater and less limited."

The truth of all the foregoing opinions is partially confirmed by M. Louis himself, when he says, in one place, respecting pneumonia, that, in those cases where bleeding was adopted but once, and this sparingly, the issue was quite as fatal as where the remedy was wholly neglected; whilst at another time he says, that "bloodletting performed twice, during the first ten days of the disease, may shorten its course a little." (2)

We will now advert to those terrific maladies where nature and reason call most urgently for help; those hydras of the

(1) On Puerperal Fever, p. 184. (2) On Bloodletting, &c.

earth, which thousands would attempt to appease by "bark and wine." We shall consider enlightened philosophy in its grapple with the most prostrating and fatal forms of congestive fevers.

Of the congestive typhus, it is said by Robert Jackson, whom Beddoes calls the Ulysses of medicine, from his experience in so many climates, (1) that "the end is not attained in many cases, particularly in the more concentrated forms of fever, that appear among the military in tropical climates, at a less expense than eighty or ninety-six ounces of blood drawn at one time." (2)

Again, after stating in another place, that the quantity of blood abstracted in fever, at one time, during the years 1813 and 1814, at the hospital of the Royal Artillery, was rarely less than 3 pounds, frequently 4 or 5, and sometimes 6 pounds, Dr. Jackson remarks, that such quantities, taken at one time, "may appear unsafe to some readers. But I am warranted to say, from a retrospect of the whole proceeding, that *no accident occurred in any instance from the most excessive bleedings* that were made; and I may add that the strength was so little impaired by this apparently revolting practice, that the greater number of persons who were treated in this manner *returned to their duty within a fortnight*, in the full vigour of health." (3) We have shown, (p. 225,) that the correctness of the foregoing experience was "frequently" demonstrated under the observation of Moseley, by the results of accident.

Moseley's remark exemplifies the correctness of Jackson's commentary on his practice, where he says that he coincides with Moseley in recommending extensive bleeding in yellow fever; but that he does not accede to the rule of bleeding *ad deliquium*, which Moseley assumes for judging of the measure. It is vague and uncertain. Deliquium occurs sometimes from the loss of a few ounces of blood, sometimes scarcely from the loss of six pounds." "Dr. Rush," he says, "carried substraction of blood to a great extent in yellow fever; but the quantity substracted was obtained by repeated substractions, — not by abstraction at one time. The mode of depletion was not abrupt, — such as arrests disease by force, and such as I have in view in the present history." (4)

(1) On Fever and Inflammation, p. 175.
(2) On Contagious Fevers, p. 256.
(3) Hist. and Cure of Febrile Diseases, vol. i. p. 228.
(4) Jackson's History and Cure of Febrile Diseases, vol. i. p. 226.

Dr. Rush, however, was never intimidated by that prostration of strength which is so constantly mistaken for "alarming debility," and upon which the fatal "bark and wine system," and even the "antimonial practice," are founded. On the contrary, he "has always observed, that the cure of a *malignant fever* is most complete, and the convalescence most rapid, when the bleeding has been continued until a paleness is induced in the face, and until the patient is able to *sit up* without being fainty." "Bleeding should be repeated while the symptoms which first indicated it continue, should it be until *four-fifths* of the blood contained in the body are drawn away." And again, he says, that "bloodletting lessened the sensible debility of the system. Hence patients frequently rose from their beds, and walked across their rooms, a few hours after the operation."

But Rush, as if in anticipation of the charge of "small and repeated bleedings," says emphatically, that "the half-way practice of moderate bleeding has kept up the mortality of pestilential fevers *in all ages and in all countries;* that it is much better *not to bleed at all*, than to draw blood disproportioned in quantity to the violence of the fever; that bleeding must not be discontinued as long as the symptoms which first denoted its necessity continue," &c. (1)

Rush adapted his practice to the exigencies of the case. He had no other rule. Posterity must do him justice. His philosophical acumen led him sometimes to bleed extensively where there appeared no hope to others, but in powerful stimulation. We have quoted, in our article on venous congestion, two remarkable examples of this kind; one of which distinguished his practice; the other, that of Sydenham. In both instances, the patients appeared to these physicians almost lifeless from the prostration of the vital forces by venous congestion. In both, there was a signal triumph of principles; for both cases were brought out of that condition by repeated bleedings.

Again we find Dr. Rush accommodating his practice in epidemics to the existing exigencies. There is a distinction to be made betwixt the yellow fever of Philadelphia, and the fevers which called forth the heroic practice of Dr. Jackson. The prostration of the *vires vitæ*, and of the general circulation, were often greater in the former, than in the latter instance. Nature,

(1) Rush's Medical Inquiries, &c., vol. iv. p. 335.

therefore, frequently interposed an obstacle which compelled the American philosopher to be sometimes content with small and repeated bleedings. Had Jackson practised with Rush, he must have done as Rush did.

So great is the difference in respect to the depression of the vital forces in different seasons of the yellow fever in our cities, that an impression has obtained with some, that the disease must be treated at one time with stimulants, whilst the lancet may be demanded at another. We do not mention this fact as being just or philosophical. On the contrary, since the same disease is always the *same*, (or there is an end to all medical philosophy;) or, rather, since it is most intense and malignant where bloodletting is, at first, most imperfectly borne, if this agent be important in the milder forms, it is more so where the prostration, and, therefore, the amount of disease is greatest. It is exactly with these modifications of yellow fever, as stated by Hey, Gordon, and others, of the milder and malignant puerperal fever. The reputation of bloodletting has been as well affected by its too liberal use at the invasion of malignant cases, as by its inadequate application in others. Or, what is a more common evil, the faintness which follows a "black," and perhaps "dissolved" state of the blood, deters from that repetition of the remedy, which, by its greater abundance, would demonstrate the utility of the first experiment. But this, we think, may be held to be certain,—that if the blood be "black and dissolved," the cause of those conditions will never be reached by bark and wine — aye, not even by "salt," or by any contribution from the humoral pathology.

An unaccountable impression has gone abroad, that bloodletting, in yellow fever, has lost much of its reputation since the time of Rush. This is a mistake. Caldwell advocates "copious bloodletting," and says it was "more extensively practised" at Philadelphia, in the epidemics subsequent to that of 1793, with the best effects.(1) The great champion of neutral specifics affirms, that "those who were *well bled* and properly evacuated in the beginning, and *then* put under the *saline* treatment, almost invariably recovered." He took blood "till he had nothing to fear from increased action."(2) "General bloodletting," says Dr. Amiel, "was found useful in the epidemic, in Spain, in 1814."(3)

(1) Note to his Translation of Senac on Fevers, p. 137.

(2) Stevens on the Healthy and Diseased Properties of the Blood, pp. 339, 350.

(3) Edin. Med. and Surg. Journ. vol. 35, p. 279.

Mr. Evans states, "we bleed largely in the inflammatory yellow fever, — repeating the operation in two hours, if there remain the slightest pain on pressing the epigastrium: and, in general, if any gastric affection remain after the second bleeding towards the close of the first twenty-four hours, we repeat it a third time, and apply the leeches afterwards." (1) This practice, as we learnt on a visit at different islands a year since, prevails throughout the West Indies; and, in Eastern India, it is well known that bloodletting was never in higher repute in all analogous affections, than at the present day. We have lately heard many voices from those distant regions proclaiming its praises, and those of one of its successful advocates in the hither world. "If it were only for his manfully enforcing the antiphlogistic system," says an India Journal, "and his powerful advocacy of the necessity of bleeding and purging in acute tropical affections, Dr. Johnson would be entitled to the respect and gratitude of all Indian practitioners." (2) Bloodletting was their only successful remedy in the malignant stage of the cholera asphyxia.

Dr. Rush has been often represented as having been almost alone in the depletive treatment of yellow fever at Philadelphia, at its early visitations. We correct the mistake in behalf of Hippocratic observation; and to fortify the timid, we may add that Dr. Dewees bled Dr. Physick to the extent of 176 ounces; Dr. Griffiths bled Mr. Thompson 110; Dr. Stewart bled Mrs. M'Phail 106; Dr. Cooper bled Mr. Evans 150; and Dr. Gillespie bled himself to the extent of 103 ounces,(3) during the epidemic of 1797.

Bampfield introduces his remarks upon bloodletting by saying, that "in medical science, all reasoning and hypothesis must yield to the results of experienee, and deductions from facts. I have employed venesection not only in dysentery, but other internal and external inflammatory complaints in the East and West Indies, with the most happy results. And is it not our sheet anchor, our principal remedy, in the cure of the yellow fever, when had recourse to within the first eighteen hours of the attack?" Mr. B. exposes the origin and fallacy of the objections that have been made against bloodletting. He has been "astonished and shocked to find bloodletting in hot cli-

(1) On the Epidemic Fevers of the West Indies, 1837.

(2) Indian Quarterly Journ. of Med. and Science. April 1837, p. 147.

(3) Rush's Med. Inquiries and Observations, vol. 4, p. 36.

mates condemned," (1) whilst others of the temperate climates think it only adapted to the tropics.

Barker says it is necessary to abstract, by repeated bleedings, two hundred ounces of blood in the malignant fevers of Brazil. (2)

Hillary urges free bloodletting on the first and second days of yellow fever, even in the worst forms of the disease. (3)

"Here is a case," says Mills, "of the typhus gravior of Cullen, or such as is commonly denominated putrid. The petechiæ disappeared after the second bleeding, an effect I daily witness from the use of the lancet, which clearly proves that this symptom proceeds from vascular excitement." (4) This is one of the earliest intimations we have of the true pathology of purpura, though the ancients manifestly bled in petechial fevers upon the same principle. It was an induction from vital phenomena, — as it was also, on the part of Parry. By the same process, Lind and others deduced the pathology of scorbutic hemorrhage. (5)

Grant says, that "even in the putrid diathesis of fevers, when much evacuation is required, more or less blood ought to be taken, before proceeding to other evacuations." (6) Dr. Beddoes endeavoured to demonstrate, by dissections, the necessity of bloodletting in fevers. He also employed "leeches by relays of dozens" when inflammation of the bowels was attendant. (7)

Pringle, "putrid" Pringle, bled in all forms of fever, whatever their putrescency. "Bleeding," he says, "in putrid fevers is indispensable. It is the first thing to be done in beginning the treatment." (8) So, also, in intermittents. (see p. 303.)

"Bloodletting," says Tissot "is necessary in four cases. 1st, when there is too much blood; 2d, when there is inflammation; 3d, when inflammation is liable to supervene; 4th, sometimes to relieve excessive pain, which does not depend on the foregoing causes." (9)

Lind, (10) and many of his predecessors, bled largely in the

(1) Bampfield on Tropical Dysentery, pp. 109, 114.
(2) Essay on the Agreement between Ancient and Modern Physicians.
(3) On Diseases peculiar to the West Indies, &c. p. 10.
(4) Mills on Bloodletting in Fevers. p. 156.
(5) Lind on the Scurvy, p. 512.
(6) Grant's Inquiry into the Nature of the Fevers of London, &c.
(7) Researches, &c.
(8) Pringle's Observations on the Nature and Cure of Jail Fevers, p. 35.
(9) Avis au Peuple, c. 32, s. 539. (10) Lind, ut supra.

scurvy, and "considered it of the greatest service." This disease is the great pillar of humoralism,—purpura hemorrhagica being the next in importance, in which bloodletting has now become general.

Baillou advises bloodletting "in all putrid and malignant fevers, even when there is a tendency to hemorrhage from dissolution of the blood." (1)

In a malignant epidemic fever, it was the practice of that terse writer Marteau de Grandvillier, "to bleed four or five times in the arm or leg," and if the symptoms were still unyielding, especially headache, "he repeated the operation upon the jugular vein." (2)

Chirac bled repeatedly in the worst forms of fever. (3)

Dr. Cartwright, the philosopher of our western wilds, bled with success in the "malignant fevers" of Mississippi, to the extent, sometimes, of about 100 ounces at one bleeding, and this when the blood was "dissolved." In his own case, he "lost 64 ounces at one time." (4) And in a far distant climate, Wendelstadt scarce wiped his lancet, in the Weltzer epidemic malignant fever, till he raised his patients from that "excessive prostration that acquired for the disease the appellative of "*putrid*." He found his reward in the ("numerical") loss of but "six patients." (5)

Dr. Burnett, in his account of the congestive fever of the Mediterranean, admonishes the practitioner not to be deceived by the symptoms of prostration, syncope, &c. "It will often happen," he says, "after a few ounces have flowed, that syncope will be induced; but this must not prevent the repetition of the bleeding, while the symptoms require it." (6) A patient whom he bled to the extent of ninety ounces in the course of six hours, was convalescent in three days.

We have cited many eminent observers in former sections, who affirm of their experience, that the prostration of strength which is incident to the early stages of acute diseases is in no respect allied to the debility which follows long continued sickness, and that it is most effectually, and, indeed, only to be removed in

(1) Epidem, t. 1, l. 1, p. 37.
(2) Sur la Fièvre, &c., in Journ. de Vandermonde, 1758, p. 275.
(3) Trait. de Fièvres Malignes, &c.
(4) Cartwright in American Medical Record. vol. 9, pp. 26, 28. 1826.
(5) Hufeland's Journ. t. 4, p. 416. 1795. (6) p. 20.

most instances by bloodletting and the antiphlogistic treatment. This, indeed, is implied by every writer who commends bloodletting in the plague, typhus, yellow fever, &c. Many authors, as we have seen, speak of it specifically. Others mention the exalted action which follows the loss of blood. Thus, Pringle: "In the bilious fever, the pulse was upon the first attack generally depressed, but rose upon bloodletting."(1) "The pulse,' says Moseley, in speaking of the West India fevers, "always rises, the faintness disappears, as the heart is relieved from its oppression by the loss of blood."(2) So, too, Mr. Boyle, of a Sicilian epidemic: he "depended on copious bloodletting. The pulse became gradually fuller and more regular whilst the blood flowed."(3) So, Riverius, in regard to the prostration of strength in the plague: "Ideo enim subito vires corruebant, quod Natura *non posset sine venæ sectione grave onus excutere.*"(4) When we come to the consideration of venous congestion, we shall have occasion to cite many high authorities in favour of decisive bloodletting in all fevers of a congestive and malignant character. The greater the prostration, the greater the burthen of disease, and the more imperiously is the lancet demanded.

The plague supplies us with a profusion of experience in favour of bloodletting, and its tendency to exalt the forces of life when broken down or otherwise severely impaired by acute disease. The ancients generally bled in this complaint; from which circumstance, alone, we may infer their equality, as practitioners, with our own morbid anatomists. Oribasius says he owed his life, when affected by the plague, to the loss of two pounds of blood by scarifications.(5) Even Galen bled in this disease,—though he was in medicine, what Descartes was in philosophy. Such was his success with the remedy, that the spectators exclaimed, σφαξας, ανθρωπε, τον πυρετον; Oh man thou hast cut the throat of the fever! "Memini enim quibusdam," says Galen, "ad *sex* usque libras sanguinem detractum fuisse, ita ut febris protinus extingueretur, *nec ulla sequeretur virium afflicto.*"(6)

Celsus commends bloodletting in pestilential fevers.(7)

(1) Observations on the Bilious Fever, p. 171.
(2) On Tropical Diseases, p. 442.
(3) Edin. Med. and Surg. Rev. No. 24. p. 422. (4) Op. Cit.
(5) Opera, coll. 7. 20.
(6) De Curand. Rat. per Sang. Miss. cap. 14.
(7) L. 3. c. 7. p. 133. etc.

Avicenna bled in "putrid" fevers. He sometimes abstracted, at once, five or six pounds of blood; "nam quidam sunt qui tolerare possunt, etiam si febricantes essent, aut *quinque*, aut *sex* sanguinis *libræ* auferantur." (1)

Bleeding in the plague was a general practice after the revival of learning. We find it advocated by Botallus, Sydenham, Fonseca, L. Mercatus, Septalius, Forestus, Mercurialis, Paschal, Pereda, Lusitanus, Andernachus, Costæus, Altomarus, and numerous other writers.

Septalius states that it was the prevailing usage of physicians to bleed in the plague of 1575 and 1576. "Unde etiam communi consensu in peste hujusmodi nobile remedium nullo modo prætermittendum esse decreverant." Riverius says of bloodletting in a similar epidemic, "sicque omnes, Deo sit laus et honos! quotquot hoc tractili sunt modo feliciter evaserunt."

Susius holds the same emphatic language as to the importance of bloodletting in all fevers, "putrid or inflammatory." (2)

Riverius, like Pringle, considers "putridity a reason for bleeding, and he employed the lancet freely at all stages of *petechial* fevers, "nec ullum inde eminere periculum." (3)

"Omnes acutas et inflammatorias febres hic Romæ," says Baglivi, "curare incipio per sanguinis missionem." (4) Fernel, in France, states the same. (5)

We have quoted a remarkable passage from Ambrose Parey in a former section, (p. 225,) as to his method of employing bloodletting in the plague and other pestilential diseases. It has not been surpassed in modern times, and it embraces the best rule that has ever been offered as to the quantities of blood that should be abstracted in all diseases.

The practice of abstracting blood in the plague has been more or less continued in recent times. Faulkner commends it. "He was assured by one of his hospital assistants, that in a few cases of blood being taken from the arm by himself, it threw up a buffy coat, and the patient did well; by others, that the blood taken in this way exhibited a *very dissolved* state, yet the cases proceeded *not less favourably*." (6)

Assalini says, "when, at the commencement of the plague, I

(1) Canon. l. 4. Tr. 2. c. 7. de feb. putrid. and l. 1 fen. 4. c. 20.
(2) De Sang. Mittend. Ratione. (3) Method. Curand.
(4) Baglivi, Prac. Med. l. 1. c. 13. and c. 15. (5) Method. Med. l. 3.
(6) On the Contagious Nature of the Plague, p. 236.

met with persons of a good constitution, who had decided symptoms of true inflammation, I saw the necessity of making use of bleeding, in proportion to their strength; and I found this happen oftener than I should have imagined." (1)

It was not alone, however, in malignant fevers, attended by great prostration of strength, but where bloodletting was encouraged by vascular action, that our ancestors, more or less remote, practised venesection. It was performed in cases which illustrate a profound knowledge of pathology, — such a knowledge as can only be acquired by a careful and philosophical observation of the phenomena of living actions, — and which can never be developed by the scalpel. We find them abstracting blood for the relief of those syncopes which attend upon the worst forms of venous congestion.

Thus Aretæus, in speaking of congestive fever, "venas itaque in cubito protinus cædito *multumque* sanguinis, sed *non semel* totum mittito; imo et bis, et ter, et alio die, quò interim vires instaurentur, repetito." This practice he again enforces in what is now denominated *typhus syncophalis*, (2) and now abandoned to the bark and wine system. Alexander of Tralles describes our congestive fevers, bleeding where syncope was an attendant symptom. (3) And thus Hippocrates: "In acutis morbis venam secabis, et si vehemens appareat morbus, qui ipsum habeant, *robur ipsis affuerit*." He advises bleeding, and purging afterwards, in all inflammations; "nam venæ sectio in talibus principalis est." (4)

It has been thought remarkable that Hippocrates has never designated the quantity of blood which should be abstracted. The reason is, he was too much of a philosopher.

He knew that no rule, in this respect, could be at all useful; but, on the contrary, both unwarranted by nature, and liable to the worst results. Look at his writings, and you will find him bleeding according to the symptoms, and general history of the case. This, indeed, he often says, was his *rule*. He had no other in relation to quantity.

From what we have now seen of the treatment of "malignant"

(1) On the Plague, p. 42.

(2) De Cur. Acut. Morb. l. 2. c. 3. de Syncope; and c. 7.

(3) L. 12. c. 3. de Syncope. In the quincy, pneumonia, &c. he often bled from three to four times. It was a favourite remedy with him for a "fit of the stone."

(4) De Vict. Rat. In Morb. Acut. s. 4.

and "putrid" fevers by bloodletting, more or less copious, according to the exigencies of the disease, we shall not expect any flinching of the same hands when they come in contact with inflammatory affections of a less depressing nature. This is universally true of those of whom we have spoken. We shall quote the language of one only.

Thus Dr. Jackson: "The quantity of blood which may be abstracted in cerebral fever, without even compromising the safety of the patient's life, exceeds a measure, which, were my experience of the fact not clearly ascertained, I should not venture to put before the public. Four pounds, taken away at one time, may be considered as *moderate* bleeding in the more concentrated forms; six have been taken on several occasions, and even 112 ounces in some. The practice, so formidable in appearance, implied no danger. It saved life by direct effect. The practice is reasonable in theory; it is proved in experience to be founded in truth. The *quantity*, moreover, is to be ***measured by the effect which arises under the act of abstraction,— not by opinion formed under a presumption of what may be right.*** Whatever be the quantity, it is the *effect* produced which constitutes the *rule* for judging the measure. The measure may, perhaps, be carried beyond 112 ounces, without compromising the patient's safety. Instead of danger at the time, or debility as a consequence of such extraordinary depletion, fainting did not always occur; and the patient in most cases, returned to his duty, *within eight days* in full vigour of health."(1)

Chisholm, so little friendly to bloodletting in the West India fevers, states, that in an epidemic inflammation of the liver, when from 40 to 160 ounces of blood were abstracted, the practice "seldom failed." (2)

But we will not multiply examples, where high inflammation might nerve the most cautious practitioner. And from the wide range of climate, and the various eras of the world, which we have passed under review, and the coincidence of practice which has distinguished the most able Hippocratists, it has become unimportant to dwell upon the coincident nature of the same diseases, at all eras, and in all countries. The human constitution, its laws, susceptibilities, &c. in a general sense, are

"(1) Hist. and Cure of Febrile Diseases, vol. 2. p. 137." "Sometimes 160 oz. in a day."

(2) Duncan's Med. Commentaries, vol. 11. pp. 369, 370.

every where the same, and the pathology of inflammation, or of congestive fever, is the same, respectively, in principle, at all times, and in all countries. The principles of treatment, therefore, must be the same under nearly all contingencies. But modifying causes impart various shades of difference to every epidemic,—to every individual case. To understand the simple or complex condition of each case,—what its general nature,—what its peculiarities from various causes,—what the exact adaptation of remedies,—how much the successive changes may be due to nature or to art,—requires unceasing vigilance.

If bloodletting, however, be demanded by simple inflammations, how much more so is it, when inflammations are complicated with idiopathic fever,—and especially when that fever is of a malignant nature, and constantly imparting its malign influence to the local developments.

We have seen that it is a capital error, that copious depletion is only adapted to fevers and inflammations of tropical latitudes. Besides, venous congestion is the principal local development which attends the fevers of hot climates. "It would appear," says Mr. Hunter, "from Dr. Blane's account, that inflammation is hardly a disease of the West Indies."(1) Those who object to the lancet upon the ground of debility cannot defend the doctrine; since, also, the human constitution is most vigorous in temperate climates. The records of medicine abound with confirmations of our statement; and we, of the present generation, are often adding to the more ample illustrations of the past. England has not yet abandoned the lancet; and here, in America, it is, as ever, the anchor of hope in inflammations and congestive fevers. In France, there are some, like Bouillaud, who venture upon a pound or two of blood, made up of successive bleedings, "coup sur coup." The indefinite expression is more alarming than the practice which it records. Broussais is supposed by some to have pursued a sanguinary course, from his universal doctrine of gastro-enteritis; but he rarely went beyond the application of leeches.

But we have stated enough, perhaps, to show, that there is nothing in the climate of France to prohibit the same liberal use of bloodletting which is so advantageously practised in other countries. Still it is thought to be a novel and hazardous

(1) Hunter on the Blood, &c. p. 227.

treatment to employ even moderate bloodletting in acute articular rheumatism. The eighteenth century, however, witnessed a more decisive use of the lancet in the land of morbid anatomy than any other country. We might cite numerous examples, not less remarkable than that which we last quoted in relation to the cerebral fever of the West Indies,—still showing that the same disease is always the *same* in every clime and at every age. The practice of Bouillaud is, therefore, but a feeble resuscitation of what once prevailed. Indeed, in the treatment of rheumatism by bloodletting, France is unparalleled in the annals of our art, if all that has been recorded can come within the limit of rational credibility. Thus, we read in a treatise entitled *Observations interessantes sur la Cure de la Goutte et du Rheumatisme*, published at Paris in 1747, that the writer cures all kinds of rheumatism by large abstractions of blood. He opens a vein in the arm and foot simultaneously, and takes away, within 36 hours, more than *twenty pounds of blood.* He admits that the patients often fainted and had convulsions, before he had abstracted, at one time, four pounds; but, in those cases, "the operator should close the orifice with his finger," and as soon as the subject recovers, he should go on with the operation. (1)

The foregoing statement will prepare us the better for the following:

Dr. Audouin, calling to witness three physicians and other observers, states that, in a violent fever which prevailed at Beaumont sur Oise and other neighbouring villages, he bled the daughter of M. Bignon, the Mayor, 21 times, and nearly to syncope. "The symptoms were not calmed and dissipated till after the last bleeding." A strong lad of twenty was bled within five days, 13 times to syncope, and was able to go out on the 12th day. M. Peteau was successfully bled, in the same way, 15 times. A strong artizan was bled thrice in an hour and a half, and four times more within 24 hours after the attack, and three times within the six following days,—when the disease yielded. The patients were generally bled, each time, to syncope. Two pounds at a time are once mentioned. (2)

(1) Pref. p. vi. and p. 329, &c.

This is manifestly a very exaggerated statement. But it shows us the prevailing practice of the French at that era.

(2) See Beddoes on Fever and Inflammation, p. 171.

The foregoing cases, like the last we cited from Jackson, were evidently attended with phrenetic inflammation, and serve to illustrate what we have said in a former section as to the special influence of the brain, (when the seat of inflammatory action,) upon the vital forces of the system, and upon the phenomena which arise from the loss of blood. The author, whom we quote, very judiciously adds, "it is almost impossible that you should contemplate such positions, without trembling for your discrimination; lest you should employ the measure recommended where it is unsuitable, or omit it where it is required."

Looking back to the illustrious men whose experience we have adduced in favour of bloodletting, as well as to most other similar examples, we shall find them addicted to the practical habits of mankind. They were students, learned, and in possession of genius; but they also mixed with the crowd, and took their active part in the stirring scenes of life. They became familiar with man in all his aspects; and with characters formed for decision by participating in the events of society, and accustomed to mark the varieties in the moral constitution, they were the better qualified to appreciate the varying conditions of the physical, and to carry their habits of business into the chambers of the sick.

It is especially to moral influences proceeding from such sources, that we ascribe much of the acumen of American physicians in observing nature, and of concurring so remarkably in one decided practice, — be it an active or an expectant plan which disease may demand; for decision and enlightened observation may be as necessary in one as in the other. "The American man of letters," says Verplanck, "is constantly called off from any single inquiry, and allowed, or compelled to try his ability in every variety of occupation." "Nevertheless, it may well be, that there are some meditations so subtle and unreal, some laborious arts, &c. that they can find no room amid the strife and bustle, the *forum, strepitumque*, — the railroad noise and rapidity of this work-day world of America. Be it so. We would not willingly lose them." (1)

(1) Oration on the Advantages and Dangers of the American Scholar, pp. 20, 21. Being lately on a visit in consultation with Dr. Brown at Newburgh, N. Y., and finding that he had bled his patient, who was affected with pneumonia, a dozen times within a few days, though much enfeebled by previous bloodlettings and hem-

Should we have had from Dover, the physician and bucanier, the following instructive lesson, had he lived and died within the walls of a Parisian hospital? We state the narrative, for it carries an internal evidence of its truth, is characteristic of the man, and is abundantly sanctioned by the experience of the foregoing masters of our art. He relates, that "not long before he

orrhages from the lungs, we had the curiosity to inquire circumstantially into his habits as to the use of this remedy in inflammatory and febrile affections. We shall state the result of our conversation, as supplying an illustration of the principles which prevail in the interior of this country; premising that Dr. Brown, who is a gentleman of great integrity, learning, and a strong mind, has practised medicine at Newburgh 31 years, with eminent success.

To our interrogatory as to the abstraction of blood in pneumonia, he replied that he had seen a great deal of the disease, always bled decisively at all its stages, and knew of no stage in which bloodletting had not been useful. He had been bled himself for this affection 35 times in 31 days, the loss of blood having averaged one pound at each depletion. Twenty of the bleedings were performed during the first 10 days, when he became convalescent for 13 days; after which he relapsed, and was then bled the remaining 15 times within the following eight days. He bled, successfully, a patient suffering gastritis from arsenic, to the extent of 12 pounds in 21 hours. This patient enjoyed good health for some years afterwards, and lately destroyed himself by opium. Dr. B. considers bloodletting the principal remedy for dysentery, and severe cases of scarlatina. It is his common practice to bleed in the cold stage of intermittents.

An energetic practice, as we shall see in our comparative view of the Hippocratic and anatomical schools, has long prevailed in these United States. Dr. White, in writing of the congestive fevers of Georgia, (1804) remarks, that "a disposition to faint from the loss of a few ounces of blood in the early stage of fever, is an additional proof of the necessity of further evacuation." (See p. 218.) Blood was sometimes drawn "with the most evident benefit on the 22d and 25th days of the fever." (*a*) Our own observation, and experience at large, render it certain that Dr. Craigie lays down an erroneous precept, when he says, "if the patient (in remittent fever) is seen only on the third and fourth day, bloodletting has very little chance of being of much use." (*b*) We notice this remark, on account of the decision with which Dr. C. advocates bloodletting in the early stage of fevers.

Now and then, opponents to bleeding in "putrid fevers," and "pneumonia typhoides" have appeared in America. Among them was the distinguished Dr. Hugh Williamson, (1797.) But, he concedes that his objections were mainly founded upon idle gossip. It is most to our purpose, however, that he admits it to have been a prevailing practice in those affections in the Southern states; and this too, among men "of talents, and well educated." (*c*) We have just given such an example in Dr. White. Puerperal fever was treated actively by the lancet, in this country, certainly half a century ago. In severe cases, Dr. Brickell sometimes bled "three times a day." (1798.) (*d*)

In this instance of Dr. Williamson, we see illustrated the practical bearing of Moseley's objection to the epithets "putrid," "asthenic," and "malignant." (*e*)

(*a*) New-York Med. Repos. vol. ix. p. 252. (*b*) Practice of Physic, p. 189.
(*c*) New-York Med. Repos. vol. ii. p. 145, 2d ed. (*d*) Ibid. vol. ii. p. 13.
(*e*) On Trop. Dis. pp. 101, 137. See our Humoral Pathology, Section V.

took by storm the two cities of Guaiaquil, the plague had raged amongst them." This disease soon broke out on board his vessels. "In less than 48 hours, we had in our several ships 180 men in this miserable condition. I ordered my surgeon to bleed them in both arms, and to go round to them all, with command to leave them bleeding till all were blooded, and then to come and tie them up in their turns. Thus they lay bleeding, and fainting so long, that I should not conceive that they could lose less than 100 ounces each man. Notwithstanding that we had 180 odd down in this most fatal distemper, yet we lost no more than seven or eight." "Now if we had had recourse to *alexipharmics*, &c. I make no question at all, considering the heat of the climate, but we had lost every man." (1)

Here was the same moral firmness, the same self-possession, the same clear-sightedness, and decision, that distinguished this remarkable man in his naval exploits. It was only operating in a new direction. It was an exemplification of the parallel instituted by Hippocrates, when he says: "Qui in tranquillitate navem gubernantes delinquunt manifesti non fiunt; at si ventus magnus, ac tempestas ipsos deprehenderit, tunc conspicui fiunt palam omnibus hominibus, quod ignorantia, et erroris sui culpa navem perdiderunt." (2)

However eccentric the practice of Dover, and however formidable it may appear, it loses much of its apparent rashness when we revert to the no less decisive habits of Jackson, and others, and more especially when we consider that nature will commonly restrain the loss of blood within the limit of safety. Moseley, as we have seen, adverts especially to the enormous quantities of blood which have been accidentally lost from the arm, in cases of fever, and where "the surprise, on discovering a profusion of blood in the bed, has been changed into joy for the alteration produced in the patient." These examples are not infrequent in the walks of medicine.

"It may happen," says Celsus, "that a disease may require bloodletting, when the system seems unable to bear it. Yet, if there appear no other remedy, and that the patient must perish unless relieved even by a rash attempt; it is then the part of a good physician to declare that bloodletting is the last resource of his art, but, that it may precipitate death. Having done this he

(1) Ancient Physician's Legacy. (2) Hippocrates Com. de Vet. Med. n. 17.

should bleed, if desired. There can be no room for hesitation in cases like this; since it is better to try a doubtful remedy, than none at all. And this ought especially to be done, when a paroxysm of fever has nearly destroyed a patient, and another equally severe is likely to follow. So, also, in palsy; and again when angina suffocates."(1)

But these are cases which demand habits of critical observation, often much experience, and an unintermitting attention to medical pursuits. It will be otherwise but little better than the hazard of the die. Without these requisites, where uncertainty prevails in critical conjunctures, it is better to leave the whole matter mainly to nature. In such emergencies, she will oftener triumph than the unskilful practitioner, who may only embarrass her efforts. "Medici plus interdum quiete, quam movendo proficerunt." This principle holds, in the foregoing cases, where art is imbecile from ignorance. Even in Dover's cases, who that has witnessed, without prejudice, the havoc of "alexipharmics," will hesitate to believe that unembarrassed and unaided nature would have come out more triumphantly than the "alexipharmic" practitioner? And so it is, as we have seen, with inadequate bloodletting.

It has been well remarked by Dr. Billing, that "in practice there should be no such thing as boldness or timidity." But, there should be moral courage, which always implies the highest discipline and exercise of the understanding; and there are probably few who have not felt that it is as necessary in withholding as in administering remedies.

It would be useless to pursue this inquiry to any greater extent. If what has now been said be insufficient, more would certainly be irksome. Still, we shall be obliged to refer to other coincident experience, in our Essay on the Humoral Pathology.(2) We justify ourselves in the collation of past observation by the importance of our subject, and to illustrate our principles; nor have we felt any disposition to offer our opinions without casting about for support. True, it may be all familiar to the

(1) L. 2. c. 10. (2) See, particularly, section 5, on Humoralism.

learned of the profession; but can it be too often brought to our attention, and enforced upon our consideration? Or shall we say with M. Louis, after contemplating all the enlarged and exact observation by men of lofty genius, and high erudition, to which we have now adverted, and the vast amount of corresponding experience at which we have scarcely hinted,—shall we venture to affirm, that "men have for ages devoted themselves to therapeutics, and the science is still in its infancy;" that, physicians "scarcely agree, except on points which are admitted without any examination, or as established by long usage, which has nothing to recommend it but time;" that, "the reader will be astonished, undoubtedly, that in the nineteenth century, authority could have been invoked in a science of observation, without remarking that that which we call experience, *even now*, is nothing but authority. In fact, to what authorities do those, most celebrated for the wisdom of their precepts, refer, unless it be to the practice of their predecessors," &c.; that, "if the experience, so justly scorned by Quesnay, is an uncertain guide in practice, it is that it possesses nothing of true experience but the name; that it is, in truth, only the common usage, not justified by rigorous observation; authority, in a word;" that, "the pretended experience of authors is worth nothing, and after all their assertions and denials, we are no further advanced than before;" that, "the experience, to which he refers, is evidently tradition, custom, common belief,—an almost worthless thing,—a compound of vague recollections;" that, "in citing authorities, and amongst others, that of Pinel, he might as well have confined himself to a simple denial; for what is authority in medicine;"—or, finally, "let those who engage hereafter in the study of therapeutics, pursue an opposite course to that of their predecessors." (1)

How imposing the contrast, when we advert to the modest declaration of Hippocrates, the true founder of the Baconian philosophy, that "I esteem it a great part of the art of medicine to consider well what has been already written. For it seems to me, that whoever knows and uses the experience of the past cannot make any great mistakes in his profession. But he must be acquainted with the disease, its remote causes," &c. (2)

We are certainly amongst the last to object to independent

(1) Louis on Bloodletting, pp. 64, 77, 79, 89, 90, 96.
(2) De Morb. Epidem. l. 3. s. 3. Ægrotus xvi, ver. 358—361.

thinking; and it is in this spirit that we have felt at liberty to examine the opinions of others on matters of vital moment to society. But every author, in objecting to others, should exhibit the ground of his conclusions, either by a rational appeal to philosophy, or to specific facts. The latter will consist mainly of practical results, in the treatment of disease. They are the great test of medical principles; and how far our author and his disciples may be justified in their condemnation of the past may be easily gathered from the brief review which we have taken in relation to the remedial effects of that agent, whose inapplicability to disease it was mainly our author's object to illustrate in his essay on bloodletting. And we shall be still better informed of the right, when we come to a more critical analysis of our author's application of his own principles, and witness the imposing contrast between the results of his practice, or such as he has commended, and those of the authors whom he condemns with so little reserve. If there be any ground for our author's declaration that "medicine is still in its infancy," we shall then see the nature of the means by which this state of "infancy" has been reproduced. We abide in the scriptural apothegm, "by their fruits ye shall know them."

There have always arisen extraordinary revulsions in pathological views, and reverses in practice not less unaccountable. They distinguish particular eras of the world; and where there is a general dereliction from the best experience of the past, it appears to exert a contagious influence upon genius itself. Thus, Mr. Hunter says, he "remembers when practitioners uniformly bled in putrid fevers;" and yet when he made this statement, the most enlightened were as uniformly employing bark and wine. (1) Theory and experience governed in one case, hypothesis in the other. The former supposed that there was a great amount of local congestion, or inflammation, which gave rise to an appearance of debility by prostrating the forces of life. The latter, leading to stimulants and tonics, recognized but little other difficulty than a putrescent tendency, arising from a vague notion of debility, and a supposed sedative power appertaining to the remote causes. Could we maintain the conviction, that, "on the first attack of inflammatory fever," in the language of Dr. Armstrong, "the strength of the patient is not subdued,

(1) On the Blood, &c. p. 227.

but simply prostrated," the suggestions of true pathology, and the instruction of former experience, would preserve their ascendency. However we might be sometimes baffled, we should still appeal to experience, and to fundamental principles, that our efforts had been defeated rather by the irresistible nature of the disease, than by any injudicious application of the means.

But one of the best evidences, that the true philosophy which respects the treatment of the great classes of inflammatory and febrile diseases was well ascertained by Hippocrates, and his immediate successors, is the undeniable fact, that all departures from that philosophy are of short duration. They are, and will remain the models, in the whole outline of treatment, even to the subject of diet itself, as much as the Grecian architecture, and Grecian sculpture, or Grecian poetry, or eloquence, will continue to be the true models of taste through all coming time. The reason is, that the philosophy of medicine, like the rules of taste, has its foundation in nature, and that, of all her institutions, medicine is the most intensely interesting. The master-spirits of antiquity observed nature correctly, and drew their inductions from this only true source of correct knowledge. They must therefore remain impregnable against all the adversities of time. This may be discouraging to the impulses of ambition, and to the spirit of inquiry. But it should be exactly otherwise. With the rules of philosophical research plainly before us, and with many fundamental principles, observation is facilitated, the avenues to truth are illuminated as by a torch, and we have little of that groping in darkness which slowly emerged into the lights of science. Nature, too, is inexhaustible; and the highest aspirant, or the more disinterested philosopher, by taking the path of nature, will always find something enduring to appease his desires, or to enlarge the boundaries of knowledge. But he cannot often hope for materials that shall serve as the basis for great fundamental principles. The human mind has been too long in active operation to admit of much encouragement upon this score. The fabrics of philosophy may be mutilated; but time will soon repair the breach, and the offender will find his proper place in the archives of history. Where the foundation had been substantially laid, the innovations of false philosophy are like the momentary pelting of the storm upon the "house that is built upon a rock." This, in recurring to our subject, has been often illustrated in the therapeutical history of inflam-

matory and febrile affections. No sooner, for instance, had Mr. Hunter announced the substitution of the stimulant for the depletive treatment in his day, than we hear from Robert Jackson, that "abstraction of blood in contagious fever, which, but a few years since was viewed with abhorrence, even branded with the epithet of murder, is *now* considered the main engine of successful treatment." (1) "Dr. Spence went farther than his cotemporaries; but he did not go the length of arresting the course of the fever suddenly and decisively by the practice he recommended." (2)

It is affirmed by Dr. Craigie, that "for destroying the prejudices against bloodletting, (in bad forms of fever,) and showing the effects of a prompt and energetic system of depletion, we are almost entirely indebted to Dr. Irvine, who, in his interesting work on the diseases of Sicily, has shown in 1808 and 1809 the beneficial effects of depletion, general and local." "Dr. Irvine has, indeed, traced an outline of treatment, the result of experience, to which may be ascribed a great and beneficial revolution in the management of fevers in modern times." (3)

To Dr. Irvine, however, can only be due the credit of having been probably familiar with the experience of our ancestors, and of directing his genius to the great purposes for which it was designed.

Again, our author remarks, that "the example of Dr. Irvine has been successfully followed by Jackson in the West Indies, Denmark, and Burnett in the Mediterranean fever, and numerous practitioners in the East Indies and colonies. By Mr. Boyle and others, on the west coast of Africa, it has been slightly modified to suit the climate, but not materially altered."

Jackson's practice of bloodletting was so incomparably greater than that of Dr. Irvine, if it were not original with him, the reader will find some other more probable guide among our authorities. "In most of Irvine's cases the quantities were 15 or 20 ounces, repeated more than once." But it is manifest from the writings of Jackson, as well as of the numerous authors whom we have cited, that neither he, nor they, were passive followers. They, doubtless, like true philosophers, availed

(1) Jackson on Contagious Fever, p. 232.
(2) Jackson's Hist. and Cure of Febrile Diseases, vol. 1. p. 226.
(3) Craigie's Practice of Physic, p. 183.

themselves of past observation; but they adopted no creed, pursued no practice, without a thorough consultation of nature.

Whilst, however, Irvine was cautiously abstracting blood in Sicily,—Jackson, (1)Moseley, and Hillary, had already astonished the world by the decision of their practice in the West Indies; and Cleghorn at Minorca; Pringle on the marshes of the Netherlands; Senac, Tissot, and others in France; Grant and Beddoes, and their compeers in England; Wendelstadt, and others in Holland and Germany; and here, in America, Rush and his cotemporaries had equally revived the practice of their ancestors, and more than Irvine had illustrated the power of bloodletting over congestive, and inflammatory, and malignant fevers. Irvine was therefore last, as well as least.

But we are glad to enlist in our cause so valuable an auxiliary as the author from whom we have just quoted. "The first measure, therefore," says Dr. Craigie, "to be taken in *every case* of remittent, is abstraction of blood from the arm, &c., to the extent of 15 or 20, or 25 ounces, according to the strength of the patient," &c. "In Great Britain, 25 or 30 ounces are generally necessary;"(2) thus adding another proof that the climate of Great Britain has remained without change, and that the same disease requires universally the same principles of treatment.

SECTION XIV.

BLOODLETTING IN INFANCY AND OLD AGE.

We shall devote a few words to the subject of bloodletting in infancy and old age, mainly in the way of presenting the experience of a few able practitioners.

M. Piorry carries bloodletting in the cerebral inflammations and congestions of infants to a great extent,—entirely beyond any thing which we have ever witnessed. He employs from one to several venesections, and 20 to 50 leeches to the head,

(1) At Jamaica from 1774 to 1778; but especially at St. Domingo from 1796 to 1801. (2) Craigie's Op. Cit. pp. 184, 186.

with purgatives, &c. (1) Such is the fatality of infantile phrenitis, and such the ability to bear the loss of blood in cerebral inflammation, that the remedy should have no limit short of affording relief. There is no disease in which efficient bloodletting is so necessary, and at no age so much, as in infancy. Again, it is the experience M. Piorry, that "in many young children affected with trachitis, large evacuations of blood have enfeebled them but little;" though, on the other hand, "excessive hemorrhage has sometimes produced convulsions."

Sydenham says, that "bloodletting may be as safely performed in young children as in adults, and in some of their diseases, there is no curing them without it." (2)

Rush thinks, "that bloodletting is more necessary in the diseases of infants, under equal circumstances, than in adults." He was an unhesitating advocate of bloodletting in inflammatory affections at all stages of infancy. (3)

So, also, G. Baillou "In hoc peccasse videntur medici, quod ætatula impediti, venam non secuerunt. Nunc scire oportet in pueris etiam, si assiduitas febris sit, tuto venam aperiri." "— debet sanguis mitti, nec dehortari debet ætatula." (4)

Forestus derives an indication to the same effect from spontaneous hemorrhage;—"quinimo sæpe lactantes infantes videmus, quibus sanguis e naribus absque noxa ulla effluit." (5)

Evanson and Maunsell think that "in the child more particularly, bleeding is required in the first stage of all acute inflammations. It may be practised with safety in the youngest infant, provided we hold in view the relation between the necessities of the case and the strength of the patient." "The buffing of the blood is not a safe guide in the child; as we have diseases absolutely requiring bleeding, (*e. g.* croup, bronchitis, &c.) which seldom produce the appearance in question." (6)

The records of medicine abound with a similar experience. (7)

(1) De l'Irritation Encéphalique des Enfans, in Rap. Med. Chir. Brux. Août. 1837.

(2) Sydenham, p. 166.

(3) Rush's Sydenham, p. 167, *note*, and his Medical Obs.

(4) Epidemiorum et Ephem. l. 2. p. 105. (5) Op. Om. l. 1. Obs. 21.

(6) On the Management and Diseases of Children, ch. 3. s. 9.

(7) Of the older writers, see—Cardan, Contrad. Med. l. 2. tr. 3. n. 17; Zacutus, Prax. Admirab. l. 3. Obs. 3; Marchettis, Observ. p. 68; Albertus, de Venæsec. infantum et puerorum, 1724; Morand an pueris laborantibus acute venæ sectio? 1648; Læber de sang. miss. ejusque ulititate, 1728; Munster, Disput. περι παιδοφλεβοτομιας,

At an early era, bloodletting was practised in infancy as fearlessly as at adult age.

As to our own habits, they have always been uniformly one way. If inflammation affect any important organ, or be otherwise attended with danger, and it seems not likely to yield at once to milder treatment, we take no risk, but resort, without delay, to the "remedium principale." We hold that it may be more important in infancy, under equal circumstances, than at any other age; and this ratio increases as we ascend to the hour of birth. Pneumonia, phrenitis, venous congestion of the brain, enteritis, croup, and some other grave affections, hold a rapid march, speedily make their deep constitutional impressions, and may be beyond the reach of art whilst we are deliberating whether we shall bleed, or devote another six hours to other remedies.

The same susceptibilities of the constitution of infants to disease, and to its rapid advances, render them, also, peculiarly sensible to remedial agents when timely and happily applied. They now operate speedily and with power. Hence it is, that milder means, which fail at adult age, may succeed, under the same apparent circumstances, in infancy. But since the dangers of disease are greater, and there is less time for delay in the latter instance, we should be sure of the right, before we decide on neglecting or procrastinating the more vigorous treatment.

Again, suppose a case of pneumonia or croup, where it is not clear whether we should bleed, or give an emetic. In such a case of doubt we always abstract blood, either by the lancet or by leeches, not only for the reasons already assigned, but on account of the prostrating effects of emetics, or nauseating remedies, which will more or less interfere with a loss of blood that may be ultimately necessary.

Still it should be borne in mind, that strong impressions are most readily made in infancy, and that an emetic, or some other remedy, may become a substitute for a certain quantity of blood, whose abstraction, in the same condition of disease, would be indispensable at adult age. These are cases which require nice discrimination; but for the reasons already stated, nothing should be left to the hazards of chance. But it is just other-

l. 5, 1604; Wegbecker, diss. de venæ apud infantes sectione, 1749; Harris de morbis acutis infantum, p. 22, etc.; Select. Med. Francofurt, t. i. p. 332; La Motte, Chirurg. Obs. 106.

wise with us in respect to active internal remedies, where they may be of any questionable utility. We delay them till we have farther light.

In the early stages of inflammation we have been sometimes astonished at the profusion of blood which infants have sustained without injury, from the obstinate bleeding of leech-bites. But, in these cases, the inflammation has been severe, and has affected some important organ. On the other hand, when the inflammation affects a less vital part, an excessive loss of blood, especially by leeches, has been sometimes disastrous; and, it is mainly at this age that practitioners have witnessed fatal effects from loss of blood artificially produced. Still, when infants survive great losses of blood, they recover much sooner from its effects, than adults; at least, this is our experience, and we think the reason may be found in physiological facts.

As we have intimated in our remarks on the philosophy of the operation of loss of blood, we commonly prefer leeching to general bloodletting in *early* infancy, and for reasons which are there assigned. In inflammations and congestions of the brain, however, experience, as well as philosophy, prompt us to the general remedy. But it is well to remark, that if effusion and pressure have taken place, the vital forces become so prostrated by the cerebral influence, that bloodletting is now imperfectly borne; and such is sometimes the case in the early stages of venous congestion of the brain, where, from the amount of sanguineous accumulation, probably, the same pernicious effect is determined upon the powers of life. Here the abstraction of blood must be very moderate at first.

Finally, we believe that all will agree with Lommius, that "it is much more eligible to snatch a child, by means of bloodletting, from imminent danger of death, however the strength may be wasted, than to let him perish by the violence of the fever."(1) And so Celsus.

Strong prejudices exist against bloodletting in all the diseases of old age,—excepting apoplexy; and here the remedy is employed at the beginning of the paroxysm, without that discrimination, which, it appears to us, is often important.

It has been the result of the best experience, however, that

(1) On Continued Fevers, p. 5.

old age, *per se*, constitutes no objection to the remedy. But were there even hazard in its use, its possible dangers are incomparably less than those of many acute diseases which now so readily destroy.

"In bloodletting," says Celsus, "the physician should not so much consider the age, as the strength of the patient." (1) So, also, Hippocrates, (2) Galen, Trallian, and other ancients. Galen, however, although he advocated bloodletting in the inflammatory diseases of old age, discouraged it in childhood.

Wepfer states that it is a very prevailing custom amongst the Swiss, even at eighty and ninety years of age, to resort to bloodletting once a year, or oftener, as a prophylactic. (3)

Forestus bled the aged without hesitation, — "firmus puer, et robustus senex, tuto curantur." (4) So, Lommius. (5)

F. Hoffman remarks, "in senili ætate interdum magis necessaria sanguinis missio, quam alia ad morbos grandævis familiares arcendos ipsam que mortem protelandum." "Venæ sectio sepius senibus utilissima, imo ad longævam vitam confert." (6)

Van Sweiten, a writer most unhappily neglected, considers bloodletting adapted to the extremes of age. "Quamvis autem in tenella ætate, senio, venæ sectionem adhibere metuerint medici, hodie tamen constat certi observationes etiam in his venæ sectionem tuto instituti posse, tali tamen quantitate ætas et vires indicant, uti alio loco jam notavi." (7)

"I would guard you," says Vitel, "against the counsel of those physicians who would dissuade you from bleeding the aged, who may suffer inflammatory or eruptive fevers. The fear of debility is unfounded. Bloodletting is as necessary to them, as to the young, and not less beneficial." (8)

Foucart, after failing with tonics and stimulants, resorted to bloodletting in the inflammatory affections of old people. He no longer witnessed "*la développement de l'Adynamie.*" Intermissions and other irregularities of the pulse formed no objection to bloodletting. (9)

Sir G. Blane adds his conviction of the safety and utility of

(1) L. 2. c. 10. (2) Aph. s. 1. a. 23, etc. (3) De Apoplexia.
(4) Op. Om. l. 1. Obs. 21. Sch. (5) On Fever, p. 61.
(6) Opera t. 1. l. 1. s. 1. c. 3. and t. 5. p. 346.
(7) Comment, &c. t. 3. p. 35.
(8) De la Sangsue Médicinale, c. 8. p. 565.
(9) Archives Gén. Juil. 1824.

bleeding the aged, for inflammatory diseases. He mentions an instance where a quart of blood was suddenly lost by spontaneous hemorrhage from the nose, in a lady of 82 years of age. "It was followed neither by faintness nor weakness, but by improvement in health, in point of vigour and alacrity." In another individual of 100 years, an attack of *pneumonia* was removed by bleeding freely from the arm. (1)

Rush (2) and Hosack (3) consider bloodletting especially indicated in plethoric and inflammatory states of old people. The former "has nothing to say upon the acute diseases of old people, except to recommend *bleeding* in those of them which are attended with plethora, and an inflammatory action in the pulse." The latter deprecates the prejudice which exists against the remedy in old age. "We have before remarked," he says, "that the use of the lancet is especially indicated in the *plethora* of advanced life, when the excretions are necessarily diminished from the diminished action of the excretory vessels. We cannot withhold the expression of our surprise, that this opinion should be resisted by the practitioners of our city, as we observe it has been in a variety of instances; and by men, too, whose education, and opportunities of observation, should have given them different views of this subject, and have led them to different practice." The doctor speaks of three instances, in which "the physicians objected to bloodletting, giving as the reason of their objection, that *dropsy* would inevitably be the consequence of bleeding!" Hosack was ultimately called; when "the patients were all bled, not only once, but repeatedly, and copiously too, and are now in the enjoyment of better health than they have experienced for years."

It is manifest, from what was stated of Rush's experience of bloodletting in infancy, that he considered the remedy most important at the extremes of life; for, in another work he says, "experience proves that bloodletting is more necessary, under equal circumstances, in old age, than in any other." (4)]

Hourman and Déchambre find that bloodletting is more salutary in the pneumonic and other inflammations of the old women

(1) Medical Logic, p. 80.

(2) Medical Inquiries and Observations, vol. 1. p. 453.

(3) American Med. and Philosoph. Register, vol. 3. p. 387.

(4) *Note* to Cleghorn's Diseases of Minorca, c. 6. p. 166. Cleghorn paid no respect to age in the use of bloodletting. See ibid. pp. 114. 166.

of la Salpêtrière than tartarized antimony. It was also the result of M. Piorry's experience that aged men bore the same abundant bleeding, (des saignées abondantes,) as the old women of la Salpêtrière. (1) Frank cured an octogenarian of pneumonia by bleeding him nine times. (2) Gui Patin cured his father of pneumonia, at the age of 80, by bleeding him freely from the arm, eight times. (3) Fréteau bled at the age of 70, to the extent of four pounds in six days. (4) Guersent commends bloodletting in old age. He employed it successfully in inflammations at 87 years.

In the Miscellanea Curiosa are numerous examples of the successful application of bloodletting at 80 and 90 years of age, and of large and salutary spontaneous hemorrhages, at advanced life. (5) Other remarkable instances have been already cited from Lancisi and others. Lancisi protests against arresting hemorrhages from the nose, when occurring at advanced life,— "etinim rarissime ex hujusmodi sanguinis fluxibus decessisse; contra vero sæpe ex violentia illorum suppressione ceterem morbum citamque etiam mortem inferri, comperimus." (6) This precept in relation to all spontaneous hemorrhages, at every age, when not absolutely excessive, had its origin with Hippocrates; and has stood the test of all experience. The Brunonians have looked on with admiration, when nature has thus taken the treatment upon herself.

Thus might we go on with many other coincident authorities. (7)

Physiology co-operates, here, with experience; since we know that the vital powers are now most incapable of sustaining any of the lesions which endanger life at earlier ages. They are approaching their natural extinction, and are readily abolished by disease. They have now lost much of their susceptibility to the ordinary effects of remedial agents. Changes from a morbid

(1) Collection de Mémoires, p. 224. (2) Clinique Méd., t. 2. p. 380.
(3) Lettres, t. 1. p. 100. (4) Trait. sur les Emissions Sang. p. 24.
(5) An. 9, Obs. 222, etc. An. 3, Obs. 143. and 1673, p. 365.
(6) De Sub. Mort. p. 22.
(7) Of the older authors who have written systematically on this subject, see Albertus de Venæsectione Senum, 1724; Coschwiz, Diss. Venæsectionem post quinquagessimum annum in utroque sexu, præsertim sequiori *maxime proficuam*, 1725; C. A. Bergen, Diss. de Venæsectionis usu in senibus, 1726; Commerc. Liter. Nor. 1731, p. 191; Büchner, Diss. de Plethora senum ejusque rationali Therapeutica Tract. per V. S. 1750, and Diss. de rite determinanda Quantitate Sang. etc. 1749; Quesnai Observ. sur les Effets de la Saignée, 1730, and Art de Guérir par la Saigniée, 1736.

to a healthy condition, are slowly determined, — save only by that remedy which makes its powerful, instantaneous, and simultaneous impression upon the instruments of vital action throughout the system. In every part the forces of life sustain a deep and abiding effect. Their condition is directly and instantly altered in the instruments of disease, and this alteration is maintained by the new sympathetic influences which are determined by other parts, as well as by the continued operation of a diminished volume of blood, and an equalized circulation. The secretions break forth ere we bind up the arm; and thus nature comes to our aid by another efficient process. It is all the work of a moment; and the great revolution begun in every part, it may, and often does, terminate speedily in health.

In the formidable diseases of old age, therefore, the remedies must be such as shall reach profoundly the forces of life, and reach them without delay. Such as would be insufficient in youth must surely fail when declining nature is least disposed to co-operate with art. (1)

SECTION XV.

ON BLOODLETTING IN APOPLEXY.

It is the well directed application of bloodletting which constitutes the principal means in the treatment of sanguineous apoplexy; and although it may be often important to delay the abstraction of blood, this remedy will be generally necessary in the progress of the cure. Such, indeed, is the concurring sentiment of almost all writers of eminence; although it is a remarkable fact that the practice is not founded upon successful experience, or any agreement in pathological views. Even those who condemn bloodletting in pneumonia, or other grave inflammations, are neither intimidated by age, nor by expiring nature, when apoplexy makes its invasion. Some are prompted by a supposed

(1) The following note to Kentish's case (p. 292) was omitted. Dr. Borland, a hospital surgeon at St. Domingo in 1796 and 1797, cast away the tonic and stimulant plan which had prevailed, and employed bloodletting and cathartics for the cure of ulcers. By these means, "he often succeeded," says Jackson, "even in persons who were emaciated to the last degrees of emaciation by the continuance of the disease." Jackson's History and Cure of Febrile Diseases, vol. ii. p. 202.

rupture of a vessel, which they expect to staunch by bleeding from another; whilst a few, more philosophical, regard the effusion as the result of a morbid process analogous to secretion.

Amongst the most unreserved in the use of the lancet, are Portal, (1) Abercrombie, (2) Mossman, (3) Stahl, (4) Howship, (5) Burserius, (6) Bœrhaave, (7) Van Swieten, (8) Morgagni, (9) Harvey, (10) Quarin (11) Cheyne, (12) Cruveilhier, (13) Macbride, (14) Avicenna, (15) Mead, (16) Baglivi, (17) Lancisi, (18) Sylvius, (19) Pitcairn, (20) Horstius, (21) Rochoux, (22) Nymann, (23) Capivac, (24) Bayle, (25) Ætius, (26) Tissot, (27) Hunter, (28) Cooke, (29) Delavautérie, (30) Normand, (31) Montain, (32) Richond, (33) Granier, (34) Galen, (35) Ægineta, (36) Sennert, (37) Willis, (38) Foderé, (39) Stokes, (40) Vogel, (41) Bell, (42) Sauvages, (43) and most others.

Some of the foregoing authors maintained that apoplexy, consequent on extravasated blood, was always fatal. A few others, discouraged by the failure of bloodletting, have exclaimed with Duretus, "non decet eum attingere, qui servari non potest;" (44) or, like Kirkland, who, finding that "death had invariably happened in the strong apoplexy in every case he had

(1) Sur. l'Apoplexie, p. 405. (2) On Diseases of the Brain, &c. p. 237.
(3) Med. and Phys. Journ. vol. 7, p. 307. (4) De Apoplexia.
(5) Observations in Surgery and Morbid Anatomy, p. 65.
(6) Institutions in Medicine, vol. 4, s. 123 and 125.
(7) Aphor. 1033, 1030, 1031. (8) Comment. t. 3. p. 308, etc.
(9) De Sed. et Caus. Morb. Ep. 2, s. 16, and his Advers. Anat. Animad. 83.
(10) Zimmerman on Experience in Physic, vol. 2, p. 59.
(11) Animad. Prac. p. 1. (12) Cases of Apoplexy, p. 13.
(13) Anat. Patholog. Livrais. 5.
(14) Introduction to the Theory and Practice of Phys. p. 563.
(15) Canon. de Med. etc. l. 3, fen. 1, c. 14, p. 215. (16) Medical Works, p. 480.
(17) Prac. Med. l. 1. p. 102. (18) De Sub. Mort. *passim.* (19) Op. Med. p. 433.
(20) Element. Med. p. 126. (21) Op. Med. t. 2, p. 76.
(22) Récherches sur l'Apoplexie, p. 449. (23) Tract. de Apoplexia, p. 14.
(24) Prac. de Cognat. et Curat. affect. Capitis, l. 1. c. 17.
(25) Tract. de. Apop. cap. 11. (26) Tetrabi. Secund. Serm. 2. p. 287.
(27) Prac. Obs. on Apop. &c., p. 80, and Avis au Peuple, c. 9, p. 114.
(28) On the Blood, &c. p. 214. (29) On Nervous Diseases, p. 141.
(30) Dis. sur l'Apop. p. 35. (31) Dis. sur les Hémorrhagies cérébrales, &c. p. 25.
(32) Trait. de l'Apop. pp. 118, 130, etc.
(33) De l'Influence de l'Estomac sur la Production de l'Apop. p. 151.
(34) Trait. sur l'Apop. p. 86.
(35) De Victu Rat. in Acut. com. 4, n. 28; and de Art. curat. ad Glaucon. c. 15.
(36) De Re Med. Apop. (37) Med. Prac. l. 1. (38) Cereb. Anat.
(39) De Apop. p. 165. (40) Lectures on the Theory and Prac. of Med. p. 265.
(41) Op. p. 461. (42) Principles of Surgery. (43) T. 3, p. 452.
(44) In Coac. Hip. p. 370.

known or heard of for fifty years," resolved to moderate his bloodletting, and to push the dangerous experiment of "lessening irritability by opium," (1)—when, too, irritability is already in the wane. So, also, Falconer. (2)

Aretæus deliberates with particularity upon the quantity of blood that should be abstracted in the strong apoplexy. (3) Hippocrates (4) bled with consideration. Celsus was equally aware of the necessity of caution,—"sanguinis detractio vel occidit, vel liberat," (5) Zulian, (6) and Wepfer, (7) expatiate upon the subject. Darwin, (8) and Fothergill, (9) and Heberden, (10) are remarkable for their temperate measures. John Brown, (11) of course, dissuades from bloodletting; and Gay (12) condemns it *in toto*. Ballonius (13) would be governed by the symptoms; as also, would Holland, (14) Lieutaud, (15) Philip, (16) Lettsom, (17) Mackintosh, (18) Copland, (19) Barbett, and Clutterbuck. (20) All these observers believe that there are cases of sanguineous apoplexy to which the lancet is not adapted, at least in their early stage. Mackintosh remarks, that "some routine practitioners will be found invariably to bleed in apoplexy, without reference to the period of the disease and the state of the pulse; and I have little doubt from what I have seen, that valuable lives are occasionally lost, which otherwise might be saved by avoiding the lancet."

Clutterbuck and Copland have rendered an essential service by their valuable papers on apoplexy in the popular works to which we have referred in our margin. The former says, that "there is perhaps no disease, the treatment of which requires to be so much directed by theory or general principles

(1) Commentary on Apoplectic Affections, pp. 38, 39, 49, 96.
(2) Mem. Med. Society. Lon. vol. 2. (3) De Caus. et Sig. Morb. Acut. l. 1. c. 4.
(4) Aph. 42, s. 2, and de Popular. l. 4. (5) Celsus, l. 3, c. 27.
(6) De Apoplexia præsertim nervosa, p. 118—178.
(7) Exercit. de loco affect. in Apop. and Hist. Apoplecticorum.
(8) Zoonomia, vol. 2. p. 402.
(9) Works, vol. 3, p. 208—210, and Lon. Med. Obs. and Inq. vol. 6, p. 680.
(10) Med. Trans. vol. 1, p. 471. (11) Elements of Med. s. 640, etc.
(12) Vues sur l'Apoplexie, p. 61. (13) Consil. Med. l. 2, hist. 1, &c.
(14) Medical Notes and Reflections. Affect. of Brain.
(15) Synop. Med. t. 1. p. 150.
(16) On Diseases, &c. p. 77, and Exp. Inq. p. 288.
(17) His edition of Fothergill's works, p. 584.
(18) Elements of Pathology and Prac. of Phys. vol. 2, p. 89.
(19) Dic. of Prac. Med. Art. Apop. (20) Cyclopædia of Prac. Med. Art. Apop.

as apoplexy. The practice in general use is, for the most part, unnecessarily violent; and in some respects contradictory. Bloodletting to an unreasonable extent, vomiting, purging, blistering, sinapisms, and a great variety of other stimulants, have all been administered with an almost indiscriminate and unsparing hand; as if, to insure recovery, it were only necessary to have recourse to sufficiently active means, without much regard being paid to their nature or effects." (1)

It appears to us, that there is no disease which requires so much skill as apoplexy in some of its instances, as it respects the application of bloodletting. It is often impossible to understand the exact pathological condition of the brain. If hemiplegia attend, it is almost certain that extravasation of blood has taken place. This, we hold, with the rare exceptions where a rupture of a large artery has followed disease of its coats, is indicative of venous congestion of the brain, with which inflammation may coexist. We have, therefore, in these numerous instances, a formidable condition of cerebral disease, and a laceration of the cerebral substance. Again, however, there may be only a state of venous congestion, or of serous effusion, or some pathological condition which is not denoted by any visible signs after death. With the exception of paralysis, the phenomena may be exactly the same in all these conditions of the disease at its invasion. In the two first varieties, bloodletting sooner or later, is probably necessary in almost every case, to overcome the morbid action. In the two last, which are known as the *serous* and *nervous* apoplexy, the loss of blood is comparatively unimportant, and may be injurious, at every stage of the disease.

But the treatment of apoplexy has been less the fault of theories than their unmitigated application,—neglecting the peculiar relations which the brain sustains to other organs, and the consequent modification of their forces and functions when the brain is suddenly and violently disturbed. So far as this organ is concerned, whether the proximate cause of apoplexy consist in pressure from *excreted* blood, simple inflammation or congestion, bloodletting is clearly indicated; and to avert an impending attack should be applied without much reserve. But when the

(1) *Ut supra.* Although this page is now with the printer, we cannot forbear referring to the valuable experience of Dr. Holland, on the foregoing subject, in his late work, entitled "Medical Notes and Reflections." 1839.

paroxysm ensues, it is not alone the brain which suffers in a new and peculiar manner. Every vital organ has sustained a shock, and each becomes a subject for particular care. Disease is now coextensive with the system, for the powers and functions are universally deranged.

We have seen that slight variations of experiments upon the brain will produce the most unexpected differences in their results; that a sudden blow will immediately extinguish organic life, whilst the whole organ may be gradually removed without greatly affecting, for a certain time, the general functions of the body. These facts lead to the induction that natural causes, acting on the brain, may produce the greatest variety of effects on animal and organic life. We know, indeed, that natural causes operate in the same way on the human subject, — the brain having gradually sustained most extensive lesions, with scarcely a symptom to denote their existence; whilst, at other times, a far slighter injury, when suddenly inflicted, has immediately extinguished life. We shall state in another place some remarkable instances of the former; and as examples of the latter, there is a case by Hennen, where death was "instantly" produced by a ramrod penetrating one inch through the orbit of the eye; (1) and a parallel one in the Ephemerides German. (2) where immediate death arose from a small splinter which entered the brain through the inner canthus of the eye. The differences, which are presented by other cases of similar injuries, tend equally to illustrate the remarkable influences of the brain upon the vital forces, and how those influences are varied by apparently slight causes. The results, indeed, are wholly more diverse upon the human, than upon the animal, brain. This grows out of not only the natural differences betwixt the constitution of man and animals, but from constitutional peculiarities which appertain to every individual of the human race. So far, however, we have only regarded man in his natural condition; but when he becomes the subject of disease, the cerebral influences are modified without end; and especially will injuries of the brain determine an incalculable variety of effects upon the vital forces of the system, if the organ itself be the direct seat of disease. The general results will then depend upon natural constitution, the precise nature and extent of the cerebral disease, the exact condition of

(1) Military Surgery, Case 38. (2) An. 1677, p. 140.

the various organs of the system, and how they may be prepared to disseminate disturbing sympathies, and lastly, upon the nature and extent of the injury which may be inflicted upon the brain. It is evident, too, that much will depend upon the exact part of the brain which may sustain the violence,(1) and this will be varied by its connection with any of the foregoing conditions. Whilst, therefore, we have just seen life extinguished by a moderate puncture of the brain, we are not surprised, on the other hand, to witness recoveries from incomparably greater and equally sudden injuries.

The varieties in the cerebral influence upon the vital forces of the system, which we have now considered, are more or less

(1) Andral says, that, in "392 cases of cerebral hemorrhage which I have found described in authors, the seat of it was in some part of the substance of the brain in so many as 386; and that effusion into the ventricles is almost always the result of a rupture of their parietes, whereby a communication was established between them and the accidental cavity." (*a*)

It is stated by Lallemande, (*b*) Morgagni, (*c*) Moulin, (*d*) Craigie, (*e*) and others, that the *corpus striatum*, and the part adjacent to its outside, and the optic thalami, are the most common seat of cerebral hemorrhage; while Rostan, (*f*) and others, say that those parts are also most liable to ramollissement.

In respect to the chances of recovery from apoplexy, perhaps some indication may be formed from the effects of wounds of the brain. Sir B. Brodie remarks, "I have never been able to discover, among all the works which I have consulted, a single instance of recovery from a wound of the posterior lobes of the cerebrum, of the cerebellum, or medulla oblongata; and in a majority of cases, in which a cure has taken place, the injury has been confined to the frontal bone, and that part of the brain which is covered and defended by it." (*g*)

Dr. Cheyne observes, that when hemiplegia attends apoplexy, we may expect to find extravasation of blood at the base of the brain, or in the tube of the spine. (*h*) This opinion has been also more recently expressed by some others. It appears, however, to be greatly founded upon the probable physiological peculiarities of different parts of the brain, and upon the results of experiments.

But it seems far from being necessary to the foregoing result, that the *direct* effect of pressure, or of mechanical injury, should be felt at the base of the brain. Sudden extravasation of blood from the external pia mater, may so affect the already morbid powers of the superior part of the organ, as to involve its base in a deadly sympathy. De Haën had observed that a nerve may be so violently irritated as to produce a derangement of the entire functions of the brain. (*i*) Many instances are recorded by Hunter, (*k*) Sirs A. Cooper and Travers, (*l*) &c., in which slight surgical operations have produced instant death.

(*a*) Patholog. Anat., vol. 2. p. 452.
(*b*) Sur l'Encéph. p. 19.
(*c*) De Sedibus, &c. Ep. 3.
(*d*) Trait. de l'Apop. p. 54.
(*e*) General Anatomy, p. 409.
(*f*) Sur Ramoll. du Cerv., p. 459.
(*g*) Med. Chir. Trans. vol. 14. p. 421.
(*h*) Cases of Apoplexy, p. 23.
(*i*) Rat. Med. t. 3. p. 121.
(*k*) On the Blood, &c. p. 397, &c.
(*l*) Travers' Inquiry, &c., p. 32, et seq.

exemplified in apoplectic affections; and the range is vastly extended when we embrace in the consideration, epilepsy, convulsions, hysteria, mania, &c. Hence the importance of ascertaining, as nearly as may be, how extensively the powers of life are disturbed in each individual case, that we may not complete their extinction by precipitate treatment. "What strongly indicates," says Dr. Cheyne, "that the barriers are sometimes but slender, which are raised between disorders of the brain, generally distinguished by the most opposite symptoms, and that a slight modification in the vascular action may produce very opposite effects, is, that madness has been known to alternate with paralytic weakness." (1) And shall we, in such a case, be guided by the pathological state of the brain, with as little reference to the varying symptoms as is generally observed in the treatment of apoplexy? Do not "opposite symptoms," however it may be opposed to the morbid anatomy of the brain, demand some modification of remedies? Is it equally obvious, because "one moderate bleeding may avert an attack," that "it is a good rule to have every patient in apoplexy, who is not plainly dying, blooded."? (2)

The consequences, which are determined by the sudden lesion of the brain in apoplexy, will depend not only upon the natural constitution of the individual, often the precise nature and seat of the lesion, and the antecedent condition of the organ, but they will be variously modified by the preexisting state of other parts; whether the system was in a state of health at the time of the seizure, or whether important organs may not have been previously diseased, and thus incur a more profound lesion after the attack, and send back upon the brain the shock they have sustained, and again receive the reverberation; and whether, also,

(1) Cheyne's Cases of Apoplexy, p. 17.

(2) Cheyne's Cases of Apoplexy, pp. 63, 64. "If," says Dr. Cheyne, "the display of the brain, destroyed by apoplexy, does not prove how indispensable venesection is, every other argument in favour of it must be accounted weak." And, again, "no lesson to enforce despatch can be more impressive, than the display of the brain after apoplexy." "Two pounds of blood ought to be removed as soon as possible after the attack." (a)

But this philosophy is founded upon morbid anatomy alone. It pays no regard to that greatest source of instruction and truth, the phenomena of vital actions. In numerous cases of apoplexy, immediately after the attack, there is scarcely any analogy betwixt the existing forces of life and their antecedent state.

(a) Cases of Apoplexy, pp. 52, 56, 60, &c.

such disease may not have developed the cerebral derangement, and remain a powerful aggravating cause.

It is in apoplexy that we see the value of symptoms exemplified far beyond any light that may be derived from morbid anatomy; and indeed, that vital signs are the only true guide in practice. It is here seen how far reliance on morbid anatomy has divested practice of its philosophy, and shut out science from its paramount right.

The variety of lesion sustained by the vital forces in apoplexy is constantly denoted by the symptoms. The pulse of an athletic subject may become almost insensible at the moment of the attack; whilst that of the feeble may acquire a volume and force exceeding its natural state. The general circulation is roused at one time, and prostrated at another. The cerebral lesion has now the effect of an excitant upon the system, and again it is a deadly sedative. In one patient the pulse suddenly falls to forty strokes in a minute; whilst, in another it is as suddenly raised to more than a hundred. In one it beats with staid regularity; in another it intermits; in another it hobbles; and in a fourth it rises and falls in its volume, in coincidence with the prolonged acts of respiration. There is nothing uniform about it. We are told, indeed, by Dr. Abercrombie, to *encourage* bloodletting, — "that the state of the pulse is a very uncertain guide;" (1) and by Dr. Fothergill, to *discourage* the remedy, — that "the pulse is often an insufficient guide; (2) whilst Dr. Kirkland wishes it to be considered, "whether the agitation we sometimes observe in the pulse, does not show a diseased state of the nerves, which forbids the operation of bloodletting." (3)

"I never feel an irregular pulse," says Dr. Cheyne, "without the utmost dread. We may consider the patient as lost when the pulse has become quick and thready." (4) The latter is a fatal sign, if consequent on bloodletting. But we must consider another fact which our author had just stated, that "the circulation often undergoes changes, which seem directly and suddenly to arise from the oppression of the brain, before the system is universally disturbed." From this early influence of the brain on the circulatory system, the state of the pulse, along with other symptoms, must be taken rather as a guide for the treatment,

(1) Abercrombie on the Brain, &c. p. 304. (2) Works, p. 590.
(3) Commentary on Apoplexy, p. 48. (4) Cases of Apoplexy, p. 15.

than in forming any certain prognosis. When the influence is strongly exerted, it undoubtedly denotes a profound lesion of the vital forces. Still it may be, in its perilous degree, temporary only, if the prostration be not increased by precipitate bloodletting, or if the forces of life be sustained, perhaps, by the aid of stimulants. Should the circulation, however, become more impaired as the disease advances, the danger obviously increases. The shock which it sustained has passed away in a measure, and the state of the general circulation becomes more determined by the constitutional derangement.

It need not be said, how profoundly the stomach is affected, — how variously respiration, — how differently the voluntary muscles, — the sphincters, &c. suffer. And here we cannot neglect an observation by Dr. Philip, which is the more remarkable from his familiarity with the influences of the brain upon the organs of circulation. "In sanguineous apoplexy," he says, "the only change which takes place in the action of the heart is the effect of impeded respiration;" (1) and even to the very last, the impaired respiration is represented as the cause of death, — "the insensibility becoming such that no want of fresh air in the lungs can excite the patient to inflate them." (2)

This, apparently, grows out of Dr. Philip's ingenious theory of the physiological cause of respiration; but, how contradictory is it of those luminous experiments, by which he ascertained the controlling power that may be determined by the brain upon the circulatory organs, and the stomach. If, by slight irritations of the brain we may interrupt the regular functions of those organs in a perfectly healthy state of the system, how much more probable is it that they will be profoundly injured in their vital forces, when the brain is lacerated, and compressed by extravasated blood, and the great viscera, in a majority of cases, antecedently in a state of disease? The cerebral influence, therefore, is direct; whilst it is probable, that, so far as indirect sympathies are concerned, the circulatory organs are more affected by the embarrassed state of the stomach, than by that of the lungs; and much more so by this cause, than by any defect of arterialization of the blood. (3) The foregoing quotation, also, conflicts with the statement in our note below.

(1) On the Means of preserving Health, pp. 95, 103. Also, Inquiry into Vital Laws, &c. (2) Ibid. p. 96.

(3) Dr. Philip, and others, suppose that the "nervous apoplexy" is the only variety

Considering, therefore, the varied influences of the brain upon the forces of life in apoplectic affections, and the manner in which we have seen that bloodletting affects this organ, and the consequent impressions which are propagated from it over the whole system, it must be obvious where the general lesion is very profound, that the abstraction of blood at the onset of the attack may so increase the pernicious influence of the brain upon the sinking powers of the system, that neither nature nor art can repair the injury. This will be especially true of such cases if we bleed to syncope. But the abstraction of blood is powerfully felt in a direct manner by the vital properties of every organ; and where these powers are excessively depressed by the nervous influence, and that influence constantly maintained by the peculiar condition of the brain, it will happen in the foregoing cases, that there will be no ultimate recoil from the depressing effect inflicted by the loss of blood. Here will be also another shock added to the direct injury from loss of blood, since the violence thus inflicted upon the system at large will be extended, by sympathy, to the brain; whilst this organ will reflect every pernicious impression it receives from others.

It should be also considered that effusion probably exists within the brain, and that bloodletting cannot reach this part of the exciting cause; that its operation, although diminished, must

which can prove suddenly fatal; "because," in the opinion of the former, "pressure of the brain never instantly destroys the powers of circulation or of respiration." (a) But it must be considered that his experiments were made on animals in health, of a different constitution from man, &c.; and he determined even in such instances, that when the causes operate powerfully and instantaneously, they were not only capable of destroying instantly those functions, but of extinguishing, at once, all muscular power. Now it cannot be denied, that extravasations of blood must operate in this sudden manner; not only disorganizing the brain, but exerting a most sudden and pernicious interruption of its organic functions. But in death from nervous apoplexy, which he ascribes to "a sudden derangement of the finer mechanism of the brain," in the absence of all morbid appearances, what evidence have we of the sudden operation of any cause, other than the phenomena themselves? In one instance we have a manifest cause, capable of instantaneous operation, and compressing every fibre of the organ; in the other, the most we can say with certainty is, that a predisposition to great and sudden alteration of the cerebral powers has been established by the slow operation of causes, and that the paroxysm is perhaps, determined by a sudden impulse of the circulation; a cause which coexists with extravasation in many cases of sanguineous apoplexy.

That, in these cases of nervous apoplexy there may be often some partial disorganization of slow accession, (like softening, for instance,) we have little doubt, from all we have been able to gather upon the subject.

(a) Ut Supra, p. 95.

continue for an indefinite time, and that if we lessen too much the energies of the system, they will at last fail from its increasing influence. Whilst, therefore, we strive to avert one evil, there should be an equal care not to increase another.

The importance of bloodletting will depend greatly, also, upon the nature of the fluid effused; of which we may, perhaps, form some conjecture from the antecedent history of the case. In serous apoplexy, the cerebral congestion or inflammation has been generally in a low state, and is probably much subdued by the effusion. (1) It may be, therefore, chiefly the immediate object of bloodletting to diminish the impulse of the circulation upon the brain, and perhaps to lessen a state of congestion in the abdominal viscera that may continue to operate upon the cerebral powers. (2) Serous apoplexy, however, is not common. Dr. Cheyne considers the ratio of the sanguineous to the serous, as 98 to 100.

In the sanguineous apoplexy we have a different state of things, and other objects must be contemplated. We have, then, not only to lessen the impulse of the blood, and to strike

(1) We have had occasion to make some comments upon this subject in another place, for the purpose of showing, that in consequence of the depleting effect of the effusion, vascularity disappears, and the disease, which had been inflammation, is set down by the anatomists as non-inflammatory. A striking proof of this effect of serous effusion is afforded by Dr. Conquest's success in tapping the head for hydrocephalus. Of nineteen cases, he cured ten by this method. (*a*) Graefe, (*b*) Hutchinson, (*c*) Earle, (*d*) and some others, have also succeeded by the same process.

Here it is evident, from the success of the operation, that the morbid action had been subdued; and since its removal was clearly effected by the effusion, and from the well known dependance of all sudden formations of serum in the brain, as well as in other parts, upon inflammation or some analogous condition, we may safely conclude that a similar pathological state had existed in the foregoing cases.

We have adverted thus particularly to the foregoing subject on account of its practical importance, and from seeing it lately stated in a valuable work, that "we agree with Dr. Cheyne, that hydrocephalus consists in many instances of a diseased action of a peculiar kind; but of what, we can as little explain as we can the nature of scrofulous action." (*e*)

These are two examples where morbid anatomy has arrayed itself against the vital phenomena, &c. "Morbid anatomy," say E. and M. "has not yet enabled us perfectly to clear up the subject." Will it not be best, then, to leave it to the symptoms, to the effusion, and to analogy?

(2) The division of apoplexies into sanguineous and serous was universally adopted at an early age, and until a recent period, the two varieties were thought to require, at least, some modifications of treatment.

(*a*) London Med. Gaz. March, 1838. (*b*) Graefe and Walther's Jour. 1831.
(*c*) Med. Chir. Trans. vol. 2. p. 105. (*d*) Med. Chir. Trans. vol. 7. p. 427.
(*e*) Evanson and Maunsell on the Management & Diseases of Children, ch. 12. s. 2.

at any remote congestions; but we must speedily reduce the congested state of the cerebral veins, and thus arrest the progress of the hemorrhage, and re-establish the natural circulation and healthy functions of the brain.

But the moment when bloodletting may be applied with advantage, and the extent of the remedy, must be directed as much by the existing state of the general symptoms, as by any pathological condition that may have lead to the paroxysm Since, however, there is no lack of bloodletting, we shall consider, principally, some of the circumstances which appear to contra-indicate it, or to warrant its delay.

We may safely say, that when the respiration and circulation are profoundly and conjointly affected, with a languid pulsation of the carotids, bloodletting must infallibly kill, if adopted before nature has effected some mitigation of those symptoms which evince a near extinction of the vital powers.

But we sometimes meet with a small and contracted state of the pulse, a cold surface of the body, especially of the extremities, with a bounding action of the carotids, and augmented heat of the head. There may also coexist a profound lesion of the respiratory function, and other symptoms that show an alarming prostration of the vital forces. Here, however, is something favourable in the active state of the cerebral circulation, something that assures us that the hand of art may rekindle the expiring flame. We know that bloodletting will be now borne to a certain extent. (See pp. 165, 187, 189, 201.) If the general circulation be not prostrated, perhaps it may be universally affirmed of these cases, that one may bleed at the onset till the pulsation of the carotids is sensibly reduced, even should three or more pounds of blood be necessary to this purpose. It commonly happens, also, in cases where the cerebral circulation remains thus excited after the attack, that the organic lesions are less profound, than in those instances in which the vascular action of the brain is also prostrated by the paroxysm.

Again it happens, that the pulse is small, perhaps almost obscure, when the breathing is less profoundly affected, and even in the absence of stertor. These cases are often complicated with hepatic congestion, which had existed as a predisposing cause of the attack, and upon which the modified action of the heart is more or less, perhaps mainly, dependent. The

immediate abstraction of blood, under such circumstances, moderate at first, will seldom fail to increase the strength and volume of the pulse; and unless the functional derangement of the liver, which, in this case, has been a principal remote cause of the paroxysm, be early subdued by bloodletting, &c., there can be no chance for the patient. It more frequently happens, however, when hepatic congestion has been the predisposing cause, that the heart, which had before pulsated feebly, is thrown by the attack into more forcible and less frequent action. This is a worse symptom than the former, if the respiration be deeply embarrassed; but if respiration be not severely affected, the change in the heart's action is favourable, and admits of prompt and extensive bloodletting. If, however, the pulse be small in these complex cases, or even full, but without strength, and accompanied by a very embarrassed state of respiration, immediate bloodletting can hardly fail of sinking the vital forces. These last cases are commonly attended by a coldness of the surface, and often demand the moderate use of stimulants, till the vital energies are so far restored, that bloodletting, on which we must ultimately depend, may be cautiously adopted.

Again, in a majority of cases, the respiration is profoundly affected, and the pulse is full, with strength; but there is no unusual pulsation of the carotids. Here the state of the respiration and some other symptoms are a more important element than the pulse, either as it relates to the carotids, or to the state of the general circulation. The action of the heart may be greatly disturbed by slighter affections of the cerebrum; it may still be influenced by pre-existing disease of different organs, and the disordered respiration and the blow inflicted on the whole system, will become concurring causes of its modified state. Not so, however, with respiration. If it be very prolonged and heavy, it denotes a profound influence of the brain upon the organic functions; and since, in these cases, the cerebral powers and actions are suddenly and greatly depressed, it is a subject for serious consideration how far bloodletting can be carried at the onset of the attack, without increasing the pernicious influence of the brain upon the *vires vitæ*, or of depressing them by the direct effect of the remedy. In proportion to the severity of dyspnœa, in these cases, should be the caution, at first, with which blood is abstracted. Other symptoms must,

of course, have their important influence. If the sphincters be also relaxed, immediate bloodletting will be out of the question. If the pupils be greatly contracted, as is sometimes the case, or are insensible to the light, these will be other causes for delay; and along with the foregoing phenomena, the temperature of the skin, the state of deglutition, and the degrees of remaining sensibility, should be duly considered, as contributing to a knowledge of the existing state of the *vires vitæ*. The best writers agree that the pulse is an uncertain guide; and we believe it to be so in many cases, not only from observation, but for other reasons which we have stated. This is particularly true in miasmatic countries, where congestions of the liver, and congestive fevers, are apt to prevail.

But, in regard to the opinions of the highly able authorities to whom we have alluded, on the subject of the pulse, it should not be forgotten, that some have set aside the indications supplied by the pulse, for the purpose of removing what might be considered an important obstacle to indiscriminate bloodletting. On the contrary, however, we think it must be obvious from what we have said of the physiological influences of the brain over the organs of circulation, that the state of the pulse should at all times form an element, (though with great circumspection,) in our deliberation upon the existing condition of the forces of life. If the pulse be greatly depressed and considerably accelerated, it is, *primâ facie*, an indication for the delay of bloodletting. But we have seen, that in some instances this symptom may be contradicted by other considerations which may justify immediate bloodletting, moderately at first, but copiously afterwards. Again, the pulse may be fallacious when it is full and slow,—there being present other symptoms which denote an excessive prostration of the *vires vitæ*, and which call for delay. At other times there is not a correspondence betwixt the action of the heart and the strength and fulness of the pulse. If the heart be examined, it may be found languid in its efforts, whilst the pulse is full and strong. In these cases, a small abstraction of blood, at the invasion of the paroxysm, may immediately prostrate the circulation, and fatal syncope may be the consequence. Here the nervous influence appears to be felt differently by the heart and blood-vessels. But whatever mysterious agency may sometimes impel the blood with augmented force through its natural courses, we cannot escape the fact

that there is not always a corresponding energy in the action of the heart. In these instances, the blood should, at least, be abstracted slowly, and the changes in the circulation accurately observed. It is not the ordinary results of syncope which we fear. It is that of superadding to the existing influence of the brain upon the vital forces that farther determination of its depressing effect which attends the loss of blood when syncope approaches. Their united operation may completely stifle the languid efforts of nature.

At other times, the foregoing state of the pulse is connected with throbbings of the heart, which impart the impression that its action is convulsive.(1) This is also a very alarming modification, and may call for a delay of bloodletting. Much, however, will now depend upon the actual force of the circulation. In proportion as it offers resistance to the touch, immediate bloodletting will be more or less safe and important, if there be not contra-indications in other symptoms. Fothergill, who has been so liberally censured, and withal misrepresented, for his views on the subject of bloodletting in apoplexy, thus notices the foregoing phenomenon: —

"If bleeding is performed," he says, "when it ought not, either death ensues, or an incurable hemiplegia. If a person fall down in a fit of any kind, the surgeon is immediately sent for. He, perhaps, upon feeling the patient's pulse, finds it full and tense, and general practice not only authorizes, but custom, become a law, generally directs the operation. The pulse, in such a situation, is often an insufficient guide. It may be that struggle which arises from an action of the *vires vitæ*, to restore health. I believe it happens in most cases, where there has been a temporary, or even momentary cessation of the animal powers; and it is, perhaps, in this situation that bleeding is performed and often very liberally." (2) Falconer, (3) and other authors, have also ascribed this condition of the pulse, at the invasion of apoplexy, to a recuperative effort of nature. But we cannot but think that, at the invasion of the paroxysm, it is one of the direct symptoms of the disease, and rather denotes a failure than a revival of nature.

(1) Bichat speaks of a convulsive effort of the heart when the vital powers sustain a great and sudden violence. (*a*)

(2) Works, p. 590. (3) Mem. Lond. Med. Soc. vol. 2. p. 213.

(*a*) Phys. Research. &c.

What shall be said of those cases in which the pulse is small, feeble, and intermittent; the breathing perhaps irregular, and the "sanguis abit, mentemque calor corpusque relinquit?" Do they not constitute exceptions, in which bloodletting may be delayed, till the almost extinguished powers of life may be partially invigorated by other means; or must we still adhere to the rule of "prosecuting the treatment of every case of apoplexy with the utmost vigour and perseverance?" (1) We shall present some cases of instantaneous death from apoplexy, in an appendix to this paper, which forcibly demonstrate the necessity of considering, as far as may be, in what various degrees the nervous influence has been exerted upon the forces of life, at the invasion of the paroxysm. (See APPENDIX I.)

We shall not pursue this analysis farther, it having been our principal object to exhibit the peculiar effect of bloodletting upon the brain in determining the influence of that organ upon the powers of life. In most of the cases to which we have referred, as presenting objections to the loss of blood at the onset of the paroxysm, its well regulated use, often to a large extent, will sooner or later become important.

It behooves the physician to meet every case of apoplexy with entire self-possession, and to reflect that no subject can require the exercise of greater skill, and, perhaps, of firmness. The authority of custom, sanctioned by the most acute and renowned observers, will be likely to embarrass our judgment, paralyze our independence, and hold us spell-bound, when all may be depending on the unbiassed dictates of the understanding. The difference of an hour in the application of bloodletting may be for the weal or the wo of the patient. Shall we deliberate? Professional reputation may be in peril; but the greater will be the reward to a sensitive and enlightened mind. Where art can be of any advantage, there will be always time for calm investigation of doubtful cases. Such are the recuperative powers of nature, they will generally struggle for a time with success,—at least, in cases where art can be instrumental. "It is probable," says Dr. Heberden, "that far the greatest part of paralytic and apoplectic patients would recover some degree of life

(1) Abercrombie on the Brain, p. 303.

It is said by an able reviewer, "that in the very great majority of cases, if an error be committed in the treatment of apoplexy, it is not on the side of using the lancet too freely, but too sparingly." (Dublin Journ. of Med. Science, vol. 1. p. 207.)

and strength by the unassisted efforts of nature." (1) This observation, in which Fothergill, (2) and Portal (3) agree, is evidently based upon long and careful observation. It is this partial recovery of the vital forces for which we should wait in certain cases, before resorting to the abstraction of blood. If nature be too much struck down by the blow for an independent effort, we shall hardly contribute any useful succour by inflicting another. If, also, the powers of life be greatly prostrated, action is, of course, in a languid state. Whatever disease may exist in the brain is, for the present, controlled by the same principle. Hemorrhage is suspended; and the functions, everywhere, whether natural or morbid, are nearly at a stand. It is here, in the severest cases, as it is in concussions of the brain; when, it is said by Mr. Abernethy, "it would appear in the first stage that very little can be done." (4) This has now become the doctrine of surgeons. Sir B. Brodie observers, that "I suppose none of those, who have suggested the exhibition of stimulants, would actually be inclined to apply this practice to cases in which the pulse has regained its strength and regularity; and, on the other hand, I conclude that no one, among those who have advised the use of the lancet, would think of taking away blood when the patient lies with pale cheeks, and cold extremities, and a feeble and intermitting pulse, or would refuse to resort to the cautious exhibition of cordials and stimulants where these symptoms are so urgent that he is manifestly in danger of sinking, in consequence of the depressed state of the circulation which has followed the first shock of the injury." (5)

(1) Commentaries, p. 284. (2) Works, p. 290. (3) Sur l'Apoplexie.

M. Portal, who places little or no restraint upon the lancet, admits that "he has seen the most formidable cases, which had been attended by the greatest intensity of stupor and of stertorous respiration, by entire paralysis of the limbs, or hemiplegia, or paraplegia, cured by the unaided efforts of nature."

The advice of Hippocrates, that we should leave the worst forms of the disease mainly to nature, like most of his opinions, appears to have been founded on critical observation; nor has all subsequent experience proved it to have been otherwise.

It is the experience of Falconer, that in "paralytic attacks that are not so violent as immediately to threaten life, we almost constantly find a degree of recovery after the stroke, which generally lasts for three or four days, and seems a sort of natural effort to resist, or throw off the complaint; after which, the condition of the patient is generally stationary till a fresh attack." (a)

(4) Injuries of the Head, case 20.

(5) Med. Chirurg. Trans. vol. 14. p. 377.

(a) Op. Citat.

Hennen remarks, that "no well informed man, now-a-days, flies to the lancet the moment he hears of an injury of the head. He examines the case, and, from appearances, decides on the immediate necessity of abstracting that blood, which he well knows he must have recourse to when reaction takes place. (1) If the concussion is so violent that the powers of life are absolutely sinking, to bleed instantly would be to destroy the patient. A glass of wine or of spirits poured down his throat will be the proper remedy." (2) The writer adds that it had fallen to his lot, on several occasions, to see the foregoing plans of treatment contrasted on a large scale, and the balance was greatly in favour of the method which he recommends.

When bloodletting is of doubtful expediency in apoplexy, and this is commonly only soon after the seizure, in cases that admit of relief, the abstraction of blood should advance slowly, and its influence be carefully observed. The result from a small quantity, may be such a relief to the brain, that its pernicious influence may be so withdrawn from the system that the remedy may be soon repeated, and to a greater extent.

Having brought the system, in bad forms of the disease, out of its alarming prostration, either by moderate stimulation or cautious bloodletting, or by entrusting it to its own resources, &c., it will become important to estimate the probable extent of disease in the brain and other organs. And here we cannot but express our strong conviction that sanguineous effusions within the cranium are generally the result of disease, and that they very rarely depend upon any primary rupture of blood-vessels. Dissections prove that this condition of disease in almost all cases is constituted by venous congestion. This view of the pathology, whilst it is entirely more inauspicious to the hopes of the patient, than that which regards the effusion as the simple result of a ruptured vessel, requires more energetic means than the latter. Indeed, were simple rupture the source of the effusion, we see not in what respect art is likely to be instrumental. It cannot be, as is commonly supposed, by diminishing the force of the circulation that we obtain much ascendancy over the complaint. Indeed, in many cases where the pulse is pros-

(1) From Hippocrates, downwards, this recovery of the circulation in apoplexy has been considered most favourable. It denotes vigour in the forces of life, and admits an extensive application of the great remedial agent.

(2) Military Surgery, p. 317.

trated, relief is effected whilst the energy of the heart rises under the influence of the lancet. The philosophy of the effects of this remedy mainly relate to its impression upon the vital forces of the capillary vessels.

Bloodletting, too, is never known to arrest those hemorrhages which arise from the division of vessels; whilst, on the other hand, as effusion advances, the laceration of the capillaries that are engaged in the morbid process of sanguineous excretion explains the reason why cerebral hemorrhages are generally small; the excretory action being thus extinguished. There are many facts which appear to demonstrate the truth of our conclusion; some of which we shall append to this section of our work. (See APPENDIX II.) In a practical sense a decision of this question is of vast importance. At present, opinions preponderate in favour of the doctrine of rupture.

We may conclude, then, with all the advantages of the most enlightened pathology, and the most appropriate treatment, the apoplectic must, generally, exist for a long time in a perilous condition. In the early stages, a formidable state of morbid action is to be overcome by energetic measures, whose timely application is more surrounded by difficulties than in any other disease. The brain, too, in the cases supposed, has sustained a fearful laceration, and a clot of blood is probably compressing and irritating the whole organ,—there to remain, quivering like the arrow of death, till it is slowly removed by a system of vessels, which, it is supposed, because unseen, have no existence.

It has not been our object to speak in relation to cases of apoplexy that obviously admit of immediate bloodletting. These are common, and may demand an extensive application of the remedy. But the only rule that can be assigned in regard to the quantity of blood that should be abstracted will probably be found in the foregoing considerations.

In estimating the effects of cerebral disease on the system, we must duly consider the various relations of the brain to other parts. Considered simply as an organ, it is liable to the same modes of disease as other organs, and to the same relative sympathies as exist among other parts. But this is a small part of the important relations of the brain. It is especially destined to preside over the great functions of the body, however they may be the result of powers that exist and act in independence of the brain; and whenever its organic functions become de-

ranged, these specific relations to the system are affected in consequence. To these influences must be also added another coexisting modification of the peculiar functions of the organ which subserve the faculties of the mind, and which connect the immaterial part with the whole system. This complex derangement of the cerebral functions will produce the most varied results; and as one or the other may be differently modified, or as they may be reciprocally affected, and according to the susceptibilities of other organs, and their reaction upon the brain, will be the endless variety of the phenomena.

From the considerations which have been offered in the foregoing sections, we arrive at the following general results.

1. That bloodletting produces its direct and efficient impression upon the *vires vitæ* of the capillary blood-vessels, by modifying their action.

2. The quantity of blood to be removed relates directly to that impression.

3. Its most salutary effect will, therefore, consist in its nearest approximation to a full, but just, impression upon the *vires vitæ*.

4. To produce and maintain this impression, will require the abstraction of a certain quantity of blood in every case, the measure of which will be the antecedent and resulting symptoms. (1)

5. Bloodletting may add to the violence of disease by coming short of that impression; or, it may equally injure, if carried to excess, and may even induce new inflammation. (2)

6. Its local, and sometimes its general, application is remedial when inflammation is induced by excessive bloodletting alone.

7. Bloodletting may be a remedy for other diseases than inflammation.

8. It is equally safe at all periods of life, is most indispensable in old age, though not less important in many diseases of infancy.

(1) "Definere vacuationis modum scripto non licet." (*a*) Or, as Hippocrates has it, "quæ prodeunt, non multitudine æstimare oportet, sed quamdiu prodeant qualia oportet. Et ubi opus est, usque ad animi deliquium ducere oportet, et hoc facere, si sufficiat æger." (*b*)

(2) "Summa prudentia ac judicia opus est; sive enim nimium, sive parcius, justo subtrahatur *nocet utrumque*." (*c*)

(*a*) Galen de Cur. Rat. per sang. miss. c. 14. (*b*) Aph. s. 1. app. 23.
(*c*) F. Hoffman de Venæ sec. Abusu, t. 5, p. 346.

9. If employed as a prophylactic, on passing from northern into tropical countries, it must be with such moderation as shall not increase irritability; and then only in the plethoric or robust. (1)

10. General bloodletting, cupping, and leeching, operate upon common principles, which are more or less modified in each mode of abstracting blood. Cupping is intermediate in this respect betwixt general bloodletting and leeching.

11. General bloodletting is ~~wholly~~ a more important remedy than leeching; and whilst cases constantly arise in which the latter cannot be substituted for the former, there are numerous instances in which general bloodletting cannot take the place of leeching. Cupping will sometimes answer the purposes of either, and may be better.

12. The brain has a peculiar allotment in the effects of bloodletting; and inflammation of this organ will generally sustain a greater loss of blood than any other. There are peculiar conditions of this organ, however, as in some cases of mania, delirium-a-potu, and especially apoplexy, in which, on account of the relation of the nervous influence to the organic forces, and the manner in which that influence is determined by bloodletting, the abstraction of blood may be either inadmissible, or must be practised with great circumspection.

13. Spontaneous hemorrhages, occurring at adult age, should not be restrained, unless manifestly proceeding to excess. (2)

(1) In our essay on the humoral pathology, (Sects. 13 and 14,) we have stated some remarkable examples where bloodletting and abstinence from stimulating food have manifestly protected the system against disease. But it is obvious that no strong artificial impressions can be made upon the *vires vitæ*, in a state of health, without increasing their susceptibility to morbific agents. There are some mysteries, however, relating to this subject, which we do not understand. The vaccine disease protects the body against smallpox, and habit is powerful in its various influences. The most, however, that we know of prophylactics is exceedingly limited. They must come, at least, within the foregoing limitation. "Whatever is contrary to custom is hurtful, whether it be hard or no." (*a*) "Neither satiety, nor hunger, nor any other thing, which exceeds the natural bounds, can be good or healthful." (*b*) But, "as husbandry promises food to sound bodies, so does physic health to the sick;" (*c*) and remedial agents that are appropriate to any known pathological state are generally useful, in a corresponding ratio, during its incubation. See sects. 4 and 14 of Humoralism.

(2) "Neque si ex pectore, aut pulmonibus, aut stomacho, aut jecore fertur, periculosum est; e capite vero si fluxit, minorem noxam affert." (*d*)

(*a*) Celsus, l. 1, c. 3, p. 27. (*b*) Hip. Aph. s. 2, Aph. 4.
(*c*) Celsus Præf. l. 1, p. 1. (*d*) Aretæus de Curat. Morb. Acut. l. 2. c. 2.

APPENDIX I—TO ESSAY ON BLOODLETTING.

See p. 357.

CASES OF SUDDEN DEATH FROM APOPLEXY.

Case 1.— *Charles Baldwin, Esq.*, aged 63 years, a member of the New-York bar, died of apoplexy on the 13th of June, 1834. Frame robust; short thick neck; habits temperate; rather sedentary.

While sitting at the dinner table of the City Hotel, and in the act of speaking, he fell from his chair, and in the expressive language of one of the papers of the day, "died in the twinkling of an eye." The concurring testimony of a great number who were present represented him as having lost every appearance of life when he reached the floor. Dr. Pratt informed the writer that he saw him in less than three minutes after the attack, and that he was then perfectly dead. He immediately opened a vein, but no blood escaped from the orifice. The countenance assumed a livid appearance at the moment of the seizure. Dr. Pratt found it highly injected with blood, and the surface of the body dry.

The body was examined 24 hours after death, by Drs. J. Kearney Rodgers, Cornell, Baldwin, and myself, the former of whom saw and approved the following statement immediately after the dissection.

The whole upper surface of the thorax was of a deep livid colour, as was also the posterior part of the head, which was entirely bald. The abdomen was distended, and a sanious fluid was oozing from the nostrils. About an ounce of blood had escaped from the orifice in the vein.

Under the skin of the thorax and abdomen was a layer of fat, from three-fourths of an inch to an inch in thickness. A large vein was divided near the upper part of the sternum, from which the blood flowed with such force, it could only be arrested by a ligature.

The lungs were a fine specimen of a perfectly healthy state of those organs. They were *fully inflated and perfectly filled both cavities.* The pericardium contained about two ounces of serum. There was no vascularity on its inner surface. The heart was examined by Dr. Rogers with critical accuracy. The right cavities were empty, and the left contained but a moderate proportion of blood. The parietes of the right ventricle were rather softer than those of its fellow. It manifested no mark of disease, and had but a moderate proportion of fat. The valves were all natural. There was no fluid in the thorax.

In the abdomen, all the viscera were natural. The liver exceeded its common size, and the gall bladder contained about half an ounce of yellow bile. The omentum and other duplicatures of the peritoneum were loaded with fat. The stomach and intestines were distended with gas. There was no fluid in this cavity.

The scalp was unusually thick, and possessed a firmness which gave to it

the elasticity of whalebone. Its division was followed by a copious discharge of blood, although the head was elevated on a block. The scull was uncommonly thin, and yielded more readily than is usual to the saw. The dura mater was natural, but very firmly adherent to the bones. On dividing this membrane, about an ounce of colourless serum escaped. A proportion remained beneath the arachnoid, by which it was separated from the pia mater, over the whole extent of the cerebrum, and presented the common appearance of a gelatinous substance. The veins of the latter membrane were more than usually filled with blood, and the minute vessels were so generally injected as to give a light florid appearance to the whole surface. Near the external part of the pia mater of the left hemisphere was an infiltration of blood into the spongy texture of this membrane, extending longitudinally about two inches and a half, and one inch in width; a similar ecchymosis, about an inch square, in the corresponding part of the right side. The colour was intermediate betwixt venous and arterial blood; and it is remarkable that this was the only apparent coagulated blood in the body. The larger veins running from these extravasations to the longitudinal sinus were *thrice the size* of any others, — affording a very striking contrast. The substance of the organ possessed its natural firmness, and neither the convolutions nor the medullary part were more than usually vascular. The ventricles were empty. The plexus choroides was fuller than usual. Every other part was perfectly natural.

The cerebellum was next removed; when more than an ounce and a half of serum was found at its base, and extending into the theca of the spinal marrow. This organ, and the crura cerebri, were in a remarkable state of *mollescence*, which equally involved the medulla oblongata. On laying the cerebellum down, it became much flattened by its own weight, and it felt in the hand like a mass of soft jelly. The knife rather tore than cut the medulla oblongata. The medullary portion had a light slaty colour, which, however, did not affect the medulla oblongata. Neither its membranes nor the organ were more than naturally vascular.

Mr. Jennings, the keeper of the hotel, who was near him when he fell, stated that he had just finished his bowl of soup, and had placed on his plate a part of the chicken; when a gentleman by his side remarked that he regretted that the shotted guns accidentally fired by the frigate Constellation during the late salute at Toulon had not done greater execution. Mr. B. immediately raised his voice in reproof; but before he had uttered a sentence, the attack took place.

In January, he had complained to Mr. J. of a cold of two or three days continuance; but did not consult a physician. Had not heard him complain since, till the morning of the day of his decease; when he informed Mr. J. that he felt some uneasiness in the region of the stomach, — laying his hand in that situation to indicate the place. He then expressed a desire that some simple chicken soup should be provided for his dinner. Mr. J. thought his countenance particularly expressive of disease at that time. He had been frequently out and in, in the course of the day, and when in the house was conversing with gentlemen in his usual manner.

Mr. B.'s student saw him on the morning of the day before his death, but not afterwards; when he thought he appeared ill. When asked what induced him

to think so, he replied, "it was only a very flushed appearance of his face. Had not heard him complain."

A waiter at the hotel stated, that he complained of a numbness in his right arm, on the morning of the day on which he died, and that he rubbed it for him for the space of five minutes.

The brother of Mr. B. informed me that their father died at the dinner table in the same sudden manner. (1) .

Case II. — *Elisha Williams, Esq.* a lawyer of great eminence in this state, aged 60 years, died within a minute in this city, June 29th, 1833.

The body was examined by Drs. Stevens, Post, Vanderburgh, and myself. Dr. Post performed the dissection, and approved the following statement soon after the examination.

On dividing the integuments of the head, which had been elevated to a level with the body, a very copious discharge of blood took place, which was preternaturally fluid; as it was, also, throughout the body.

On passing the diploe, the blood exuded rather profusely from that portion of the scull. The divided part was at last separated, with considerable violence, by means of a large lever. At the moment of its separation, there suddenly gushed from the inside of the cranium not less than *ten or twelve ounces of a bloody serum.* The bones of the cranium were remarkably thin; and the head being very large, the size of the brain was necessarily beyond the common proportion. The pia mater exhibited a gelatinous appearance, arising from serum beneath it. About an ounce and a half of the same fluid was found in the lateral ventricles, and about half an ounce issued from the theca vertebralis. The basilar artery presented a mottled appearance. The brain otherwise natural.

There was a large quantity of fat beneath the skin, with which the omentum, mesentery, and bowels were also loaded. The viscera of the abdomen were natural. There was about a quart of colourless fluid in this cavity which was very capacious.

The thorax, which was also very spacious, contained nearly three pints of serum in the right, and about two pints in the left cavity. The pleura, throughout, was natural, and there were no adhesions. The lungs were free from all appearance of congestion, or other marks of disease. The pericardium was covered with fat, and contained about an ounce of serum. The heart was particularly large, with much fat at its base. There was some hypertrophy of the left ventricle. Its substance was softer than usual. The left cavities were entirely empty, and the right ventricle contained about half an ounce of soft coagulated blood. There were several small portions of bone in the valves of the aorta. The mitral valves were also thickened and ossified in several points.

This was a complex case, though the immediate cause of death was serous apoplexy. The subject was a large and corpulent man, had a short thick neck,

(1) A case analogous to the foregoing, as it respects the condition of the cerebellum and preservation of intellect, is related by Dr. James Johnson, in the Med. Chir. Rev. Oct. 1838, p. 638.]

of laborious industry in his profession and other multifarious engagements; lived freely in respect to food, but abstemious in the use of liquors, and wholly so for the last three years. His health had been uninterruptedly good till within the period just mentioned; and it was now about two years since I prescribed for symptoms of severe abdominal congestion, and from which he soon recovered perfectly.

He had resided during the last few months of his life in a miasmatic district of country. On the 26th of June, three days before his death, he reached this city after a long journey from his place of residence. About ten weeks previously, his appetite began gradually to decline, and his food ultimately to produce nausea and vomiting. Three weeks before his death, a difficulty of breathing supervened, which soon rendered it inconvenient for him to remain long at a time in a recumbent posture, especially on the left side. Coincident with the difficulty of breathing was a beginning œdema of the legs; when he consulted a physician, who bled him moderately, but without any sensible relief. The urine was scanty and high coloured. He suffered no pain or other uneasiness in the head, but complained of a "torpor of his mind, and great depression of spirits."

When I first saw him on the 26th, he referred much of his uneasiness to the region of the stomach, where there was great tenderness on pressure. He could lie on his right side, with an additional pillow, for an hour or two, when he would suddenly start from orthopnœa, and await the subsidence of the paroxysm in an erect posture. Conversation produced great exhaustion. The countenance was expressive of anxiety and distress. Mild fluids were oppressive and nauseating, even in small quantities. The tongue was covered with a white, though not a dense coating; rather livid and moist. There was great thirst. A slight fluctuation was felt on percussing the abdomen. The lower extremities were greatly distended with fluid as high as the abdomen, but the œdema terminated there. The pulse contracted, hard, about 90, rather irregular, and obscurely intermittent.

I bled him in an erect posture to the extent of about ℥xxv. The blood was dark, and flowed slowly from a large orifice. The respiration became much relieved before the bleeding was arrested. A slight nausea ensued, but no faintness. The pulse soon rose in volume, — a circumstance which attracted the attention of the patient; and it became perfectly regular. The blood was very thickly buffed and cupped. A large blister to the thoracic and abdominal region; pil. hydrarg. grs. xx. with instructions to give him an ounce of ol. ricini in the morning, if the pills did not operate.

June 27th. This morning there was, in all respects, a remarkable improvement. "He had slept better than during his sickness." Dyspnœa had nearly ceased; the irritability of the stomach had disappeared; urine greatly augmented; the limbs already much reduced in size; thirst extinguished. Still the breathing was embarrassed by conversation; which, with the late severity of the case, induced me to abstract about another pound of blood, the good effect of which was immediately manifest in his ability to converse without any embarrassment of respiration.

The blood was yet dark, and in a wineglass still exhibited considerable buff, but was not cupped. The pulse stood at 75 after the operation, free, soft, and

regular. The urine, in the course of this day, became more and more abundant, and although the patient was sitting up a greater part of the day, the limbs were not swollen above the knee, on the approach of evening. He preferred his chair, that "he might better contemplate his improvement." His mind was no longer depressed,—and every hour he became more and more buoyant with hope. The pill and the oil of the preceding night had produced copious dejections of a dark fluid.

June 28th. He arose at about 8 o'clock in the morning, having "enjoyed an excellent night." No difficulty, whatever, in respiration, tongue becoming clean, urine copious, countenance animated, and pulse natural. Swelling of the legs nearly gone. Mind cheerful and contented, and the patient "considered his case no longer doubtful."

He remained in this state of convalescence till about 2 o'clock, P. M., when he was suddenly invaded by a cold chill, and retreated to his bed and was covered with blankets. The chill and shivering lasted an hour, when a high fever set in. At 5 o'clock, I found him in this part of the paroxysm, sitting up, countenance much flushed, skin hot and dry, pulse much excited, the whole frame being jarred by its action. There was weariness and aching of the limbs; but there was no embarrassment of respiration. The patient was cheerful, and "doubted not that things would soon be right again."

Here was evidently an invasion of idiopathic fever. I bled him to the extent of sixteen ounces. The blood poured out freely, and was almost of an arterial hue. The febrile action was assuaged, and the relief was so considerable that the patient felt more inclined to sit up than to lie down. The blood, drawn into a wineglass, presented no buff. Ordered pil. hydrarg. grs. vi. aloes grs. vi.

June 29th, 6½ o'clock A.M. Slept well till midnight, but restless afterwards. There had been no difficulty in respiration. The pulse was beating vigorously, the countenance flushed, and a sensible jar was communicated to the whole upper region of the body and head. The urine had become still more diminished and presented a reddish hue. Tongue again coated, and now, for the first time, he complained of pain in his head, and desired that it might be supported by the hand. The eyes had lost their expression, but the state of the pupils was not noticed. The patient thought the cathartic was about operating, and I delayed farther bloodletting in consequence.

Calling again at 10 o'clock, for the purpose of abstracting blood, I found the patient sitting up at stool; the medicine having now operated for the first time. The patient felt much relieved, and "thought that he was again started on his recovery." After a short conversation, I stepped into an adjoining room, and in about two minutes was called back by the attendant. He had assisted the patient into bed, whom I found slightly convulsed. A livid suffusion of the countenance betrayed the immediate danger, and excluded the hope of temporary syncope. The eyelids were half closed, the orbits upturned, and their muscles were fixed with tonic spasm. Nor did I witness the last effort at respiration, nor perceive even a convulsive movement of the respiratory muscles; but seizing the radial artery, was just in time to feel the last tremulous action of the heart, which immediately became insensible. The patient was perfectly dead in less than half a minute after my entrance into the room. The tem-

perature of the skin was rather warmer and dryer than natural. Two minutes before, when the servant had assisted him into bed, he replied to an inquiry, whether he felt more indisposed,— "no my good fellow, and in a day or two more we shall be riding out."

I apprehend, that in the foregoing case, the effusion in the brain was entirely subsequent to the invasion of the fever. There had been a most rapid absorption of the serous depositions, and the sanguiferous organs were highly charged with the fluid, when its excretion was suddenly arrested by the febrile paroxysm. This was a concurring cause of that irritation of the brain which resulted in the serous effusion. Pressure was thus suddenly constituted, and the impulse of an excited circulation contributed farther to the determination of the apoplectic paroxysm, which extinguished life almost on the instant. Something may have been owing to intestinal sympathy; since numerous cases are recorded where apoplexy has made its invasion during the operation of a cathartic.

Case III. — *C. G. T. Esq.*, another member of the bar of this city, aged 35 years, was attacked with hemiplegia of the left side on the 1st Sept. 1833. Form slender, neck long. A sedentary, temperate, and laborious student. Had consulted me a few days previously, on account of indigestion, to which he was occasionally liable.

At the time of the hemiplegiac attack, the pulse was small and oppressed, tongue coated yellow, bowels constipated, and urine reddish and deficient. Mind and senses unimpaired. No cerebral symptoms. Voice feeble. Bled him in an erect posture to ℥xxv. The pulse rose soon after the beginning of the operation, which was continued till he became faint. The circulation being thus liberated, some febrile action took place in a few hours afterwards, when I bled him again to the extent of ℥xviii. The operation was repeated on the two following days, — about ℥xvi. of blood having been abstracted at each time. Cathartics of calomel and castor oil were fully administered. The dejections were abundant, being at first blackish, then green, and ultimately natural. Blisters were also applied to the epigastric region, and some other subordinate measures were employed.

He gradually recovered the use of his limbs, so that, by the first of December, he could walk without much inconvenience. There remained, however, a dragging of the leg, and the arm possessed, perhaps, about half its natural power. The temperature of both of these extremities was often examined, and constantly found three degrees less than on the opposite side. The circumference of the affected thigh, (naturally slender,) at about four inches above the patella, was now one inch and a half less than that of the opposite limb. There was also a difference of three-fourths of an inch betwixt the arms at the same distance above the elbow.

Connected with these unpleasant circumstances was a permanent dilatation of the pupil of the right eye, which had existed for nearly fifteen years, and

which had supervened on an attack of constitutional fever; without, however, any cerebral symptoms, either at that time or at any intervening period. He became short sighted, and could see much better with the natural eye.(1)

I urged a retreat, for the winter, to a southern climate; and, preparatory to his voyage, he visited Philadelphia to consult that superior physician, Dr. Physick. Dr. P. recommended a liberal diet of animal food, to which the patient subsequently adhered.

He sailed from this port for Charleston, S. C. on the 12th Dec., and after a disastrous voyage of 17 days. during much of which time he was lashed to his berth, he reached that port in quite an enfeebled state. He began, however, to recruit immediately, and was able to walk a number of miles every day, till that which preceded his death; when, from a cold, he considered it prudent to confine himself to the house. His appetite was good, his food in the highest degree nutritious, and his digestion well established. Just previously to his death, he had an evacuation, which he pronounced to his brother very natural. They immediately after sat down by the side of each other, and the patient called up the happy scenes of futurity. Whilst thus discoursing, he suddenly leaned his head upon his hand, and in a very altered tone of voice, desired his brother to send for a physician. The patient was immediately placed on the bed in a state of complete insensibility, and in less than three minutes from the attack all signs of life had disappeared.

The body was brought to New-York for interment, but it was thought too late for an examination.

CASE IV.—*Peter Hawes, Esq.*, another member of the New-York bar, aged about 55 years, died instantaneously, Sept. 30th 1829. Frame stout and muscular, neck short; health rarely impaired; habits sedentary and temperate; addicted to mental application.

Saw him first on the 26th. Appetite had been for some time impaired. Tongue extensively coated white, moist, and reddish. Tenderness and pulsation at the region of the epigastrium. Pulse small and oppressed, about 80. Countenance flushed, headache and drowsiness. Bowels constipated. Urine reddish and deficient. Extremities cool. Strength greatly prostrated.

Bled him in an erect posture about ℥xiv. when there was some tendency to syncope. Blood very dark, and became buffed, and partially cupped in a wine-glass. Sub mur. hydrarg. grs. xx., to be followed by ol. ricini. A blister over the epigastrium.

27th. Too ill to see the patient, who was visited by my friend Dr. Gilbert Smith. The Dr. bled him to the extent of about ℥viij. the pulse giving way. The cathartic had produced dark, and slimy dejections.

28th. Resumed my attendance. The circulation better established, and heat

(1) Sir. B. Brodie relates a similar case in respect to the pupils, from an injury of the head, which had been attended by "manifest symptoms of pressure." (*a*)

(*a*) Med. Chir. Trans. vol. 14, p. 354.

more uniform. Pulse about 75, and soft. Some headache, face flushed, tongue continues loaded. Dejections very dark. Strength improved. Sub mur. hydrarg. grs. x., at night, and ol. ricini ℥ss. in the morning.

29th. Feels better. Stools green. Head more natural. Tongue as before. Some reaction; skin warmer than natural. Pulse 100, fuller, with some hardness. Sits up considerably against instructions; says "he is not accustomed to the bed." Ipecacuanha gr. 1. sub mur. hydrarg. gr. ss. every four hours.

30th. Made an early visit. Found the patient sitting up. Evidently not so well. Pulse small, about 100. Face flushed. Drowsiness so great, he frequently nodded between interrogatories. No headache. Prescribed jalap, grs. xv. tart. potass. 3ii. and that he should be kept in bed. Intended to bleed him in about three hours after taking the medicine.

About an hour after leaving the patient, I was summoned to visit him, in great haste. Was informed by Mrs. H. that he had continued to grow more dull and inanimate, and was at last affected with convulsions, and insensibility, of short duration,—"his face and neck being bloated with blood."

Found his face still injected; carotids and temporal arteries throbbing; pulse 90, and rather small; drowsy, and answered inquiries in a broken manner. Set him upright, and drew about ℥xiv. of blood, when the pulse began to fail, but rose immediately on restoring the recumbent posture. In about four minutes afterwards, his face became highly suffused, a slight convulsion took place, and within half a minute the action of the heart had become inaudible, nor was there any other remaining sign of life. The blood was very dark and threw up a buff, in a large bowl. Dr. J. K. Rodgers came in and remarked upon the latter appearance. Great infirmity of health prevented my taking any measures for a cadaverous examination of this interesting case.(1)

(1) A case very similar in all its aspects is related by Annesley. The patient was attacked with sanguineous apoplexy, whilst preparations were making to bleed him, after two "copious depletions by leeching." "He expired in a moment." There was effusion of blood in the brain. (a)

(a) On the Diseases of India, vol. 2. p. 476.

APPENDIX II.—TO ESSAY ON BLOODLETTING.

See. p. 300.

PATHOLOGY OF CEREBRAL HEMORRHAGE.

The question as to the dependence of cerebral hemorrhage upon a rupture of the vessels, or upon a process analogous to secretion, which is determined by a morbid state of the vascular system of the brain, is intimately connected with a philosophical treatment of sanguineous apoplexy. It affects deeply not only the treatment of the paroxysm, but the management of convalescence, and the means which should be subsequently taken to prevent a recurrence of the disease. If the hemorrhage truly depend upon morbid action, and that action, as in other analogous cases, consist of venous congestion or inflammation, the curative means must be more or less determined by this particular pathological condition. Considering, also, the great predisposition of an apoplectic subject to repetitions of the paroxysm, and the constant tendency of extravasated blood during its presence in the brain to renew the hemorrhagic action, it is obvious, if hemorrhage depend upon a vital process, that such general means should be continued after convalescence, as may tend to counteract the disposition to that particular mode of disease.

Dr. Abercrombie states the general doctrine on this subject, when he remarks, that "there is every reason to believe that sanguineous effusions in the brain depend upon the immediate rupture of a considerable vessel, without any previous derangement of the circulation; the rupture probably arising from the disease of the artery at the part which gives way." (1) Amongst all his cases, however, which occurred to his own observation, our author produces no instance in which such a rupture was detected.

Rochoux defines apoplexy, attended by hemorrhage, to be "hémorrhagie par rupture, suite d'une altération du tissue de l'encéphale."

Armstrong says, "in almost all cases of apoplexy which occur instantly, there is a rupture of a vessel;" and were it not for some important facts which we shall state, it might be supposed that Dr. Armstrong had some demonstrative proof of this opinion, since he remarks, that "he had seen two cases in which there was no rupture." (2)

(1) On Diseases of the Brain, &c. p. 237. So, also, Dr. Bright, Medical Reports, vol. 2, p. 331. Sir B. Brodie, in Med. Chir. Trans. vol. 14, p. 350.

Dr. Abercrombie is disposed to exclude these cases, which constitute the vast majority, from the denomination of apoplexy. (a) By most authors, however, they are considered genuine examples of the disease. Rostan says, "they should alone preserve the name." (b) So, also, M. Bouillaud: "Le nom *d'apoplexie* ayant été donné à plusieurs affections morbides de nature différente, je dois commencé par dire que ce mot est, pour moi, synoyme d'hémorrhagie cérébrale." (c)

(2) Lectures on Acute and Chronic Diseases, vol. 2, p. 556.

(a) Ibid. p. 236. (b) Sur Ramol. du Cerveau, p. 497.

(c) Mémoires de la Soc. Méd. d'Emulation, t. 9, p. 147.

It is the opinion of Dr. Hall, that "the injection of inflammation, in cerebral hemorrhage, is probably seated in the minute arteries and the capillaries; whilst the morbid anatomy, in these cases, consists in congestion or rupture of the minute veins and capillaries of the membranous substance of the brain." (1) This appears to us correct; only the morbid anatomy is generally more extensive, — especially as it regards the larger veins which are commonly in an injected state over a certain extent of the pia mater.

Many contend that the rupture is owing to an ossified state of the vessels; but "we should remember," says Rochoux, "that this morbid state has never been detected in any vessels beyond the pia mater, and which are imbedded in the substance of the brain." As to the doctrine of Müller, that blood cannot escape without a previous laceration of the vessels, on account of the supposed absence of terminal orifices, we shall inquire into its merits in another place. It is less, however, our object to show that the capillaries are not ruptured, than to prove the primary dependence of sanguineous apoplexy upon a formidable state of disease.

1st. Next to apoplexy attended by extravasation of blood, that variety is the most common which is only distinguished, in its morbid anatomy, by venous congestion of the brain. In this respect, it is exactly the counterpart of apoplexy attended by extravasation of blood, which appears to be merely a superadded result of the vascular congestion. The most essential pathological distinction in the cases appears to consist in a greater predisposition of the cerebral powers to undergo the change which results in the paroxysm, in one case than in the other. And that the predisposition is already constituted in cases of sanguineous effusion, before extravasation takes place, is abundantly shown by the frequent symptoms of an approaching paroxysm before the effusion happens. (2) In all these cases, "we never fail," says Dr. Cheyne, "to find the remains of greatly increased action, and great congestion, in the arterial and venous systems of the brain." "This is probably the most important, as it is the most unvarying appearance." (3)

2d. It is admitted, on all hands, that the vessels which are supposed to have given way are very rarely discovered. "It is in vain," says Dr. Abercrombie, "in general to attempt tracing the rupture to particular vessels; but Dr. Cheyne was able to do so in some instances." But even in these cases, Dr. Cheyne evidently saw nothing but the orifices of vessels lacerated by the extravasated blood. "In many cases," he says, "by the careful use of a camel's hair pencil, (a work of two or three hours, and of great delicacy,) we may at last show many vessels, all along the walls of the irregular cavity, not larger than a human hair, ending in small clots of blood; and he will sometimes find the same appearances in various and distinct parts of the brain. It is thence evident that the bleeding *does not depend on erosion, aneurism, nor ossification*, but on a great and simultaneous action of the smaller arteries of a hemisphere, or of the whole brain." (4) But since the coats of these vessels are stronger, in proportion to their size, than those of the larger arteries; and, as appears to be

(1) Lectures on the Nervous System, lec. 7, p. 144.

(2) So, also, "an attack of palsy, even when it does not lead to apoplexy, is sometimes attended with every symptom which denotes inflammatory disease." (a)

(3) Cases of Apoplexy, pp. 22, 24.

(4) Ibid. p. 30.

(a) Cheyne's Cases of Apoplexy, p. 54.

sufficiently shown, there is no evidence of erosion, &c., whilst, also, these minute vessels have, probably, a circulation independent of the *vis a tergo*, or, at most, but little influenced by it; and, it being also opposed to final causes, that vessels, whose coats are not diseased, should be ruptured by their own natural action, or even by the impulse of the general circulation, it appears to result, that the hemorrhage depends, as in other parts where a coincident state of the blood-vessels is constantly found to exist without rupture, upon a process analogous to secretion. Dr. Cheyne farther states, that "in none of his dissections has he been able to discover extravasated blood from the rupture of a considerable artery; and in the few dissections recorded, in which this accident had taken place, *most of the symptoms which usually characterize it have been absent.*" (1) In these rare instances, also, the coats of the vessels have been found diseased, whilst their natural strength is in an inferior ratio to the size.

Lerminier, Serres, and some others, have pursued the same inquiry, with the aid of powerful lenses, and, although they have discovered the orifices of the torn vessels, they have found nothing indicative of a disorganized state of their tissue. "In all these instances," says M. Serres, "the arterial and venous system of the brain was greatly dilated." (2)

Rochoux, and some other authors, have a particular section, embracing the few cases in which there has been unequivocal proof of a primary rupture of an artery. In these cases, the artery has been always large, and diseased. Rochoux knows of but nine examples; and he believes that Serres is the only individual who had witnessed the rupture of a vein. (3) True, M. Serres refers to 18 others, described by authors; but does not consider the proof satisfactory.

3d. When vessels are merely lacerated, even those of considerable size, there is generally but little hemorrhage; and this, too, where no mechanical pressure is exerted. The vessels, also, from which cerebral hemorrhage arises, are scarcely visible without the aid of a magnifying glass. The extreme subdivision begins before the arteries leave the pia mater. We shall not here argue the question of an independent action of the smaller blood-vessels, as we have endeavoured to show the fact in our article on inflammation. But waiving this controverted question, it is manifest, from what we have just said, that the power of the *vis a tergo* must be very feebly exerted upon the vessels which penetrate the substance of the brain; and with any independent action which they may possess, the hemorrhage from these minute vessels should be extremely small, unless that action be increased, and maintained more or less after their laceration, by disease. In the spinal marrow, also, M. Cruveilhier argues, that "apoplexy of this part evidently cannot depend on rupture; for none but vessels very fine, though extremely numerous, penetrate the gray substance." (4)

(1) Op. Cit. p. 41. (2) Annuaire Méd. Chirurg. t. 1, p. 246, etc.

How does the foregoing statement, as well as that just quoted from Cheyne, correspond with Dr. Macartney's late observation, that "arteries found in a congested part (venous congestion) are smaller than their natural size?" (a) Dr. Macartney's conclusion appears to have been founded upon the result of an experiment of applying a ligature to the jugular vein of a rabbit. Is this disease? Is it nature?

(3) Winteringham proved, by experiments, that a much greater force is necessary to rupture the veins than the arteries.

(4) Malad. de la Moelle Epin. Livrais. 3. p. 5.

Mr. Chevalier has recorded three interesting cases in the Med. Chir. Trans. of London; in

(a) Med. Chir. Rev. January, 1839, p. 135.

Again, pressure is powerfully exerted upon the lacerated vessels as soon as the hemorrhage begins, which should tend especially to arrest the effusion, were there not some powerful cause in operation. On the contrary, however, the hemorrhage from those minute vessels sometimes amounts to no less than 30 or more ounces; (1) the quantity depending on the extent of disease, the degrees of vascular action, and the enlargement of the vessels. Where action, too, is not obstinately established, it soon becomes suspended after the vessels are torn; and this, with the pressure, puts an early stop to the effusion.

4. We have presented a case in the note below, which might be safely taken as the foundation of a principle, when connected with all other considerations that relate to this inquiry, that cerebral hemorrhage depends on what is denominated hemorrhagic action. But our records abound with similar examples, where the effusion begins simultaneously in many distinct parts of the brain; presenting *ecchymoses* and other peculiar appearances, which are known to be characteristic of hemorrhagic action in other parts. Added to this, also, is an almost invariable coexistence of venous congestion, without any remarkable turgidity of the larger arteries. Can it be entertained, that the parietes of vessels should become diseased in numerous parts of the brain, and that these should be simultaneously ruptured, and form as many distinct extravasations? We shall cite a few instances.

Dr. Abercrombie states a case, in which, "on removing the scull-cap, an appearance was observed on the surface of the dura mater, of coagulated blood in small detached portions. These appeared to have been discharged from small

which he supposed the effusion to have depended on "the rupture of a vessel." He thinks that "similar mischief should frequently occur, in consequence of violent strains, is naturally to be expected."(*a*) But it is well known that effusions within the spinal marrow have never happened from such a cause; rarely in the substance of the marrow even from fractures of the spine. In two of Chevalier's cases, it is stated that the effusion consisted of a bloody serum, with evident marks of inflammation.

(1) Wepfer relates a case, in detail, in which he found more than two pounds of extravasated blood, but could discover no ruptured vessels; (nec vel ruptam venam, aut arteriam deprehendere valui.") (*b*) In Rochoux's 14th case, there was an effusion of two pounds of blood. He could discover no ruptured vessel, — "not even of a vein." He imputes the effusion to exudation. (*c*) In his 12, 14, 18, 22, 83, 84, 85, 86, 87, 88, and 89th cases, there were prodigious quantities effused, without any apparent rupture. A quantity of blood probably equal to two pounds was extravasated in a case related by Bouillaud; and although he wrote his essay, partly to prove that cerebral hemorrhage depends on rupture of the blood-vessels, he states that he was not able to detect the source of the effusion. That it was the result of a vital process, in the foregoing case, is obvious from the variety which distinguished the morbid appearances. "There were three ounces of bloody serum at the base of the brain. At the posterior extremity of the right hemisphere, there was an *ecchymosis* of the size of a three franc piece; and at the corresponding wall of the cranium there was a corresponding layer of coagulated blood. Within this hemisphere was an enormous apoplectic *foyer*. The surface of the corpus striatum and thalamus opticus was covered with voluminous, injected vessels, and we observed *around them elegant groups of ecchymosis*, — below which were *several smaller* apoplectic *extravasations*. The cerebral substance which surrounded the great cell was equally marked by *lenticular ecchymoses*." (*d*)

Here, then, were numerous points from which the effusion had begun simultaneously; all concurring to show, by the character of the effusion, as well as by the number of distinct places, that the effusion had no dependence on rupture. This case also exemplifies what we have stated in the text as to the extent of the hemorrhagic disposition when the effusion is large.

(*a*) Vol. 3. p. 102.
(*b*) Hist. Apoplecticorum, hist. 1. (*c*) Récherches sur l'Apoplexie.
(*d*) Mém. de la Soc. Méd. d'Emulation, t. 9. p. 151.

glandular-looking elevations on the outer surface of the dura mater, which were very vascular." (1)

Similar to the foregoing is a case by Dr. Bright, (2) in which a complete cyst was found in the substance of the brain, containing more than an ounce of serous fluid, and a clot of recently extravasated blood. The parietes were lined with a highly vascular membrane, the vessels being numerous, large, and loose. Within the cavity were four projecting papillæ, which were highly vascular, and appearing as if stained with blood.

Another variety illustrating the hemorrhagic disposition occurs in Rostan's 79th case, in which there was an immense extravasation. Small effusions occurred at different points. The corpora striata "présentent dans leur épaisseur deux ou trois points, d'un noir foncé, comme naissant d'orifices béants de petits vaisseaux." (3) Similar to this is a case in the Med. Chir. Rev. (4) and another is related in the Dublin Journal, (5) in which "the corpus callosum appeared to be studded with minute red points, looking as if any white surface had been sprinkled with red paint," and from which it was considered evident that the blood had been effused. (6)

Dr. Bright states a case, in which "the ventricles were all completely filled with a clot of blood and serum. The right crus cerebri was lacerated, soft, and full of dark bloody *spots*. The left crus and the portion of the brain immediately between the crura were in a similar state. There were two or three small coagula in the right thalamus; but they appeared to be quite detached. There was a very small spot of the same character in the corpora quadrigemina." (7)

Mr. Howship has a case in which the extravasation of blood appears to have proceeded from innumerable vessels. It represents the hemorrhagic action in its incipient state. "The vessels of the pia mater were found extremely turgid with blood, but did not appear to have given way at any external point. The cortical substance was darker than usual; but, wherever divided, there was very much the appearance of *petechiæ* upon the skin. This appearance probably arose from some alteration in the structure, or action of the capillary vessels, by which a portion of the colouring matter of the blood had been allowed to escape, without proceeding to any more obvious degree of extravasation. In the left lateral ventricle, there were two ounces of serum. *Several* small coagula of blood were observed near the fourth ventricle, on the posterior surface of the medulla oblongata; and, on making a vertical section of the medulla, *several thin strata* of blood, approaching to a state of coagulation, were

The foregoing appearances of local vascularity and ecchymoses have never attended any of the few cases of undoubted ruptures; and M. Bouillaud relies for his induction upon four cases of rupture of the internal carotid and basilar arteries, which he quotes from other authors.

Bonet, Baglivi, Lieutaud, Corvisart, Kellie, and others, have quoted the case of the celebrated Malpighi, as supplying an instance in which two pounds of blood were extravasated in the brain. But Lancisi, who was present, and communicated a statement to the Royal Society of London, makes the quantity only two ounces. (a)

(1) On the Brain, &c. p. 250. (2) Medical Reports, vol. 2. case 146.
(3) Op. Cit. (4) July, 1832.
(5) Dublin Journ. of Med. and Chem. Science, vol. 2. p. 141.
(6) This appearance is frequently noticed by authors.
(7) Medical Reports, vol. 1. case 13.

(a) Philos. Trans. Lowthrop's Abdgt. vol. 3. p. 31.

observed. These coagula were deposited at *some distance* from each other, and not in a line." (1)

Cheyne's 11th case shows the vast extent of the hemorrhagic action in cases of great effusion. The patient was the subject of an obstinate intermittent fever. There was a layer of blood over the right hemisphere under the *dura mater*. Below the falx, "there appeared a very large quantity of coagulated blood between the two hemispheres," &c. "reaching almost to the base of the brain. Both ventricles full of blood." "Above the left ventricle, there was an extensive lodgment of blood in the substance of the brain." "There were *distinct* and *separate* clots of blood found in *different* parts of the brain, in both hemispheres, and particularly in the left anterior lobe, and in the corpus striatum of the right side." "The pia mater, over a great part of the brain, was suffused, as if a brush, dipt in blood, had been drawn over it." (2)

In Dr. Bright's 132nd case, (3) the ventricles were excessively distended with blood, and the substance of the brain ecchymosed. "There were two small spots of extravasated blood in the corpus callosum, and several of much more considerable size in the pons varolii; they were long, thin, and parallel, and placed transversely." Dr. Bright has given a fine plate of this case. This peculiar arrangement of the linear clots is not uncommon.

Dr. Craigie observes, that in many of the cases of sanguineous apoplexy which have fallen under his observation, "the blood, instead of being in the shape of clots in distinct cavities, consisted simply of long linear streaks, stretching through parts of the brain, sometimes in the neighbourhood of blood-vessels. In such cases, the blood-vessels are *much injected*, especially in the vicinity of the membranes." (4)

In Cheyne's 7th case, "the pia mater was marked all over with deep red, and purple patches of inflamed vessels, and streaks of extravasated blood, which prevailed equally over both hemispheres, and at the base of the brain." (5)

M. Gendrin says, that "when the inflammatory congestion is great, and the disease occurs in some vascular part of the brain, filaments and clots of blood are deposited, from which the part receives a red striated appearance." (6)

In Rochoux's 12th case, we have a remarkable instance of hemorrhagic effusion, which occurred in distinct and numerous places, throughout the brain; parallel to which are two cases by Mr. Dance, in the Dublin Journal. (7)

M. Bouillaud relates an instance, in which there was "an extravasation of blood in the corpus striatum, or rather an *infiltration* of blood, extending an inch and a half in all directions." (8) M. Lallemand relates a case, where the lining of the ventricles was of an amaranthine red, and the cerebral substance below the left ventricle was of the same colour. He found in the left ventricle three spoonfuls of bloody serum, and in the right ventricle three spoonfuls of black, coagulated blood; while *below* the ventricle, in the substance of the brain, was another clot of the same size. (9) Here we have a common action producing bloody serum in one ventricle, and pure blood in another; whilst there was a distinct hemorrhage in another part of the brain.

(1) Observations in Surgery and Morbid Anatomy, p. 59.
(2) Op. Citat. p. 110. (3) Op. Citat. (4) General and Patholog. Anat. p. 401.
(5) Op. Citat. (6) Hist. des Inflammations, t. 2. p. 114. (7) Ut Citat.
(8) Mémoires, etc. ut Citat. p. 150.
(9) Récherches Anatom. Path. Sur. l'Encéphale, etc. p. 30.

Dr. Cheyne "has found many vessels, along the walls of the cavity, not larger than a human hair, ending in small clots of blood, in various and distinct parts of the same brain." (1)

In Bright's 24th case, (2) of a child 20 months old, we have an instance of great vascular injection without rupture, and numerous points of hemorrhagic effusion. The whole vertex was covered under the membranes with extravasated blood of a deep purple colour. A little above the ventricles, a number of small, round, dark-red spots, more *venous* than arterial in colour, were seen thickly distributed in patches." Although "a high magnifying lens was employed, the *vessels* from which these little clots of blood had escaped could not be discovered;" nor could any rupture of the larger vessels be detected. In his 134th case, the left ventricle contained a large clot of dark-red blood. There were, also, "several smaller clots, each of the size of a pea, bearing the appearance of having come from separate vessels. There was also a bloody serum distending the right ventricle." In case 136th, "two ounces of blood were effused into the anterior lobe of the right hemisphere of the cerebrum. The walls of the cavity were uneven, in some parts pretty solid, but in others broken down and ragged. In some parts, there were *numerous small brown points* in the surrounding cerebral matter, like a *cluster of petechiæ*. On the surface of the cavity issued many bloody points; and when pressure was made near the walls of the cavity, these rapidly increased; but it did not appear to me that *any opened* completely *on the surface*, — the blood appearing to be effused *beneath* a very thin membrane, or only a surface of condensed brain. (Plate 22, fig. 2.") Vessels were seen ramifying on the surface. "Several of them were distended and varicose." "I carefully washed away a little more of the cerebral matter, and then, at not more than the fifteenth of an inch beneath the surface, brought into view a radiating bundle of large vessels running from the large vessels in the fossa sylvii towards the outside of the corpus striatum."

In a case by Mr. Howship, a large extravasation of blood had taken place from the corpus striatum, "which appeared *very tumid* and *much darker* in its colour than natural." The corpus striatum was not broken down; but the blood issued from two points, and was found accumulated immediately beneath. (3)

Rostan remarks, that "we often find, in sanguineous apoplexy, a bloody infiltration of the pia mater, especially on that side where the hemorrhagic effusion has taken place;"(4) thus evincing a disposition in the exhalants to pour out blood, simultaneously, in the different tissues. "Sometimes," says Cheyne, "the whole surface of the pia mater acquires a bright vermilion tint, and between the minute and florid vessels there are patches of scarlet extravasation. The membrane at these parts is bloodshot." So also many others. In a case by Mr. Howship, "the pia mater appeared extremely vascular and red. The vessels, both as to number and magnitude, were amazingly increased. The most remarkable circumstance, however, in the state of the pia mater, was a degree of diffused extravasation of blood which had taken place. It might be traced upon almost every part of the membrane, — upon the basis, as well as

(1) Cases of Apoplexy, p. 39.
(2) Op. Citat. vol. 2.
(3) Observations in Surgery and Morbid Anat. p. 58.
(4) Réch. Sur l'Apop. p 144

the lateral and superior parts of the cerebrum. The extravasated fluid had formed superficial coagula, corresponding to the sulci, between the convolutions of the brain. The effusion seemed not only to have arisen from the capillary arteries upon the external surface of the pia mater, but also from the processes of the membrane that dip down between the convolutions forming the tomentum cerebri." (1)

The same appearances have been observed in the theca vertabralis. In the only case of spinal apoplexy recorded by Dr. Bright, there was a clot of blood near the upper dorsal vertebræ, "and lower down, the internal ligament was, to some extent, deeply marked by ecchymosis, as if in progress towards the formation of another similar affection." (2) In a patient, affected with hydrocephalus, aged 7 years, numerous small spots of ecchymosis were found in different parts of the brain. (3) In a case of "meningitis," by Cruveilhier, attended with coma and loss of sensibility, "the whole surface of the brain under the arachnoid was covered with a large quantity of serum, containing pseudo-membranous flocculi. The ventricles were distended with a milky fluid, and the surface of the lateral ventricles was spotted with ecchymoses of variable dimensions." (4)

Several instances are recorded, where the effusion has taken place from the inner surface of the dura mater, and between this membrane and the duplicature of the arachnoid by which it is lined. Did these effusions depend on rupture of the vessels of this firm membrane, it is hardly probable but the same force of the circulation would have extended its ravages to the more lacerable parts. These cases are also marked by a plethoric state of the blood-vessels, and by serous effusions in other parts of the brain. Dr. Bright's 31st plate (5) represents an ecchymosis between the dura mater and the duplicature of the arachnoid, which beautifully illustrates the incipient stage of cerebral hemorrhage. "The whole internal surface of the dura mater was marked with small spots of ecchymosis; showing a hemorrhagic tendency in the vessels of the arachnoid lining the dura mater." (6)

5. That cerebral hemorrhage depends upon a process of vascular action, which is carried on during the effusion, is manifest from the small quantity of blood which escapes when the brain is sliced up by a knife. M. Serres, (7) in his experiments upon the brain, after excavating the substance, was generally obliged to open the longitudinal sinus, to effect his purpose of producing an effusion of blood.

Again it is stated by Brodie, Abernethy, and others, that coma is never induced by wounds of a smaller vessel than the trunk and large branches of the middle meningeal artery; and, as to the veins, the former remarks, that he has never known a wound of the longitudinal sinus to produce any derangement of the functions of the brain, whether the effusion took place above or below the dura mater. (8)

On the other hand, when the capillaries are engaged in hemorrhagic action

(1) Op. Citat. (2) Op. Citat. vol. 2. p. 340. (3) Ibid. p. 263.
(4) Anat. Patholog. Livrais. 8. (5) Med. Reports, vol. 2.
(6) They were more probably the vessels of the dura mater, since red vessels have never been seen, according to the best authorities, in the arachnoid membrane, even when inflamed.
(7) Annuaire Méd. Chir. 1819. p. 246, &c.
(8) Med. Chir. Trans. vol. 14. p. 334.

in the mucous lining of the lungs, stomach, &c. the effusion of blood is often, speedily and enormously great.

6. The same is apparent from the frequent invasion of apoplexy when the patient is at rest in bed, and when the general circulation is most tranquil. (1)

7. It is equally shown by the rarity of its occurrence when the general circulation is violently excited in fevers, or by exercise, anger, &c.; whilst, on the other hand, it is common when the circulation is depressed by congestive affections of the abdominal viscera.

8. It is shown by the extreme rarity of cerebral hemorrhage in the most violent cases of phrenitis.

9. It is shown by the nearly uniform absence of hemorrhage when the blood is suddenly and violently accumulated in the brain, in cases of hanging, and especially when the jugular veins are tied in man and animals. "How much soever the vessels of the head are excited and loaded in consequence of strangulation, the blood is never found to have escaped from the vessels." (2) This, and many other facts, are opposed to Dr. Armstrong's conclusion, that when apoplexy occurs in bed, it is owing to an imperfect return of blood from the head, (3) and to the opinion of Dr. Bright, (4) Cruveilhier, and other able observers, that cerebral hemorrhage proceeds from obstructions to the circulation, or from a mechanical distension of the vessels. Or if, again, such be the true causes, why does not hemorrhage result from the cerebral congestions of children, where the vessels are more fragile than at adult age, and in whom the distension of the veins is often greater than rarely happens in apoplexy? If emphysema, as supposed, be one of the obstructing causes, why did not cerebral hemorrhage take place in that "most striking case of venous congestion ever witnessed by Dr. Bright, in an old man who died with fever and emphysema of the lungs"? (5) Why does it not sometimes follow the highly congested state of the cerebral veins in typhus fever? Why not in the insane, who are so frequently affected with cerebral congestion? We apprehend, for the reason that the hemorrhagic action does not exist.

10. It appears from the extravasation taking place, in most instances, from the most highly vascular parts of the brain; generally from the corpus striatum and its adjacent parts, and the optic thalami. (See p. 347 *note*.)

11. Hypertrophy of the heart has been often assigned as a concurring cause of apoplexy. This coincidence, however, has been entirely overrated. A few cases have been put forth with so much emphasis by mechanical pathologists, that one is almost induced to believe that hypertrophy of the heart is necessary to apoplexy. But when the coincidence is contrasted with the vast number of apoplectic cases in which it is absent, it must be regarded as accidental. At

(1) Morgagni says, that those who die from cerebral congestion are often found dead in the bed; and Cheyne remarks, that "the attack occurs in the course of the night, oftener than at, or immediately after, dinner." (*a*)

(2) Op. Citat. p. 166. Cook states two exceptions. See, also, Morgagni, Ep. a. 11. 15, 36. Lancisi de Sub. Mort. Obs. 4. M. Piorry's Collection des Mémoires. pp. 291, 302. Déjean, in Mém. de l'Acad. des Sciences, 1782. Bœrhaave de Morb. Nervorum, p. 671. De Haen, Rat. Med. t. 1. Portal sur l'Apop. p. 300. Coleman's Dis. on Suspended Respiration, 1790. Kellie, in Medico. Chir. Trans. Edin. 1824., pp. 113, 128, 163, &c., and many others to the same effect.

(3) Lectures on Acute and Chronic Diseases, vol. 2. p. 273.

(4) Medical Reports, vol. 2. pp. 292, 231. (5) Ibid. vol. 2. p. 670.

(*a*) Cases of Apop. p. 144.

most, however, if hypertrophy of the heart do contribute to cerebral hemorrhage, it is only by the tendency of a violent propulsion of blood, when long continued, to develope that morbid action from which hemorrhage results, or to start into hemorrhagic action congested vessels of the brain which may have originated in other causes. The philosophical Dr. Parry remarks, that in certain degrees of vascular atony, we may conceive the usual impulse of the heart sufficient to overcome the resistance opposed by the tonicity of the exhalants, and thus allow blood to be effused." And he adds, that "a state similar to this in kind, though less in degree, seems to occur with regard to some of the contents of the exhalants, in the cold sweat which bedews the surface of the body, especially about the head, in syncope; and also in that, which is observable in many cases a short time before death." (1) These two examples, so apparently opposite, demonstrate the error, when taken in connection, of referring the former phenomenon to the mechanical impulse of the circulation. This power, with the increased quantity of blood which it determines upon the part, operates only as a stimulus to the vital actions of the "exhalants." In the latter instance, no such principle has any existence; and yet the exhalation is sometimes profuse. So, in the cholera asphyxia, when the pulse may be nearly extinguished, the discharge from the skin and intestines is often excessively profuse. There is no intelligible mode of accounting for the phenomena, in these cases, but by referring them to an independent vital action of the capillary and exhalant vessels. It may seem not a little strange, that such actions should spring up, just as the forces of life are on the verge of extinction; but we must take the facts as they are, — not as our speculations and experiments would have them. Could we understand the exact changes in the vital powers, which constitute the foregoing conditions, perhaps we might cease to wonder at the results. But, it being abundantly manifest, that the effusions, in the foregoing instances, are independent of the *vis a tergo*, and are wholly the consequence of capillary action, we may safely carry the same interpretation to other analogous effusions where the general circulation is undiminished, or exists in preternatural force. And here we would express our desire, that what we have now stated in regard to effusions which occur when the powers of life are greatly altered, — approaching, even, their extinction, — and especially the fact that violent local inflammations spring up in the cholera asphyxia after the general circulation has come almost to a stand, — should be duly considered, when we speak, in another place, of the philosophy of that morbid condition of the brain and other parts which is found in subjects who perish from frost.

Dr. Parry, at another time, regards the foregoing subject in a different, and, as it appears to us, its true aspect. In respect to hemorrhage, a principle would follow, which resolves what have been often regarded as different modes of disease into a uniform, specific action, and which establishes the identity of "active" and "passive hemorrhages." It seems to us so important in its application to cerebral hemorrhage, that it should be stated in the language of our author. "Hemorrhage," he says, "like inflammation or dropsy, is not directly as the local disposition of the vessels, or as the degree of the momentum of blood; but in a ratio compounded of the two. Whence it follows, that a great degree of momentum may be required in order to produce hemorrhage in vessels which

(1) Elements of Pathology, vol. 1. pp. 159, 389. &c.

are little disposed, — while a slight degree will be sufficient in vessels which are strongly disposed." (1)

Here, it appears to us, is the whole explanation of any participation which hypertrophy of the heart, or any other cause which increases the force of the general circulation, may have in cerebral hemorrhage. We believe that every fact warrants the conclusion, that the augmented force of the circulation only increases the hemorrhagic disposition by its influence on the vital forces of the vessels which are engaged in the hemorrhagic process.

Again, hyperthropy of the heart is a common affection; and since, therefore, it rarely leads to apoplexy, we argue, farther, from this fact, that cerebral hemorrhage is independent of the *vis a tergo*. We might quote many observers to sustain our affirmation. The accurate Dr. Kellie remarks, that "of several cases of enlargement, and of other structural diseases of the heart, which have come under his observation, not one of the patients had lethargic or apoplectic symptoms. In 54 cases of hyperthropy of the left ventricle, according to M. Bouillaud, there was cerebral hemorrhage in only six. M. Louis reported 45 cases of organic disease of the heart which occurred at La Charité, in no one of which had there been any apoplectic symptoms. Rochoux does not believe that apoplexy has much, if any, connection with hypertrophy of the heart. (2) Kellie gives some examples, which are in point. In one case, "the blood was projected with surprising force upon the aortic arch, the carotids, and the head." Alarming hemorrhages took place from the nostrils and per anum. "There was, also, violent sickness, and bilious vomiting. The heart was found enlarged to twice the usual size, as well by a real increase of muscular structure, as of capacity." But, "powerful and awful as were the exertions of this heart, there was yet no stupor, no lethargy, no apoplexy, no congestion, no rupture of vessels within the head, although such congestion and rupture happened in other parts of the body." (3)

12. Sanguineous apoplexy has often occurred epidemically, and mostly at a particular season of the year. The Roman authors have recorded many of these epidemics, and represent the subjects as often falling down in the streets. From Baglivi's account of those which prevailed at the close of the 17th century they were evidently produced by an unusual constitution of the air. (4) Lancisi says that the worst of them happened about the equinox, or summer solstice. (5) Hippocrates, Forestus, F. Hoffman, and others, who have witnessed these epidemics, ascribe them to atmospheric influence. It is also remarkable, that during these epidemics other diseases often diminish in frequency. Hippocrates notices this fact; (6) and Lancisi says of it, "grassantibus apoplexiis, cæteri morbi, et mortes, Romæ minus frequentes." (7)

These cases are of the "strong kind." Morgagni, in speaking of the epidemic at Padua, in 1729, "when numbers were cut off instantly, to the great consternation of every body," states that they died of cerebral hemorrhage. (8) These patients were not affected with febrile paroxysms, nor were they exposed

(1) Elements of Pathology, vol. 1. p. 387. (2) Discussion at the Royal Academy, 1836.
(3) Dr. Kellie in Edin. Med. Chir. Trans. 1824, p. 141. &c.
(4) Opera, p. 499, &c. de Apoplexia Epidem.
(5) De Sub. Mort. p. 69. (6) Popular. 1. s. 2.
(7) Ut. Supra, pp. 95, 90. (8) L. 1. Ep. 3. s. 11 and 13.

to any causes of excitement. They dropped suddenly from their seats, or while walking, or were found dead in their beds.

Now, is it not incredible, that at a particular season of the year, and without any obvious cause, there should arise such epidemic ruptures of blood-vessels; and that 90 individuals, from this cause, should have dropt dead in the street during the procession of St. Gregory?

That the affection, however, was owing to venous congestion of the brain is not only probable from the foregoing considerations, and from analogy, but from the fact that this condition was well ascertained by numerous dissections.

13. The great liability of the subjects of apoplexy to repetitions of the paroxysm, evinces its dependence on vascular action; since it is in the highest degree improbable, that these particular individuals should be constantly exposed to ruptures of the cerebral vessels. The recurrence of the disease cannot be imputed to any mechanical abrasion by the coagulum, since we commonly find the recent effusion in another, and often remote, part of the brain.

14. Cerebral hemorrhage is often manifestly the result of sympathetic influences exerted upon the brain by congestive diseases of the abdominal viscera. And again, it has its origin, in very numerous cases, in the same kind of influence which is determined by an overloaded stomach. The cerebral congestion existing antecedently, the violence of sympathy establishes the hemorrhagic action.

15. The frequent coexistence of serous effusions, and of bloody serum, with cerebral hemorrhage, but in distinct parts of the brain, refers the latter to the same process upon which the serous effusion had depended. This induction is strengthened by the vascularity of the brain, by analogy, &c.

16. Analogy affords a strong proof that cerebral hemorrhage depends upon morbid action of the capillary vessels. Almost by common consent, this is now a fundamental doctrine in relation to hemorrhages in other parts.—(See our Appendix on Endosmosis, &c.) The analogy is also complete as it regards the morbid appearances of the blood-vessels. The effusion is one of the great final causes of nature for the relief of certain conditions of disease. If it add to the evil in the brain, it is no reason why nature should step aside, in this instance, from her fundamental laws. They operate here exactly in the same way as in other parts. Consequently inflammation of the brain terminates in the same products as in other organs,—in the effusion of lymph, of serum, and of pus. These results are probably designed by nature as curative means; since we find that disease is more or less subdued by their supervention.

17. The seat of vascular fulness, in sanguineous apoplexy, is the venous system. The larger arteries are not in a plethoric state during life, as we have every reason to believe. It is only the capillary [illegible] which acquire a preternatural volume, and this is frequently absent. There is, therefore, no remarkable mechanical cause operating upon this system of vessels, to produce their rupture; whilst gravitation, and the suction power of the heart, lead us to explain the venous plethora on some other principle than mechanical distension.

18. Cerebral hemorrhage is a rare attendant on the part affected in ramollis-

ement (1) ulcerations, and other disorganization of the brain, which either involve an erosion of the blood-vessels, or place them in the most favourable condition for laceration. In a case by Dr. Bright, "many vessels of considerable size might be traced, the trunks of which seemed to run along the internal surface of the sac, and their fine, extreme divisions to float in the curdled pus with which the cyst was lined." (2) We infer, *a fortiori*, therefore, that cerebral hemorrhage does not depend upon ramollissement of the brain, —as well, also, from the antecedent symptoms.

19. Cerebral hemorrhage occurs in parts of the brain where the vessels are most concerned in the production of a serous fluid,—either upon surfaces, or interstitially. It never occurs, unless from ulceration, between the skull and dura mater, and very rarely beneath the pia mater, where the blood-vessels abound, and where they are often greatly distended. Neither do we meet here with morbid depositions of serum. From the connection of the blood-vessels of the dura mater with the skull, and those of the pia mater with the brain, nature, ever provident, has been either very sparing of her excretory apparatus in these parts, or has exempted them from conditions of disease which result in effusions, the effect of which would be a laceration of the communicating vessels. In the meshes of the pia mater, however, we often find considerable depositions of serum; and here, likewise, extravasations of blood frequently occur.

20. If hemorrhage were determined by the *vis a tergo*, or depended upon a diseased state of the vascular parietes, there should be a constant renewal of hemorrhages in the apoplectic cavities, as soon as the force of the circulation returns, and there should be no cicatrization of the cavities.

21. The symptoms which precede an attack of sanguineous apoplexy are generally indicative of cerebral congestion,—so much so, indeed, as frequently to suggest the means for prevention. If they be absent at other times, so also disease results in great disorganization in other parts, the brain especially, without any attendant symptoms.

22. Age is an element in predisposing to apoplexy. Rochoux and others have shown, that it is most frequent about sixty, and diminishes towards seventy. This farther evinces that ruptured vessels from an ossified state, and organic affections of the heart, are not connected with the apoplectic state, as cause and effect. Both these organic lesions progress rapidly in old age. But the actions of life become more languid after the age of seventy, and venous congestions are sensibly lessened.

23. The treatment of apoplexy in no respect corresponds with the general theory as to its pathology. It is the only instance in which bloodletting is applied to arrest a hemorrhage from ruptured vessels, or such as are supposed to be ruptured. Much less do we apply leeches and blisters to a part for a like purpose. But all these remedies are manifestly among the great curative means of apoplexy. It is, therefore, a necessary induction that the pathology of sanguineous apoplexy is constituted by morbid vascular action.

(1) M. Bouillaud affirmed that he had never known an instance of cerebral hemorrhage accompanied by an unequivocal state of ramollissement of any part of the brain. Louis does not think them connected. (*a*)

(2) Med. Reports, vol. 2. p. 151.

(*a*) Discussion at the Royal Academy, 1836.

General bloodletting is the most efficient mode of arresting hemorrhage which depends upon the vital action of the capillary or exhalant vessels. Its operation, as we have endeavoured to show, is directly upon the vital powers of the capillary blood-vessels. In hemorrhagic action, it modifies their powers and functions in the same way as in inflammatory affections. The action is then arrested, the vessels contract, and the hemorrhage ceases. It has not resulted from a mechanical diminution of the volume of blood, or the force of the general circulation. "Nothing is more common than to find the pulse, upon the first attack of sanguineous apoplexy, weak, languid, and compressible, and becoming strong and full after the brain has become in some degree relieved by large bloodletting." (1) And so in many congestive diseases of other parts, when attended by hemorrhage. The effusion ceases, notwithstanding the increased vigour of the circulation.

24. One of the strongest proofs that cerebral hemorrhage depends upon congestion or inflammation, as its proximate cause, is the vast quantities of blood which may be abstracted from subjects that are threatened with an attack. The philosophy of this we have already examined.

If we have dwelt long upon the foregoing subject, it has been from our conviction of its importance as it regards the treatment of apoplexy, and the management of convalescence. If the pathology of the disease consist merely in rupture of the blood-vessels, the treatment of the paroxysm by bloodletting, &c. would not be philosophical; whilst, on the other hand, a neglect of the true pathology has induced common errors in the management of patients who have just emerged from the attack. We consider, especially, stimulating food after a partial recovery from apoplexy as retarding convalescence, and, more than any other cause, contributing to a return of the paroxysm. There appears, also, to exist in numerous cases a morbid desire for food.

"Antequam de remediis statuatur, primum constare opportet, quis morbus et quæ morbi causa, alioqui inutilis opera, inutile omne consilium." (2)

(1) Abercrombie on the Brain, &c. p. 304. (2) Baillou, l. 1. Cons. 14.

THE HUMORAL PATHOLOGY.

SECTION I.

"Quot vota concepta, *Microscopica* recepta *oculorum indicia decepta*, circa *corpusculorum* constitutionem sanguinis, lymphæ, imo, neſas, *spermatis* figuram, structuram, &c. Porro, ex *Chymia*, nobilissima illa cæteraquin, et jucundissima arte, quot inconsultæ applicationes *fermentationis*, ad naturales et præter-naturales constitutiones et alterationes *humorum* corporis humani; *effervescentiæ*, cum organismo anamali confusiones: Motuum *intestinorum* fermentalium cum *extraneis* mechanicis, a *locali* commotione fluctuatoria pendentibus, improportionatæ comparationes? Ex *Physica* denique improvidæ applicationes motus microcosmici ab ære, æthere, colore, fluiditate, dependentis, cum motus microcosmico, in corpore animato, arbitrarie, congrue, scito, directo, instituto et gubernato, &c."— *Stahl's Opuscula, p.* 53.

"Inanis gloriæ desiderium simiola fuit, quæ medicos omni ætate compulit ad sectas condendas potius, quàm ad nova indies detegenda phænomena, quæ morborum historiam illustrarent confirmarentque."— *Baglivi, Op. p.* 35.

"Sic hypothesibus, quibus hodie tantopere delectemur, quid incertius?"— *Ibid. p.* 26.

"As believers in philosophy, we claim unbounded liberty of thought, and by thinking we hope to arrive at truth."— *Prof. Tappan's Review of Edwards on the Will.— Introduction, p.* 4.

Having hitherto investigated the character of the forces and actions of life, we are better prepared for considering the important subject of the humoral pathology.

What recollections are not inspired by our introductory sentence! What mind so insensible to the past, that it has not already travelled over the various eras of medicine, and passed in review those countless sages that gave distinction to each? Who has not traced from Galen to the last of the race, the brilliant achievements, the heroic renown, the unexampled career of humoralism? Who has not fancied that *last* man standing in solitary, hopeless defence, like Caius Marius swearing revenge over the ruins of Carthage? Dividing into adverse schools, they yet maintained a common bond of union through the doctrine which is now uniting us with remote ages, and with every empyric in the land; "ut præter omnium expectationem ex quadam veluti mistione rationis et experientiæ; quod sane tanquam nuncius veritatis trans Alpes, atque trans maria, in omnes denique medicorum omnium academias admirabile celeritate pene-

trat, et ab omnibus indiscriminatim recepta est;"(1) et "cujus *nomen* tamdui stabit, quamdiu movebitur sanguis."(2)

But perhaps this is not remarkable "in an inquisitive age, when opinions spring up, flourish, decay, and give place to others with a rapidity, to which one scarce finds any thing equal, except among the processes of nature in the tropical regions."(3)

Galen was the founder of humoralism. The physicians at Rome were now divided into numerous sects, and, as in our own day, the Methodists abounded most. The dogmatists had also divided with Hippocrates, Asclepiades, and Erasistrates. Galen had too, the advantage of the peripatetic philosophy, which had become the reigning system; and he was thus enabled to erect imaginary hypotheses upon suppositious data, instead of adhering to the Hippocratic method, which had been transplanted by Celsus upon Roman Medicine, of studying the living actions of nature, and the phenomena of disease. The times, therefore, like our own, were auspicious to innovations upon the Hippocratic philosophy, in its application to medical science. Galen established the doctrine by the force of assumption; and, leaning upon the philosophy of Aristotle, its plausible subtleties were carried into Egypt, Arabia, Persia, and over all Europe, where it continued to flourish even long after the Baconian reformation.

Still be it said, in behalf of our practical ancestors, that their treatment of diseases was rarely determined, in its essential points, by their theoretical views in relation to the blood; and, perhaps, as it regards our forefathers, there is not in the whole history of medicine, a more remarkable exemplification of the general abstraction of great speculative views from the indications which are supplied by the real phenomena of nature. The latter were the paramount guides among all the great apostles of humoralism. This we have abundantly shown in other places. The hypothesis was but the recreation of fancy. It rarely found its way into the chambers of the sick. Or, if it sometimes contributed its part, it was generally as a *placebo* to the imagination. There was only now and then an attempt to cure malignant fevers by soda or salt. The lancet was one of the great remedial agents. It was efficiently employed by Galen and his followers, even more so than by the vitalists of the present day. At an

(1) Baglivi Prac. Med. l. 1. c. 12. (2) Ramazzini Oratio Secunda, p. 11.
(3) Beddoes' Researches on Fever and Inflammation, p. 1.

earlier era the whole gist of the writings of Hippocrates, Celsus, &c. goes to enforce the importance of founding all our inductions, and practical conclusions, upon the phenomena of nature. Still, many of the old masters have done their mischief. They have been more or less looked into at all ages; and although their readers may have aimed at all parts of their books, they have been often carried away by a hypothesis that promises something to the senses in a tangible shape, — something for precipitation, or for the art of the crucible. Montesquieu compares them to the village curate, who has shown the moon through the telescope, or saw in the glass only the steeple of his own parish church. (1)

Like its harmonizing influence upon the ancient schools, the revival of humoralism is more or less greeted by all parties, and all are more or less shaking hands over the resurrection. But chiefly is it the *protege* of the iatro-mechanical and iatro-chemical schools; which, in their turn, have reappeared in their ancient costume. The countenance bestowed upon this resurrection is very various, according to the shape it is apprehended the ghost may ultimately assume. For the phases of humoralism have been as numerous as the doctrines of fever; of which "there are as many schools as there are of theology. *Fever* is nothing but debility, teaches one school; *fever* is nothing but inflammation, inculcates another; *fever* is a morbid state of the brain, says one theorist; *fever* is a morbid state of the intestines, says another: one is giving wine, while another is drawing blood; and some, disgusted with such empyricism, or deterred by such opposition, leave nature to herself," (2) believing with Rutty, that "those who are left to God's Providence, and cold water, have the best chance of recovery." "Reipsa enim febris monstrum est, cui non secus ac Furiarum maximæ, sunt nomina mille, mille nocendi artes." (3) Or, as Lieutaud has it, "in Cimmeriis latet tenebris genuina febris indoles." "Non nostrum inter vos, tantas componere lites."

Just so has it been with humoralism; nothing settled, nothing consistent; and although the modern humoralists have written upon the wall, that "one theory has followed the other almost as closely as the evanescent spectres in the ghost-scene of Macbeth," (4) still we shall see that the march is onward. Humor-

(1) Défence de l'Esprit des Lois.
(2) Med. Chir. Rev. Lon. vol. 11. p. 340.
(3) Ramazzini, Oratio Quarta.
(4) Stevens on the Blood, &c. p. 177.

alism, too, as we shall show, with all its professions of a compromising spirit, is, nevertheless, disposed to be exclusive. The former is a winning expedient, and has had its game. But it is all *black* blood for miasmatic fevers, and all *pus* for hectic and often, indeed, most other fevers. "Pus," says Dr. Hosack, "produces a peculiar form of fever, called hectic fever; and this, I believe, is the only way in which hectic fever is *ever* formed; I have never seen hectic fever but from this cause." Mr. Gulliver considers pus the "proximate cause of sympathetic, inflammatory, sympathetic typhoid, and hectic fevers."(1) We shall see farther on, that M. Andral, with all his apparent qualifications, and other distinguished humoralists, make a general sweep of diseases.

We therefore protest, "once for all," against any charge of exclusiveness from the opposite party. "Let the beam be first cast out, &c." We are perfectly content that our opponents should take this ground, whilst they can maintain it,—that they shall be "aut Cæsar, aut nullus," and we do not complain that they have no fellowship with solidism. We shall, therefore, on our part, occupy the same position; and, with this fair understanding, shall endeavour to show that humoralism has no foundation in nature.

Again, if humoralism be partially true, we can never know when solidism is right. It must lead, therefore, to the most shocking practical results, if its principles be carried out. This we shall sufficiently establish. As to the humoral specifics which were formerly attempted, their absurdity is demonstrated by the records of two thousand years. Besides, what do they amount to? Muriate of soda, magnesia, or some analogous means,—something that will make the blood red in a "porringer,"—are the only substitutes which are offered for bloodletting, calomel, antimony, blisters, and the residue of the materia medica. Those are the great agents for miasmatic fevers, scurvy, small pox, *et ea omnia genera*,—save, only, bark and wine, which are the antidotes for "putridity."

We ought to have excepted a neutral class of practitioners, who make a compromise with philosophy, and take for their rule, "in medio tutissimus ibis." They justly loathe all extremes; but take little trouble to ascertain the fact whether one doctrine or the other be fairly entitled to this appellation. They

(1) Philosophical Magazine, Sept. 1838.

see only the direct opposition which exists, without caring whether one "extreme" may not be perfectly true, whilst the other is as perfectly false. They, therefore, jog along with a mixture of both; and when, upon one part of their system they prescribe bloodletting, it is accompanied by bark and wine to accommodate the other.

Whilst other sciences are steadily advancing, it is often the destiny of medicine to go backwards. Although interrogated by the light of accumulated knowledge, we neglect the true aspect of nature, mistake her ruins for her living actions, and confound the organic with the inorganic world. The moderns have certainly gained vastly in pathological anatomy; a most important basis of pathological principles, when applied to its legitimate object. But what shall we have gained by this knowledge, in a *practical* sense, if it do not resist, or if it administer to, the chemical, the mechanical, and the humoral doctrines? In this, the only useful sense, we are carried back to a remote antiquity; and, as we shall see, without making our speculations, as our ancestors did, subordinate to the phenomena of nature. We, of course, speak only of a certain part of medical inquirers; but they have vast power, are amongst the most zealous, and carry in their train no small proportion of the silent multitude. They are busy men,—alchemysts in medicine,—who, whilst they enlighten, by their conflicts with nature, the more thinking and practical class, draw a veil over their own eyes, and become a blind guide to the blind. The philosophical inquirer no sooner demonstrates the nature of the errors, than the light is clouded by doubts, and we again find ourselves gathering the fragments of exploded systems; or, at other times, so ambitious is man of new projects, that if he possess not architectural skill, he is content with laying the finest edifices in ruins.

We perfectly agree with Dr. Wardrop, that "it has been of late too much the custom to *ridicule* the humoral pathology, (1) —"telumque imbelle sine ictu conjecint;" (2)—and with Dr. Stevens, that "the modern solidist talks with *contempt* of the contracted views of the humoral pathologist," (3)—and we therefore purpose treating the subject with greater seriousness.

(1) On Bloodletting, p. 4. (2) Virg. (3) On the Blood, &c. Preface, p. ix.

On the other hand, we may say that Dr. Stevens has imbued his book with the most unsparing ridicule of solidism. *Vide, hic et ubique;* particularly severe at pp. 170, 171, 177, 185, 190.

The Doctor thus descants upon the merit of the solidists. "Mr. Hunter's

"Laude dignos potius quam derisione et calumniis judicarem." We would confine ourselves to facts,—especially of the humoralists; and conduct our inquiry upon acknowledged principles. We would carry into science the apothegm of Pythagoras,

"ἦν σὺ κακῶς δικάσῃς σε θεὸς μετέπισα δικάσει."

We are also sensible that it is incumbent on such as oppose the host of enlightened men who are now employed in reviving the humoral pathology, that they should be ample in their facts, and strong in their physiological premises. The subject, also, derives a grave importance from the practical application of its principles which is now making its way through the most enlightened countries. A superficial examination of its merits would, therefore, be a matter of arrogance in any opponent.

In the meantime, however, that it may appear from the outset, that we are not engaged in "beating the air," we may state on high authority that the doctrine is essentially the same now as at former ages. Thus, the learned editor of the American Medical Recorder, in reply to a direct interrogatory from Dr. Caldwell: "If I rightly understand them," (humoralists) says Dr. C., "their creed embraces a belief *in* the contamination of the blood in the production of certain diseases, and *in* the medication of that fluid *in* their cure." "Yes," replies the reviewer, "this is their creed; and we are the more firmly persuaded of its correctness, since we see it so feebly assailed by one who has long fought in the front rank against its doctrines." &c.(1) We shall soon see that the foregoing is the prevailing humoral creed.

M. Andral lays down "the universal principle," that "the blood is not merely a vehicle to carry to the solids the deleterious substances; but that its usual appearance leaves no room to doubt its being altered in its nature." "That the blood is first vitiated by the commixture of deleterious substances, and that it is the vitiated blood which ultimately affects the solids."(2) And yet we see it put forth, that M. Andral is moderate in the doctrine.

The foregoing statements abound in modern works on the

book on the Blood," he says, "has done less good, or, perhaps, more evil, than any other that has been published during the last hundred years, unless we except Cullen's First Lines, or the Pathological writings of M. Broussais." (p. 157.)

(1) Review of Caldwell on Sympathy, 1822, p. 359. We state more than is necessary of the reply, that the Reviewer and his friends may have the advantage of his principal argument.

(2) Pathological Anatomy, vol. 1.

humoral pathology, in which we are told that such poisons, when administered by the stomach, as "corrosive sublimate, arsenic, colchicum, tartarized antimony, gamboge, and scammony, are, probably, *perfectly inert* until they first mix with, and *give new properties to the blood.*" Foreign agents, say the humoralists, "must change the physical properties of the blood, and give it new qualities." (1)

The inquiry, therefore, is of great practical moment; since upon it may depend the decision whether we are to treat "the worst of all fevers by table salt," or by bloodletting, cathartics, &c. Indeed we consider its importance without any parallel in medicine. It is thus briefly indicated by Dr. Stokes: "The humoral pathologist, who seeks for disease in an alteration of the fluids alone, neglects the study of visceral lesions; and when he turns his attention to the digestive system, he only considers its secretions, and not its *actual condition* or the *state of its sympathies.* His sole purpose is to evacuate sordes, or to produce a flow of healthy bile, and to eliminate depraved secretions; and this he does without possessing any knowledge of local inflammation, or of the effects of disease of the digestive system on other organs." (2)

Again, it is right that something should be stated more specifically as to the weight of opinion which is arrayed against us, and the powerful patronage under which humoralism rears its gigantic form. And as we purpose "breaking a lance" with philosophers whom we hold in great reverence, and to whom, as we sincerely think, none can be more indebted than ourselves for sound information on other subjects, it seems to us proper that we should first pay our respects to the reviewers. Not that there has been any attempt, on their part, to crowd the doctrine; but

> "The name of Cassius honours this *corruption*,
> And chastisement doth therefore hide his head."

We certainly do not intend to review the reviewers. This, we admit, would be indecorous, and beyond our province, since it is conceded that they possess a "final jurisdiction." Besides, we are single-handed, and have nothing but facts for our weapons. The contest, therefore, would be manifestly unequal; and being so, we have long since made up our minds, "that

(1) Dr. Stevens, on the Blood, &c. pp. 152, 157.
(2) Theory and Practice of Medicine, p. 18.

discretion is the better part of valour." "Obsequium amicos, veritas odium parit;" "yet would we reluctantly hazard the latter by offering our sentiments on the doctrine laid down by them, did we not consider that all enlightened minds are open to the impression of truth, and that in scientific pursuits, it is not the man, but the opinion, which is the subject of disquisition." (1)

The prospects and modern basis of humoralism are thus announced by the British and Foreign Medical Review. (2) "It seems now probable that a mitigated humoral pathology will be founded on the basis of pathological anatomy, the cultivators of which were for a considerable time determined solidists." Such, for instance, were Hunter, and Bichat (see section 13;) but were they not so from making pathological anatomy entirely subordinate to the phenomena of living nature?" We respectfully submit, however, whether pathological anatomy, in its proper sense, has yet supplied a solitary fact in favour of humoralism? On the contrary, we think, even when abstracted from the phenomena of life, that all its tendencies are the opposite way. (3)

It has been lately said by an eminent anatomist, that "organic science, commencing with the solids, concludes with vitalism; that is to say, with the Hippocratic doctrine properly comprehended; whilst, on the other hand, the school of Hippocrates, commencing in vitalism, concludes in the course of ages by associating itself with the science of organic pathology." This, we should say, is a pretty strong argument in behalf of vitalism, —which means nothing but solidism,—since the doctrine is reached by such different routes. An elegant deduction is then made by our author, that "their ultimate concurrence proves, at once, both their individual insufficiency and their reciprocal dependence." But it must be here understood that it was vitalism which directed inquiries into morbid anatomy. The latter, *per se*, could never suggest an idea that relates to life. Without an antecedent knowledge of the vital forces as derived from vital phenomena, and tracing their results, we might as well ascribe the lesions of structure to one cause as to another. Then it was, however, that morbid anatomy reflected its light upon disease, and upon the science of life.

(1) Chisholm's Essay on Pestilential Fever, vol. 2. p. 260.

(2) No. 1. 1836, p. 252.

(3) In the October No., 1838, of the foregoing Review, is an excellent article upon the blood, which confirmed us the more strongly in our own opinion.

"It is not improbable," says that stable solidist, the Medico Chirurgical Review, "that a practical basis will be laid, for the distinction of fevers dependent on the state of the blood from those where the nervous system is the primary seat of disease."(1) And again, "M. Bouillaud is too sharp-sighted not to perceive the weak points of his pet doctrine, and not to see the necessity of admitting that the fluids as well as the solids may become primarily altered in some diseases. He alludes to the phenomena of scurvy; and assuredly he could not specify one in the nosological catalogue more appropriate, or one which more unexceptionably illustrates the truth of a cautious and scientific humoral pathology."(2) But, "that the humoral pathology ought never to preside in medicine, we are convinced; that it never will again, we believe; but we have ever thought, and do still think, that the doctrines of solidism have cast it too far back into unmerited neglect."(3) &c.

We quote the following for its very just rap at pathological anatomy: "Since pathology is the spoiled child of the day, we trust that more attention shall be hereafter devoted to the alterations, whether primary or secondary, occasioned by disease within the humoral department of our system; and that a portion of the labour which is now spent, perhaps wasted, in delineating the most shadowy aberrations of the solids from healthy structure, will henceforward be given to the study of humoral pathology."(4)

This leading Review thus adverts, also, to a late work which we have not seen, and know not how far it may substantiate the conclusions of the humoralists. "In this Essay, the author embraces only the physiology of the blood,—its pathology being reserved for mature observation and experiments. It is thrown into five chapters, with a short introduction containing the rise, progress, and downfall of the humoral pathology,—on solidism, —and on the present state of the question. Like most men of sense and experience, Dr. Maitland inclines to the opinion that solidism went as far in one extreme of error, as the humoral doctrine did in the other,—much farther indeed."(5)

It will be one of our objects to exhibit the intellectual merit

(1) Vol. xv. p. 354. (2) Vol. xxx. p. 388.
We consider Dr. Johnson himself, in all respects, a solidist. See ibid. July 1, 1836. p. 148.
(3) Ibid. vol. xv. p. 337. (4) Ibid. vol. xi. p. 337. (5) Ibid. Jan. 1839. p. 69.

of humoralism, as we go along, by which it may be seen on which side the balance of "sense" preponderates. This can only be done by *facts*.

"From the results furnished by the experiments of Dr. Stevens," &c. says another able Review, "results which may be regarded as creating a new era for chemical physiology, there can be but little question, that the deficiency of saline particles, and modifications of the serum, operate in no trifling degree in inducing the pathological state of anæmia." (1)

"In a recent publication," says the able editor of the American Medical Intelligencer, "the editor has drawn attention to the influence which saccharine matter is capable of exerting in certain cases of dyscrasia, as in the consequences of syphilis, in chronic eruptions, &c. The mode in which the sugar acts is doubtless by modifying the condition of the fluid of the circulation. When the sugar is added to venous blood out of the body, it immediately communicates a florid hue to it, in the same manner as many salts which are presumed to have an alterative effect when administered internally. Some change is produced by all those alterative agencies, so that when the modified blood attains the capillaries, it induces a new action in them, and breaks in upon the pathological catenation that constitutes the cachexia. When the capillary functions are morbidly affected, as in chronic cutaneous eruptions or ulcerations, there are two great methods in which we may reach the disease. The one is by the application of external remedies to the diseased capillaries, the other is by changing the impressions made upon them by the fluid which circulates within them. Sugar, like arsenic, creosote, iodine, and other alteratives, acts in the latter way." (2)

The distinguished Mr. Slare, in his "Vindication of Sugars," endeavours to show that they are not only "vulnerary," but that "their virtue is not lost in the stomach and bowels, which produces healing effects there, and carries into the blood its self-nutritious and embalming particles." Sugars, he says, "are both food and physic." (3) Slare, therefore, was both solidist and humoralist, so far as sugar is concerned. But, can it be properly affirmed that any article of food is truly possessed of medicinal properties?

"We have also maintained," says another reviewer, "that the

(1) Baltimore Med. and Surg. Journ. and Review, No. 4, p. 352.
(2) American Medical Intelligencer, 1837, p. 22. (3) P. 3, 6, 7. 1715.

benefit arising from loss of blood in diseases of a putrescent tendency does not obtain according to the principle, that bloodletting tends to reduce inflammation; but that it is a means of depriving the system of a part of the morbific cause upon which the general derangement depends, and which acts through the medium of the blood."(1) Putrescency, in the living body, is not recognised in the philosophy of solidism.

The foregoing reviewer also remarks, that "it has always appeared to us, that the *quality* of the blood has much more to do than its *quantity* in predisposing to disease."(2) We shall endeavour to show that the last is no more a primary cause of disease, than the former.

The Cyclopædia of Practical Medicine, of whose value none entertain a higher estimate than ourselves, has done its powerful share, in at least one of its articles, towards the propagation of the humoral pathology. Thus, Dr. Tweedie: "we shall endeavour to show, in treating of the pathology of the fluids, that the blood is not only materially changed in fever, but that the diseased state precedes the attack, and that the changes take place in a determinate order." The facts which are adduced will be considered extensively.

Dr. Babington's able defence of humoralism, in the Cyclopædia of Anatomy and Physiology, is too well known to have required even this allusion.

"We have abandoned the humoral pathology very absurdly," says Dr. Armstrong; "there are more secrets in it by far than in our philosophy."(3) This last is exactly what we affirm.

"I am now satisfied," says Dr. Hosack, in his correspondence with Dr. Stevens, "that in malignant fevers, the disease can only be cured by the proper use of those medicines that possess the power of acting on the blood, and that prevent or remedy its diseased condition."(4) And yet it may be seen by Dr. Hosack's lectures that he never carried the humoral doctrine into his practice.

"It is necessary to remark," says Dr. Carswell, "that the formation and manifestation of tubercle, as a morbid product, cannot take place, unless the fluid from which it is separated,—the blood,—has been previously modified."(5)

(1) London Medical and Surgical Journal, vol. i. p. 529.
(2) Ibid. p. 205.
(3) Lectures, &c. vol. 1. p. 107.
(4) Stevens on the Blood, p.116.
(5) Morbid Anatomy, Art. Tubercle.

Morton, (1) and De Haën, (2) propounded this doctrine; in which they have been also followed by Clark, Andral, and some others. But Andral superadds inflammatory action as a probable element in the formation of tubercle. If the latter cause exist, it is philosophically sufficient till a vitiated state of the blood is more than assumed.

There are many learned speculations at present, as to the transmission of hereditary diseases by way of the blood. Amongst the most able defenders of this hypothesis is Dr. H. Holland. "We can go little further," he remarks, "than to say, that the evidence as to the agency of the solids, and the changes they severally undergo, is more distinct and complete; but we are not justified in denying that the blood may also take on morbid conditions, directly transmissible to offspring." But we are justified in saying that there is not one well ascertained fact to warrant the conclusion; nor has it been attempted to offer one of the nature which we are now supposing. Indeed, it is conceded by Dr. Holland, that "this is a question involving much curious but doubtful speculation." Shall we not rather, then, adhere to our facts where they are "distinct and complete," than embark upon an unmitigated hypothesis? But, again, says Dr. Holland: "The accumulation of this matter of the disease (gout) may be presumed to be in the blood; and its retrocession or change of place, when occurring, to be effected through the same medium." Why are "females so exempt from this disease," if "we can proceed little further than to affirm, that certain kinds of diet do produce distinct changes in the blood, and in the secretions thence derived"? Or, why should "it be so much limited to the wine-drinking classes,"—always excepting wine-drinking females? But it is also "hereditary;" and why have not females their share of the sanguineous impurity? Dr. H., however, considers the disease almost entirely in a humoral sense, and thinks that *colchicum* can only act in three ways; either by "destroying the matter of gout by some specific change;" or by "withdrawing it from the part affected into the general circulation;" or by "procuring its removal from the system, through some of the excretory organs." "Each of these suppositions may be possible; and collectively they seem to include *all the modes* in which the medicine *can* act." Not a word of its possible action upon the

(1) Phthisologia, seu Exercit. de Phthisi, 1689. (2) Rat. Medendi, t. I. p. 60.

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vital properties of the solids. In cholera, Dr. H. says, that "the changes in the properties of the blood" are "the basis of all the other symptoms." (1)

Dr. Latham, after remarking that Dr. Stevens had discovered that the blood, in yellow fever, "loses a proportion of its salts, and that these facts led his mind to the employment of a *new* practice," observes, ("nihil magis lectorem invitat quam inopinatum argumentum," (2) observes, that "here chemical experiment and clinical observation, leading, as it were, each other by the hand, proceed together, and arrive at the seminal principle of disease. Passing by this organ and that, and this function and that secretion, they penetrate to the spring and source of all, even to the blood itself, and *there* they find it, and apply a remedy which is able to reach it *there*. Are we upon the verge of a great pathological discovery? Dare we presage that the worst, and hitherto most fatal symptoms of all fevers, will soon be shown to have one origin? That a pravity, or deficiency in the constituents of the blood is the cause, that this is demonstrable, and that this is remediable by the simplest means which are always at hand." (3) This means is common table salt.

"The researches of Dr. Stevens," says Mr. Jennings, "respecting the state of the blood in yellow fever have *proved* that, in this affection, it is almost deprived of its saline constituents." "Whether or not it be the *first change induced by the morbific poison*, and a *cause* of the subsequent diseased action which we call fever, future observations must determine." (4) How does the poison remove the saline matter from the blood?

"For my part," says M. Magendie, "I declare boldly that I look upon these ideas about vitality, and the rest of it, as nothing more than a cloak for ignorance and laziness." "All the physician can do is to order certain remedies, which, if necessary, the *nurse could prescribe equally well.*" "You saw me," he continues, "give rise, *at my pleasure*, to pneumonia, *scurvy*, *yellow fever*, typhoid fever, &c. not to mention a number of other affections which, so to speak, I called into being before you." (5) "The nurse," of course, cured them.

(1) Medical Notes and Reflections, pp. 15, 116, 122, 125, 126, 127, 135, 374, 524, 585. (2) Scaliger, ad Patisonem.

(3) Lectures on Subjects connected with Clin. Med. p. 53.

(4) On the Chemistry of the Blood, in Transactions of the Med. and Surg. Association, vol. 3. 1835. p. 60.

(5) In Med. Chir. Rev. Lon. Jan. 7, 1839, pp. 204, 207.

Such is the unavoidable result of the humoral pathology. "The nurse can as well prescribe," "table-salt for the worst forms of fever," as the best of us. No wonder, therefore, that we hear the humoralist (1) exclaim, "like me, gentlemen, you have no doubt been struck with the trifling good that study confers on society. But could it be otherwise when there is scarcely a sound idea on physiology abroad?" And yet our author talks of the "laziness" of the vitalists. "*Jupiter ambrosia satur est, nunc nectare vivit.*"(2) Still we grant it right, that "study" would be as little in harmony with the doctrines of humoralism as we have now seen avowed, and as we shall portray them hereafter, as the ermine of royalty with the rags of a beggar. We are glad, however, that our author has administered this dose. We have no doubt of its *purifying* qualities. His reviewer exclaims, "the BLOOD! the BLOOD! (as our friend Dr. Wilson would say) will, in future make the grand distinctions between doctors and old women."(3) We do not, however, believe it. Europe has not yet lost its spirit; and our able reviewer has nobly spoken. As for America, she is but coming into being; powerless as yet, but pregnant with mighty achievements. The imbecilities of declining society will find no quarter in a rising empire. They will receive their strangle the moment they approach the region which is destined to preserve and to advance the knowledge and the accumulated wisdom of ages. It is the language of all history which we speak. Here it is that the rich lore of the older and declining countries will be cherished and improved. Here it is, that "study" will progressively receive new impulses, and science and arts stretch forward, till the indolence of age shall expel them to another rising empire. That empire may be Europe; perchance it may be again the lot of Arabia, perhaps of Africa. (4) Of Magendie, and his "experimental fallacies," we must occasionally speak, — since with many he is *primus inter pares.*

The foregoing contributions from Stevens, Latham, and Magendie, as well as others in anticipation, reminds us of Girtanner's serious prediction in relation to his discoveries in the laws of irritability, — being the antipodes of the former in respect to principle. "The irritable fibre," he says, "being the same in

(1) Magendie, ut supra. (2) Martial de Jove.

(3) Med. Chir. Rev. Jan. 1839, p. 208.

(4) See last APPENDIX in vol. 2, on the ADVANTAGES OF STUDY.

all organized nature, diseases and their remedies will, of course, be the same for all organized beings. There will be no distinction between *medicine, farriery*, and *agriculture;* but all these sciences will be *confounded*, and become one, under the general name of *universal philosophy*. The art of pharmacy, and the science of prescription-writing will become useless; a phial of alcohol and laudanum will supply the place of that enormous quantity of drugs which crowd the shops of the apothecaries." (1)

Now look on this picture, and then on that; — which of the two is most prepossessing?

When humoralism was dandled as a toy, — when it was but the "king's fool," — the great masters were wont to castigate it after the following manner: "There cannot, in the nature of things," says Hillary, "possibly be any one medicine, how useful and *efficacious* soever it may be in some particular cases, that can be proper in all cases, and much less in all times of any fever, no more than one coat can be made to fit all men; and the one is as absurd and impossible as the other; and if any one does pretend they can, they must either want judgement, or honesty to acknowledge the contrary. I have no prejudice against any particular persons, nor any self-interested views, but the good of my country and of mankind." (2)

"It may be remarked," says Dr. Black, "that, to whatever science or object physicians particularly directed their attention, this they generally interwove in their theory of diseases. The anatomist pretended, that by dissecting the smallest minute fibres, (3) he should reach the cause and recess of disease, and learn the method of cure." (4) This fantasy is again in opera-

(1) On the Laws of Irritability.

(2) Hillary's Inquiry into the Method of improving Medical Knowledge, p. 333. Also, on Fevers of the West Indies. The learned Hillary was one of the most thorough humoralists, in fancy, that ever flourished. Standing, however, in a practical sense, upon the broad basis of nature, he was an enlightened defender of the best principles in medicine, and violated his hypothesis in every prescription. He has many parallels at the present day; and we have stated this fact for the purpose of showing *a priori* that humoralism is an offspring of the imagination. As to the *saline treatment* of yellow fever, he utterly denounces it. He also repeatedly condemns, in his Inquiry, the "chemical treatment" of diseases.

Although Hoffmann considers different salts in the blood as the cause of scurvy, he ridicules the attempt to correct them by chemical agents. (a) It is well known that Sydenham exploded the humoral treatment of small pox, measles, fevers, &c.

(3) See our APPENDIX on the MICROSCROPE, vol. 2.

(4) Historical Sketch of Med. and Surg. p. 299.

(a) Med. Ration. t. 4. c. 1. p. 5.

tion. "It is likewise," says another eminent philosopher, "much easier to establish three principles to determine the nature of diseases, as the ancient methodists did, and to oppose three formulæ to these diseases, or to reject all rules whatever, as the empyrics do, than it is to *dive deeply into the healing art.*" "The physicians in Chili blow around the beds of their patients, to drive away diseases. The people there think that physic consists wholly in the wind, and their doctors would take it very ill of any body who should attempt to make the method of cure more difficult. They think they know enough, when they know how to blow."(1) This, as we have seen, is Magendie's doctrine. And is not Abernethy's predilection for the blue pill, which Dr. Latham reprobates,(2) founded on better principles, than "the treatment of the worst forms of fever by table-salt"?

If, however, it be really true that we are on the verge of the "great discovery" which humoralism every where predicts with confidence, shall we not sooner attain "the consummation so devoutly to be wished" by taking, at once, the decisive step of Paracelsus, who publicly burnt, from his professor's chair, all the valuable records of medicine within his reach? We, of course, except from the general conflagration the disciples of the humoral school. We need not say that Paracelsus had his "Elixir of Life," or that all the iatro-chemists had a particular antidote for disease.

But, suppose that the foregoing articles of food, table-salt, and table-sugar, so liberally consumed at every meal, produce a *florid hue* of the blood when "mixed together in a porringer," does it equally follow that a similar effect will take place *within* the body? On the contrary, it has not yet been shown, (as will subsequently appear,) that sugar is ever taken into the circulation. But admitting all that is required, and that the conversion of the blood, out of the body, to a florid hue constitutes the philosophical basis of practice; what shall be said of the salu-

(1) Zimmerman on Experience in Physick, vol. 1. p. 19.

(2) Op. Cit. p. 42. "Practically," says Dr. L. "the sum of all is this: that be the local disease what it may, the constitutional ailment what it may, and the derangement of the stomach and bowels what it may, this one method of treatment is at all times applicable.

"What a tempting theory! and what a still more tempting practice. As soberly set down in print, the student can hardly help receiving them; for, being once faithfully received, what a world of tedious study and observation must they save him." Here we have a proper antidote to the commendation of the "saline treatment."

tary acids and other efficient remedies that give a *black* hue to the blood, in diseases to which salt and sugar are said to be the best adapted?

SECTION II.

It appears, then, to us, that conclusions have been adopted in favour of the humoral pathology, without a proper inquiry into the nature of the supposed facts. It is affirmed that pathological anatomy has even supplied a basis for what was but the dream of our ancestors. The putrescency which arises after death is the only fact of which we have any knowledge, that can have been assumed as a foundation for the doctrine. We know not how far this may have prevailed; but sure we are that pathological anatomy, in its proper sense, cannot be shown to supply the least encouragement.

As to chemistry, we shall not only show in this essay, that it has utterly failed of contributing any support to the humoral hypothesis; but shall endeavour to prove in other places, that it has no relation to the science of life. The only ostensible ground to the contrary consists in assumptions, or the misapplication of facts; and what is extremely remarkable, the most important general principles relating both to solidism and humoralism have been founded by the same philosopher upon exactly the same facts.

One of the principal evidences offered by Dr. Babington of the probability that disease may depend, primarily, upon morbid conditions of the blood, is derived from the speculations of Dr. Stevens; and we have already seen that, in the opinion of Dr. Latham, they are likely to subvert the whole science of medicine. Indeed, the assumptions and inductions of Dr. Stevens are more constantly employed, than any other, in behalf of the doctrine, both in Great Britain and on the Continent; "Lectorem delectando simul atque monendo."(1) Being, therefore, an oracle that the humoral pathologists now swear by, we shall take our author's work as one of our guides, "Κατ' ἐξοχην," through the labyrinth of humoralism. "Ex uno disce omnes."

(1) Horace.

"Quis strepitus circa comitum! quantum instar in ipso est!
Sed nox atra caput tristi circumvolat umbra." (1)

On no other principle, however, should we feel obliged to answer the statements and assumptions of this writer; nor indeed would any reply be otherwise defensible. Our remarks, therefore, have a far greater reference than to the abstract merits of our author's treatise. Nor shall we neglect an examination of all the proofs which have been advanced by the best modern authors in favour of the humoral hypothesis.

The work of Dr. Stevens having been especially instrumental in propagating the humoral doctrine, and much of its importance reposing on his saline treatment of fever, we shall state, at the outset, that this gentleman first bled and gave calomel or castor-oil, but attributed the whole credit of cure to saline draughts that were employed towards the decline of the disease. (2) We have our author's own authority for this statement, as well as of those with whom he was more or less associated. (3) Thus, "in 1828, Joseph Kenyon, attacked with fever, was bled to the extent of about *fifty* ounces, which produced instant relief. In half an hour he took a *croton* pill, which operated freely. A sponge dipped frequently in *cold water* was constantly used. As the excitement continued he was again *twice bled* during the day; and on the following morning, when I had nothing to fear from *increased action*, he was put under the *saline* treatment." (4) But, owing to the officiousness of a garrulous old landlady, he got a quantity of vinegar and water, which upset the case, and again darkened the blood. *Hinc illæ lachrymæ.* (5)

Again, in contrasting the success of his own practice in an epidemic fever at St. Thomas, where "forty-nine out of fifty patients recovered," with that of "a physician who lived nearly opposite, and who lost three out of four," he says that "those who were well bled and properly evacuated *in the beginning*,

(1) Virg. Æn. l. 6. l. 865.

(2) When De Haen extolled the virtues of Vervain, a sacred amulet of the ancients, he was willing to think that he might have overlooked the effects of other remedies which he employed in conjunction with it. He therefore says, "nunquam enim in similibus tentandis, evulgandisque, sufficienter cauti esse possimus. Cœvis scribimus, et posteris. Hi, institutis iisdem experimentis, aut eadem, quæ nos, aut aliter, quam nos, aut nihil omnino, quod nos, videbunt, &c." (*a*)

(3) We had this information, also, personally from physicians, when we were at St. Croix, and St. Thomas, in 1836–37.

(4) On the Blood, &c. p. 339. (5) P. 342.

(*a*) Rat. Med. t. 2. p. 225. (De Medicam. vir.)

and *then* put under the saline treatment, almost invariably recovered." (1)

So much for practical evidence, and the collision of specific statements with general affirmations. The "saline treatment" had evidently no part or lot in the therapeutical process. The disease was under the control of nature before this hypothetical expedient was called in to the subversion of philosophy, and afterwards to be drummed up, merciless of the human race, as a sovereign cure for "the worst forms of fever." Hillary, Robert Jackson, Moseley, Bancroft, and others, practising in the same climate, slew the same fevers by the lancet alone. The whole history of medicine assures us that art has done its ablest work, in these "putrid diseases," when it has depleted freely by the lancet. And what has all this to do with the humoral pathology?

We are now ready to go on with Dr. Babington's statement. "In those fevers which arise from marsh-miasmata or from contagion," says this able writer, "it is an opinion held by Dr. Stevens, and supported at great length in his work on the blood, that a diseased condition of that fluid is the *first* in the train of symptoms which occur, and the *immediate* cause of those which follow. The blood itself, says Dr. Stevens, is both black and diseased even before the attack. (2) During the cold stage it is very dark. When first drawn it has a peculiar smell, and coagulates almost invariably without any crust. There are black spots on the surface of the crassamentum; the coagulum is so soft that it can be easily separated by the fingers, and during its formation a large quantity of the black colouring matter falls to the bottom of the cup. In the hot stage it becomes more red and in some cases it is even florid for a time, but during the remission it is darker in colour than the blood of health, and decidedly diseased in all its properties." (3)

(1) On the Blood, p. 350.

(2) Again, he says, "every drop of the blood is deranged even before the attack. (*a*) So windmills became giants to the imagination of the hero of Cervantes. "The desire of seeing a thing," says Zimmerman, "occasions us to see it every where. I know several physicians, who see only certain diseases. One of these, who is a celebrated practitioner, and who has an obstructed liver, fancies he discovers a similar complaint in all his patients, and it is only one particular remedy that he constantly employs,"—and so on. "A Stahlian thinks only of his soul, and his hemorrhoids; like a fond lover, who sees only his mistress." (*b*)

(3) Babington on Morbid Conditions of the Blood, p. 18.

(*a*) P. 17. (*b*) On Experience in Med. vol. 1.

Now, to our mind the whole of this statement may be philosophically, and must, through the force of irresistible facts, be converted to the interests of solidism. It embraces, however, some assumptions which it is not even attempted to sustain. Must it be passively conceded that "the diseased condition of the blood is the first in the train of symptoms;" that there is no morbid state of the solids till the invasion of the cold stage of fever, or even until the colour of the blood has departed from its normal aspect? Is there no change in the vital forces and actions of any of the great viscera that are concerned in the process of sanguification during the long period of incubation? Can we not learn, as we shall endeavour to show, that in most cases of miasmatic fever there had been many symptoms attending the solids which denoted a gradual formation of disease before the great explosion had taken place? (1)

Another assumption consists in the affirmation that the blood becomes "decidedly diseased in all its properties." We think it likely, on the ground of solidism, that this may be true; but it has never been demonstrated. The most that has been shown are the sensible variations in respect to the component parts of the blood, its specific gravity, colour, and coagulability. But these are fallacious signs. They supervene on slight disturbances of the sanguiferous organs; such as amount not to actual disease. They are forever varying at every hour of disease. They arise spontaneously, or may be artificially induced by a great variety of remedial or morbific agents, which supply a demonstrative proof, that they depend upon varying conditions of the solids. This analogy is irresistible, and we carry it back, in virtue of the soundest philosophy, to the earliest period of disease. We see that this and that agent suddenly modify the morbid action, as denoted by changes in the vital phenomena, and by agents, too, whose direct action upon the blood it would be absurd to imagine. We see this in all fevers, and under all circumstances of fever, from the simple ephemera to the most genuine specimen of "putridity." Without, then, taking another step, if there be any thing in medical philosophy that is worth respect, it is a sound, and the only sound, conclusion, that the primary causes had exerted their original effect upon the vital

(1) See an excellent account of the precursory symptoms of tropical fever by Annesley, in his "Diseases of India," vol. 1; and farther on, in this Essay, by Dr. Stevens.

forces of the solids, and that all subsequent changes in the blood are the consequences of alterations thus induced in the vital actions.

The most astonishing changes in the sensible condition of the blood may take place, through the action of the solids, almost instantaneously. We need not speak of the familiar changes in the coagulability of the blood during venesection; but it is less generally considered that, "there is a very considerable difference to be sometimes observed in the quantity of coagulable lymph in blood taken in different cups from the same patient at the same bleeding; and that in some instances this difference has been observed nearly *one half*." (1) Scudamore sometimes found the proportion of lymph diminished more than one half; and so constantly did this phenomenon attend his experiments, that he ascribes the diminution of the inflammatory buff, during the progress of the same bleeding, to the decrease of fibrin. (2) "The serum," says Thackrah, "is relatively increased during the continuance of bleeding; and it is surprising how great a change in this respect the lapse of a minute produces." (3) Gendrin, Stokes, and other accurate observers, make the same affirmation.

No one, we think, will contend that the foregoing effect is directly upon the blood, simply in virtue of its diminished volume; or, that there is no other intelligible mode of explaining the phenomenon but by referring it to a very sudden change of action in the sanguiferous organs, — perhaps in that series by which the fibrin is deposited in the molecular structure.

Again, the foregoing phenomenon is directly reversed, and an increased quantity of fibrin will be found in each successive cup. This shows that diminution of volume, *per se*, has no connection with the phenomenon; whilst it is another evidence not only of its dependence upon a modified state of the solids, but that those solids are animated by forces peculiar to themselves. Whoever denies the primary agency of the latter commits intellectual suicide. The very changes of which we have now spoken, as also the colour, and other characters of the blood, will depend in a measure upon the rapidity with which the blood is abstracted. (4) These various changes, says Musgrave,

(1) Wardrop, on Bloodletting, p. 14. (2) Essay on the Blood, pp. 119, 151.

(3) On the Nature and Properties of the Blood, p. 97.

(4) Scudamore on the Blood, p. 94, &c. We all know the facts; but it is important to state them. "These results," says S. "show that we ought not to estimate

"require the agency of some third power; for to suppose that the blood undergoes so sudden a change merely by the quantity being lessened, would hardly be less extraordinary, than to imagine that pouring a glass of brandy out of a bottle would turn the rest into cider." (1)

Again, in a case of inflammatory fever, Hewson says the buffy coat appeared only on the fourth cup. (2) Whether this be owing, as supposed by Hewson, to an increased fluidity of the fibrin, or to its greater proportion, as surmised by Scudamore, and others, we must look to the sanguiferous vessels as the instruments by which the change is produced.

"The blood, which passes from the vessels when a sheep is dying, is almost instantaneously solid. Blood, drawn from a person about to faint, coagulates very rapidly." (3)

We have stated many analogous facts in our essay on bloodletting, to show the direct influence of this remedy upon the vital forces. Amongst the remarkable changes which occur in the appearance of the blood, in inflammations, during its abstraction, the buffy coat does not form after the approach of syncope.

Mayer, Dupuytren, Dupuy, Gendrin, and others, have variously demonstrated by experiments, that the nervous and vascular systems may produce rapid and important changes in the constituents of the blood. That most able and practical man, Dr. Mead, who was a thorough humoralist in a speculative view, states that "the blood may certainly undergo any imaginable changes by alterations made in its motions only." (4) And in our own times, it is conceded by a leading humoralist, the able Andral, that "no one solid can undergo the *slightest modification* without producing some derangement in the nature and quantity of the materials destined to form the blood, or to be separated from it." (5) We might illustrate the principle, on which the foregoing phenomena depend, by examples without

the character of the blood merely from its *appearance*, without taking other circumstances into consideration, and especially the *kind of stream* in which the respective portion flows from the orifice." (p. 95.) Zimmerman states, that he observed the inflammatory crust only when the orifice was large. (On Experience in Physick, vol. 1, p. 306.)

(1) On the Qualities of the Nerves, p. 67.

(2) Thackrah speaks of a similar instance. (Op. Cit. p. 115.) See, also, some excellent remarks by Dr. Babington, in Med. Chir. Trans. vol. 16, p. 11.

(3) Scudamore, Op. Cit. p. 40. Also, Wilson on the Blood, p. 53. Thackrah Op. Cit. p. 48.

(4) Introduction to Essay on Poisons, p. 27. (5) Patholog. Anat. vol. 1.

end, derived from morbid states of the body; and this, too, where the blood is supposed to be mainly in fault. They abound amongst the humoralists, although they do not awaken their attention. Thus, in a very malignant epidemic at Minorca, Cleghorn says, "the blood frequently changed its appearance in the space of a few hours in the same person; what was drawn in the morning having a crust; that in the evening, none, and *vice versa*. I could never positively determine *which sort* afforded the *best* or the *worst* prognostic." (1)

Again, the coagulability of the blood is destroyed in animals that have been run to death. The same phenomenon is common in apoplexy, when death is sudden; or when suddenly produced by organic affections of the heart, or by blows upon the stomach, or by violent passions. Whatever cause, indeed, operates with the same suddenness and intensity upon the nervous system, and through that upon the vessels which give being and vitality to the blood, will divest it of its coagulating property. It is remarkable, too, that in all these cases, according to Mr. Hunter, and others, "the blood soon putrifies," much sooner than in death from fevers, &c. (2) This shows how possible it is for sudden and violent impressions upon the nervous and vascular systems to alter more profoundly the constitution of the blood than malignant diseases. And yet it is assumed with great confidence, that when the blood remains fluid after death from fever, it is an unerring proof of its primary vitiation by morbific causes.

That the alteration of the blood, in the foregoing instances, is owing to a change of condition in the solids is also manifest from the flesh of an over-driven ox, or the poultry that are flogged to death for the epicure, being more tender, and putrifying sooner, than when they are bled to death. The same effects are produced by violent poisons acting upon the stomach. "In the West Indies, they kill their poultry with vegetable poisons, in order to render them tender without keeping." (3) A remarkable anatomical fact which goes to the same conclusion, as to the controlling power of the sanguiferous organs over the blood, is

(1) On the Epidemical Diseases of Minorca, c. 6, p. 157.

(2) Mr. Thackrah (a) supposes that the blood remains fluid after sudden death, in consequence of its vitality not being extinguished.

(3) Beddoes' Additional Observations on the Laws of Irritability.

(a) Inquiry, &c. Ut Cit.

frequently stated by M. Louis, viz.: that "the blood contained in the cavities of the heart had various characters, according to the condition of the organ." It was most coagulable *in all diseases* where the heart was "most natural," and least so where the "softening was most considerable."(1) We desire that this fact may be duly regarded, when we come to the consideration of scurvy; a disease in which the foregoing condition of the heart is not uncommon.

Passions, and other analogous causes, which can only operate upon the solids, may speedily develope violent diseases. We need not speak, specifically, of the long catalogue of infirmities which follow abrupt, or more subdued, but prolonged, emotions of the mind; and how remarkably peculiar are some of these diseases to particular passions;—how anger rouses the circulation, whilst it is depressed by fear; how the former excites inflammations, whilst grief prostrates the action of the same vessels, and undermines the whole body. Here,

> ——— "agitur furis, totoque ardentis ab ore
> Scintillæ absistunt: oculis micat acribus ignis."
> *Virgil.*
>
> "Ora tument ira, nigrescunt sanguine venæ;
> Lumina gorgonio sævius angue micant."
> *Ovid.*

There,

> ——— "Sociis subita gelidus formidine sanguis
> Diriguit; cecidere animi ———."
> *Virgil.*

Or, in the jealousy of a lover,

> "In dewy drops my limbs were chilled,
> My blood with gentle horrors thrilled,
> My feeble pulse forgot to play,
> I fainted, sunk, and died away."
> *Sappho.*

Or, as Collins sums up the whole.

"This fulmen perturbationum, (as Piccolomineus calls it,) this thunder and lightning of perturbation causeth violent and speedy alterations in this our microcosm, and many times subverts the good estate and temperature of it." It is remarkable, too, how the emotion and its consequences may be delayed by reason, after the remote cause has been applied and withdrawn. Thus, it is related by Riccius,(2) the traveller, that, if the Chi-

(1) On Typhoid Fever, vol. 1. p. 285. (2) Expedit. in Sinas. l. 1. c. 9.

nese be told that they will sicken on a certain day, on that very day they will surely be sick, and sometimes die in a few hours. Similar facts are recorded by other observers.

Charles the Sixth, of France, became a lunatic for life, from the mere effects of passion. If humoralism be good in the temporary cases, it must be equally so in such as this. It is well known that fear has suddenly changed the black colour of hair to gray; and it is related by Hodgkin, that Rostan states an instance where the skin of a female, who had been condemned to the guillotin, became black in consequence of the shock. The colour was formed by the "deposit of pigment in that part of the tissue in which its presence is natural in the dark races of mankind." (1)

Several instances are on record, where a disease analogous to hydrophobia, having all its characteristic symptoms and fatal consequences, has been induced by emotions of the mind, fatigue, drinking cold water, and mechanical injuries. Velpeau believes that the bite of a dog not rabid will produce hydrophobia, and advises that every such wound should be cauterized. He has seen the disease arise from a fright. (2) We do not believe the affection to be the same as produced by a rabid animal; but the symptoms are violent and there is dread of water. They are, at least, strongly indicative of the independence of hydrophobia, from the bite of a mad-dog, of any primary lesion of the blood. Moreover, the disease occurs spontaneously in the canine and feline tribes.

Here is no contaminating matter, no primary vitiation of the blood. And yet, from such causes, this fluid shall undergo as remarkable changes as occur in ordinary fevers, or smallpox, or gout, or other affections of reputedly humoral origin. They supply an interpretation of the modifying causes where the blood may undergo changes in the latter diseases.

Our able Caldwell inquires whether any thing passes from the orator, the musician, the poet, the warrior, &c., into the blood of the multitude, when they rouse the heart into tumul-

(1) Lectures on the Morbid Anatomy of Serous and Mucous Membranes, vol. i. p. 306.

(2) See Velpeau in La Lancette Française. Juil. 1835. Morgagni's Epis. ar. 31, 32. Journ. de Méd. t. 7. p. 3, &c. and t. 8. p. 81. Ephemerides N. Cæs. An. 6. Ob. 9. An. 9. Appen. p. 249. Plater, Obs. Med. l. 1. Sauvage's Nosol., Rush's Med. Inquiries; on Tetanus, &c. Percival's Med. and Exper. Essays. vol. 2. Celsus, l. 5. c. 27. p. 307. Ettmuller Instit. Med. Thes. 11. p. 15. Hildan. cent. 2. Ob. 97. cent. 4, 98, 47, &c. Purman's Chir. Curios. l. 3. c. 3.

tuous action. "As well might we say that thought itself is of humoral origin." (1)

And yet, in all the foregoing cases, the humoralists will have it, that when the mind is moved by passion, "the pure spirits forthwith flock from the brain to the heart, by certain secret channels, and signify what good or bad object was presented; which immediately bends itself to prosecute or avoid it, and, withal, draweth with it other humors to help it. So, in pleasure, concur great store of purer spirits; in sadness, much melancholy blood; in ire, choler. As the humors in the body be likewise prepared, and the temperature of itself be ill or well disposed, the passions are longer and stronger; so that the first step and fountain of all our grievances in this kind is *læsio sanguinis*, which causeth all these distemperatures, alteration and confusion of spirits and humors; by means of which, concoction is hindered, and the principal parts are much debilitated."

We shall not stop to comment upon this philosophy. We give it as it stands. How far "pathological anatomy is its basis," we offer no conjecture; but shall go on with what appears to us to be farther indubitable proofs against all such gibberish.

"Sicuti plures morbi a parva causa, quandoque invisibili, et in corpus non introducta, vel non subsistente dependent; ita pariter *plures magni morbi momento temporis curantur*, et non per evidentem aliquam evacuationem." (2) Thus, "fear is frequently an occasion of typhus fever in Rome," and "on this principle, charms and spells operate in curing ague." "The doctrine of predestinarianism, which prevails among the modern Turks, is a powerful preventive against the plague;" "and it is owing to courage, rather than camphor, that medical men seldom have typhus fever." (3) Do courage and predestinarianism prevent a contamination of the blood, by which this humoral fever and the plague are counteracted? And yet, with these facts before the author whom we quote, he defended the humoral pathology. Again, says the same writer, "when typhoid patients are apparently dying, they will often recover from that state by the stimulus of pleasant impressions on the mind." Valesius shows, by strong examples, that diseases may be almost

(1) Phila. Journ. Med. and Phys. Sciences, 1822. p. 304.
(2) Baglivi. Op. cit. l. 2. c. 10.
(3) Armstrong's Lectures on Acute and Chronic Diseases, vol. 2. p. 111.

instantly cured by sudden affections of the mind,—"non nunquam etiam morbi diuturni consequuntur, quandoque curantur."(1)

The philosopher Boyle, who employed himself about physiology, also insists upon the humoral hypothesis in the very face of similar facts, and accounts for them upon this dogma. We quote him for the sake of the facts, and to show how a great mind will reason upon this subject. True, Mr. Boyle had not our modern knowledge of physiology; but we shall see it makes no difference. "That a disease," he says, "may, sometimes, seem to be removed, whilst the peccant matter is lodged in the body, appears from the cure of agues by a *sudden fright*, wherein no discernible *evacuation* is made. This seems confirmed by many instances already produced."(2) He then relates an instance where a *quartan* was cured by a fright from a rat; and, again, where the *gout*, and an obstinate case of *scarlatina*, &c., were suddenly removed by a paroxysm of fear. In two other cases, whitlows were removed by putting the finger into a cat's ear "within a quarter of an hour;" and he says that Riverius, who relates the cases, "thought that the cats attracted the morbific matter of the patients." And so, of animals. "I have seen," says Boyle, "a coach horse ready to drop down dead, of the staggers, on the road; but, upon well rubbing his gums with a coach whip, till they bled, he was instantly relieved, and enabled to perform his journey."(3) Are not infants often liable to fearful convulsions from dentition, and do we not relieve them, at once, by lancing the gums? Is a diseased state of the blood the cause of the foregoing phenomena? "Dr. Frank, senior, describes a quartan, which resisted all possible drugs for several months. The patient was cutting a *dens sapientiæ*. The gum being lanced, the ague was seen no more."(4)

"Intermittents," says Senac, "when on the decline, are extinguished, at times, by a single blow, as it were; nor can we discover how the causes that thus destroy them possess such an influence over them. They are removed by great and sudden emotions of mind or body, by intense heat, violent exercise," &c.(5) Again, on the other hand, they are excited by the same causes. They may be removed by remedies whose chemical

(1) Controvers. Med. et Philos. l. 5. cont. 6.
(2) Philosophical Works, vol. i. p. 82. (3) P. 86, &c.
(4) Beddoes' Resh. on Fever and Inflam. p. 82. (5) On Fevers, pp. 58, 161.

properties are opposite to each other. "Indeed, there is scarcely anything, whether noxious or innocent, that has not cured intermittent fevers."(1) And yet these are favourites with the humoral pathologist. In our examination of the scurvy, many analogies will be stated. There, for instance, it will appear that this disease, in its very worst conditions, is often immediately relieved by the influence of air alone, and by other causes which can have no conceivable effect upon the blood, but through the agency of the solids.

We have seen it affirmed, according to our quotations, that humoralism is essentially the same doctrine now as ever; but more to this effect hereafter. But we have never seen the philosophy of the *modus operandi* of the foregoing causes, in the cure or production of intermittents, in any respect explained upon humoral principles. The older writers avoided it, excepting as they tell us that the morbific matter is expelled by the paroxysm, and that the intermission follows in consequence.(2) But then, what is the cause of the next, and the next paroxysms,—each increasing in severity? Solidists and humoralists have had their theories; the latter neglecting the original suppositious cause after the first paroxysm. Cullen(3) mystified the periodical return by ascribing it to the general influence of diurnal revolution, and his hypothesis has been again brought forward by M. Bailly;(4) Darwin,(5) by an accumulation of sensorial power; Hildebrand,(6) Mead,(7) Moseley,(8) and others, by ascribing it to solar or lunar influence. Θυσώσιν ἡλιω, και σέληνῃ, και γῇ, και πυρόι, και ὕδατι, και ανεμοι. (9)

Here we may take notice, also, of another specimen of the humoral philosophy, as it affects true genius. Sydenham, Boerhaave, and Van Sweiten, direct us to delay the administration of bark in intermittents, till the paroxysm has passed off. But the reason was, that it is important to afford nature an opportunity of completing her fermenting and concocting process. What is the present doctrine? As it respects those true observers of nature, whom we have just mentioned, is it not probable that they

(1) Ibid. On Fevers, p. 160. (2) Willis de Febribus, c. 3. and others.
(3) First Lines, &c., sec. 55, 56, 109.
(4) Bailly Trait. Anatom. Patholog. des Fièv. Inter. 1825.
(5) Zoonomia, Supplement, 7, 8. p. 556—575.
(6) Instit. Med. Prac. t. 2. p. 105.
(7) Works, Influence of Sun and Moon, p. 191, &c.
(8) On Tropical Fevers, Of the Moon, &c., p. 569, &c. (9) Herodotus.

founded their practice upon the vital phenomena as presented by the solids, and afterwards amused themselves with the theory? If this be the present aim of philosophy, it may be innocent enough with the projectors; but are they not leaders of others?

Going back to the influences of the mind in the cure of diseases, the vitalists have no difficulty in solving the problem upon their own views of physiology. Upon the same ground they allow that homœopathy, and animal magnetism, may sometimes produce astonishing effects. The most intelligent of the humoralists formerly explained this matter partly in our way; but there was always an intermingling of the hypothesis. We quote from an author who appears to have read every thing. "All the world knows," he says, "there is no virtue in charms; but a strong conceit and opinion alone, which forceth the humors, spirits, and blood, which takes away the cause of the malady from the parts affected. The like we may say of our magical effects, superstitious cures, and such as are done by mountebanks, and wizards. An empyric oftentimes, and a silly chirurgeon, doeth more strange cures than a rational physician. Nymmannus gives a reason; — because the patient puts his confidence in him, which Avicenna prefers before art, and all remedies whatsoever. 'Tis opinion alone, saith Cardan, that makes or mars physicians; and he doeth the best cures, according to Hippocrates, in whom most trust. So diversely doth this phantasie of ours affect, turn, and wind, so imperiously command our bodies, which, as another Proteus, or a chameleon, can take all shapes, and is of such force, as Facius adds, that it can work upon others as well as ourselves. How can otherwise blear-eyes in one man cause the like affection in another? Why does one man's yawning make another yawn? One man's —— provoke a second many times to p? Why doth scraping of trenchers offend a third, or hacking of files? Why do witches and old women fascinate and bewitch children, but, as Wierus, Paracelsus, Cardan, Mizaldus, Valleriola, Vannius, Campanella, and many philosophers think, the forcible imagination of the one party moves and alters the spirits of the other. Nay more, they can cause and cure not only diseases, maladies, and several infirmities, by this means, as Avicenna supposeth, in parties remote, but move bodies from their places, cause thunder, lightning, tempests; which opinion Alkiadus, Paracelsus, and some others approve of: so that, I may certainly conclude, this strong conceit or imagination is

astrum hominis, and the rudder of this our ship, which reason should steer, but, overborne by phantasie, cannot manage, and so suffers itself and this whole vessel of ours to be over-ruled, and often over-turned."

We have thus, in the foregoing variety of examples, suited to all classes, not only ample evidence of the controlling power of the solids over the blood; but that it is through the former alone that changes are established in the latter; that impressions even upon the nervous system may be so determined upon the capillary vessels, that the character of the blood, and as we shall see, that of the secretions, may be essentially changed in a moment of time, either for the better or for the worse. In respect to the changes induced by bloodletting, however, we have already explained, in our essay on that subject, that the primary alterations are, through a direct influence upon the *vires vitæ*, established by the loss of blood; but, that the vital forces become farther affected by the cerebral influence as soon as a certain amount of impression is determined upon the brain. Hence is it, that as soon as syncope approaches, the blood loses its buffy coat, and coagulates with great rapidity.

Again, a thousand external agents are constantly acting with great instantaneousness upon the vital forces, changing their action, establishing disease, and affecting the appearance of the blood, whose direct operation upon the blood it would be as absurd to imagine, as that mental emotions act in this manner.

By parity of reason, all those foreign causes which are supposed to alter the condition of the blood primarily, such as miasma, the principles of contagion, animal, or other poisons, &c., may so impress the organic forces, or the nervous system, as to bring about analogous changes of action in the blood-vessels; and the circulating fluid may be rapidly altered in consequence, although our senses may not be able to appreciate the modified actions. Yet are there often, — nay, generally, present, before any sensible deviation of the blood from its natural character, certain vital signs which denote the altered states of the organic actions. When slight, they are overlooked; and it is only when quite advanced, that the physician is called to employ his lancet. "The slightest organic lesion, the smallest injury, the least perceptible disturbance of health, may become, if neglected, the parent of much serious mischief." (1) "Morbi sensim fiunt, ac

(1) Med. Chir. Rev. Lon. vol. 21, p. 516.

generantur, derepente vero adoriuntur."(1) Our argument, founded upon the changes which affect the blood during venesection, is a presumptive proof that, the changes which occur in this fluid in disease are owing to an analogous cause.

Before, therefore, proceeding farther in our inquiry, it appears to us that humoralism must despair of all but possibilities. It is met at the very threshold by repulsive facts. If individuals, remaining in apparent health after exposure to concentrated miasmata, offer examples of some modified state of the blood, how can it be said that the change is not owing to some antecedent change in the vital actions? Shall we have mere conjecture, without a shadow of proof, instead of the strongest analogy, and the direct evidence afforded by the effect of remedies after such incipient diseases shall have farther advanced?

Since it will not be objected that, in the foregoing cases, all the results, the modifications of the blood, &c., are brought about by the direct instrumentality of the solids, let us apply the philosophy to all other cases, which M. Andral considers so conclusive in the humoral pathology; adapting the phraseology, however, to the change of cases. "If," he says, "these phenomena are perfectly identical with those evidently produced by vitiated blood; if, on examining the body, we cannot detect here, any more than in the preceding cases, any constant lesion in the solids, and if we always observe a certain number of fundamental symptoms, whether these lesions exist or not, what is the conclusion consistent with sound philosophy? Certainly this, that here, as in the preceding cases, it appears that the primary cause of the disease should be referred to the blood."(2) The reasoning is better in our case, than in the other, since, as we shall endeavour to show, the humoral premises were only assumptions. We may say, however, at present, that in the case of the humoralist, it has not been shown, when disease is spontaneous, that the morbific cause enters the circulation, or that the blood is primarily diseased. It is evident, from the import of the foregoing argument, that no such proof has been offered. When poisons are artificially introduced into the circulation, nobody will deny that they will excite disease in the solids, as readily as when taken into the stomach. But even then, it is not the blood, as we shall show, but the poison, which excites disease of the solids. Finally, in our cases, it is proved and ad-

(1) Hippocrates 1, de Dieta. (2) Patholog. Anat. vol. 1, p. 407.

mitted that the blood is profoundly, and suddenly, altered by the action of the solids. Here, then, the principle in philosophy, for which the humoralist contends, is concerned about matters of fact: in the other case, with mere assumption and hypothesis.

The examples which we have hitherto stated demonstrate the superintending agency of the vital properties of the solids, and explain the various modifications of the blood which take place in diseases; thus conforming to the suggestions of observation, that, whilst foreign causes are constantly acting upon the vital properties, there appears to be no conceivable principle in the blood through which they can essentially vary its character. Poisons may act by inoculation, and the blood may speedily lose its characteristic appearances. But this may be rapidly effected by an almost inappreciable quantity, as in the bite of the viper, whilst some fifty pounds of blood may sustain a most notable change. Would not a morbific agent, which could so deteriorate the blood, be most likely in the first place to affect the vital properties of the solids, and thus to modify the functions? Or is the blood more susceptible to morbific agents than the irritability, sensibility, mobility, &c. of the solids?

In the foregoing case, the poison, being applied to lacerated vessels, is probably taken more or less into the torrent of the circulation; but very sparingly so, as we shall endeavour to show hereafter. If, however, absorbed at all, it is merely mechanically mingled with the blood, and therefore falls under the philosophy of solidism. And we may now say of all morbific agents, whose quantity may be supposed sufficient to affect the chemical composition of the blood in a direct manner, that this hypothesis necessarily supposes a chemical change in the agent itself; and that, therefore, the phenomena which result should be different from those which follow the agents in its unaltered state. The principles involved in the case supposed are alike applicable, as we shall endeavour to show, to diseases of all kinds, whether produced by miasmata, the virus of the small-pox, the mad dog, the bee, &c.; only, in the last case, they are more perfectly manifested by the very circumscribed condition of the disease, and an absence of constitutional disturbance; unless, perchance, the sting be inflicted upon the tongue, when the constitutional symptoms farther exemplify the philosophy of the active or more virulent poisons, as that of the viper.

SECTION III.

"THE poison may insinuate itself into the blood," says Dr. Mead, "and in the nature of a *ferment*, corrupt the whole mass." (1) Or, as Van Sweiten has it, "dispose all the humors to a sudden and bad putrefaction." (2) Cruikshank is confident in the doctrine of fermentation, which "certainly does take place in living matter." But take an unexceptionable case, like Dr. Cullen,—one who was professedly more a vitalist than humoralist,—who says that "the contagion of smallpox acts as a *ferment* with respect to the living fluids, and assimilates a great part of them to its own nature." (3) So, also, Pringle, (4) Alexander, (5) Walker, (6) Macbride, (7) Good, (8) &c. Or a later authority, Dr. Hosack, who says that, marsh "miasma no doubt operates as a *ferment* upon our whole system." (9) Or a still later, Dr. Billing, who has it thus: "I must indulge my propensity of searching for proximate causes, and would prefer any tolerable analogy to none at all. The process of *fermentation* affords an analogy. A little *leaven leaveneth the whole lump.* A most minute portion of smallpox virus, on the point of a needle, produces an inflammation similar to that from which it is taken. What is this inflammation but chemical decomposition? (!) What else is fermentation?" (10)

(1) Medical Works, p. 440; and ibid. on the Influence of the Sun and Moon, p. 191. And yet, "thus have I shown," says Dr. Mead, "how death may enter at the nostrils, though nothing properly venomous be inspired." (p. 138.)

(2) Commentaries on Boerhaave's Aphorisms, vol. 5. p. 230.

(3) First Lines, sec. 597. (4) Diseases of the Army, *Appendix.*

(5) Experimental Inquiry, p. 36. (6) Inquiry into Smallpox, p. 48.

(7) Exp. Essay, and Humoral diseases.

(8) Study of Medicine, vol. 2. pp. 546, 547.

(9) Lectures on the Theory and Prac. of Med. p. 169.

(10) Principles of Medicine, p. 56, 1838. *Third edition.*

Our author then goes through with the process of kneading, baking, &c. to the completion of the *crust.* We then read that, "all this passes off, we know, in a definite time, as we know that *dough* ceases to *rise* when all the particles *inflamed* by the *leaven* have gone through their process of decomposition. *So* ends smallpox, measles, scarlatina, plague, synocha petechialis;" and *so* we believe will end the humoral pathology. We think our author's "leaven" will produce such an "inflammation of the particles," that "the process of decomposition" will soon be over, and "the *dough* cease to *rise.*"

We cannot forego the temptation of administering to our humoral friends a little more "leaven" from our ingenious author. "Lues," he says, "is different; and here I must resort again to analogy." "As the acetous fermentation is less violent, though more permanent, than the panary or vinous, so the syphilitic decomposition (inflam-

Some of our humoral friends hold aloof from the exact *modus operandi* of the *ferment*. The doctrines of Hunter and Bichat are not sufficiently exploded to encourage a full avowal of the creed. Some modify the old doctrine, to suit the exigencies of any possible laws of vitality. Thus, Dr. Hosack. "This species of fermentation is called *silent* fermentation, as having some *laws peculiar to itself;* as *opposed* to that which takes place in the open air, or under *other* circumstances." (1) Will the humoralists be good enough to explain what sort of fermentation it is, that is "opposed" to fermentation? Or whether it be not, that "peculiar laws" have their own "peculiar" philosophy?

Still, when thus modified, the interpretation is in perfect harmony with the whole doctrine, and perhaps not the less so, from being utterly unintelligible. Even Billing completely identifies the "fermentation of inflammation" either with the panary, the acetous, or the vinous process. This is sound philosophy,—no clashing of fundamental laws. It is said by Dr. Hosack, that the panary fermentation, in humoral pathology, "is called by Walker the assimilating fermentation." But is not the bread-making, the vinous, &c. equally so?

And then as to *vital putrefaction*, Dr. Hosack enables us to furnish the true humoral *rationale*. According to the foregoing explanation of humoral fermentation, "so, also," he says, "may that *silent* form of putrefaction, that putrescent grade, take place in the less vital parts of the living system; for all the chemical changes which occur in the body are assuredly *controlled*, and more or less modified, by the influence of the vital principle." (2)

Here we have a recognition of our "vital principle," and,

mation) is gradual, but steadily pervades the system if unchecked by remedies." And so of hydrophobia. "It is a leaven which poisons the nervous system, takes a considerable time to ferment," &c.

And yet Dr. Billing considers it "of great consequence to correct erroneous theories, and thereby enable students to arrive sooner at well founded notions of practice." Well may our author say, that "John Hunter is not the law and the gospel." (*a*)

(1) Lectures on the Theory and Practice of Medicine, p. 111.

The *Italics* are ours. We have already said that we have taken this liberty with authors to save the necessity of some comment. We again make this general acknowledgment, and wish it to be understood, that whenever extracts may appear to operate in our favour, by this mode of emphasis, it is probable that the *italics* are ours.

(2) Ibid. pp. 114, 112.

(*a*) Ibid. p. 57, and Advertisement, pp. 9, 10.

perhaps, a clue to the manner in which "putrefaction" takes place under the direction and "control" of the vital principle.

Doubtless it has been supposed that to Dr. Billing is due the originality of the *leavening* process, from his "propensity," on this occasion, "of searching for proximate causes."(1) But, in matters of science, we think that all honours should be fairly distributed. This has been held sacred even in fiction; and Burton takes up the defence of the latter. "Oribasius, Aëtius, Avicenna," he says, "have all out of Galen, but to their own method, diverso stylo, non diversa fide. Our poets steal from Homer. He spews, saith Ælian; they lick it up. Divines use Austin's words *verbatim* still, and our story dressers do as much. He that comes last is commonly best;

> ——— "donec quid grandius ætas
> Postera sorsque ferat melior." (2)

We are less interested, however, about the origin of the leavening philosophy, and of the similitude of the humoral method of making diseases to the making of bread, vinegar, &c., than we are respecting the ill-considered nature of such principles, and the pernicious influence which they have created in medical philosophy. As to the doctrine, it is as old as Galen; and we shall show, once more, the patronage it enjoys, and the demonstration by which it is enforced in our day of accumulated knowledge. Thus, Dr. Hosack: "That 'a little leaven leaveneth the whole lump' *is as true in fevers as in making bread*, or in the conversion of acescent fluids into the acetous acid,—and that upon the *same principle of assimilation*. That one spoiled herring will taint the whole cask, is well known to every housewife or fishmonger. Hence the great care of the Dutch

(1) Dr. Parsons, in his Crounian lecture, after stating that Dr. Croune had solved the cause of muscular motion, by showing that it results from the "animal spirits flowing to the muscle, arterial blood determined in greater quantity than ordinary, and a *fermentation* raised by their admixture, by which the muscle is swelled and shortened," complains that "this learned author's *fermentation*, as it serves to account for muscular motion, has been seized on by most of those that followed him, *without giving him thanks for it.*" (*a*) These pirates were no less than Mayow, Borelli, Willis, Bernouilli, Keill, &c.

Bernouilli explained the doctrine of fermentation as applicable to muscular motion, by supposing that the "*spicula* of the nervous juice strike against the finer particles of blood." (*b*)

(2) Anatomy of Melancholy, vol. 1.

(*a*) Philos. Trans. vol. 43, p. 1, &c. And Abridg. vol. 2. p. 1118.

(*b*) Dis. de Motu Musculorum.

in their herring fisheries to salt down their fish as soon as they are taken. They never permit the sun to rise upon them." (1)

This, by admission, is humoral philosophy; and that it has no better foundation, we shall have demonstrated by the records which surround us. (2) "Thou canst not say, 'twas I that did it."

The able Mead, in accounting for the effect of venomous poisons, says "it is alone by the great activity of the nervous fluid, one part of which being infected, immediately taints all the rest." (3) Dr. Stevens lays down the same doctrine of contamination. (4) He also affirms that "the blood often turns to an *infected jelly* in the cholera." At another time we have from him the following definite and intelligible enunciation: "During the dormant period which precedes the attack, the poison itself *either* undergoes certain changes, and its narcotic properties become more active, *or, it chills* and produces *certain changes* in the blood, which renders it almost incapable of stimulating its own vascular organs; and then comes on the cold stage which is so invariably (?) met with in all those fevers which are produced by the aërial poisons;" "the heart becomes paralyzed," &c. (5)

It is no sort of consequence, whether the virus operate on the instant, as after the insertion of the worari poison, or that of the viper; or, whether it suddenly produce its manifestations, for the first time, some months afterwards, as in hydrophobia, when the blood has undergone many renewals, and, perhaps, every

(1) Op. Citat. p. 114.

(2) As we are most anxious, however, not to misapprehend this subject, or that it should be misapprehended by others through any mistake of ours, we refer back to our first section for expositions that are given by other able humoralists, who justly rank high amongst philosophers.

(3) Preface to Essay on Morbid Poisons, p. 27. (4) Op. Cit. p. 143.

(5) Pp. 244, 257. This doctrine, of the "paralyzing" nature of the blood, was very common in Mr. Hunter's day, and mainly led to the general use of stimulants. (*a*) A writer in the American Medical Recorder (*b*) considers it a rare specimen of Brunonian Philosophy as applied to medicine. It has a strong advocate in Dr. Cook, (*c*) and has lately received an impulse from the excellent pen of Dr. Hooker. (*d*) Beddoes (*e*) and Trotter advocated it in scurvy, though neither of them regard this disease in a humoral sense.

(*a*) See Hunter on the Blood, p. 227; and Jackson on Contagious Fever, p. 222.

(*b*) Vol. 14, p. 417. (*c*) Transylvania Journ. of Med. and Surg. vol. 1, 1828.

(*d*) Essay on the Relation between the Respiratory and Circulating Organs. Boston, 1838.

(*e*) Obs. on Scurvy, &c. p. 50.

thing, save the vital forces, has been carried off, since the deposition of the poison. The assumption is just as good in one case, as in the other.

Seeing, however, that nature is hostile to empty words, and that she is utterly opposed to the doctrine of fermentation, whether panary, acetous, vinous, or putrefactive, or, either of these "opposed" to each other, we shall still endeavour to arrive at the truth by interrogating, as in the foregoing section, her actual phenomena. There is no other possible method of understanding her laws, but through their direct results. Any other process begins in hypothesis. In physiology, the vital signs are the only sure guide. You will never find the light in the physical products. The darkness with which chemistry has overspread the science is an ample proof of this. In pathology, how far morbid anatomy contributes, we have explained our own views in other places. Its relation is wholly subordinate; and when thus kept in its place, morbid anatomy is a useful auxiliary, and we acknowledge our indebtedness to it. As to inductions founded upon appearances of the blood, which affirm its primary deterioration, we shall still endeavour to show them to be a mere assumption.

It should also be borne in mind, that from what we have now seen, the humoral pathology proceeds upon the assumption that morbific and remedial agents combine chemically with the blood. This conclusion, of course, nearly or quite divests the blood of vitality; and the whole doctrine is adverse to the supposition that foreign causes induce disease in the blood by acting upon any vital properties that may be supposed to belong to it.

In regard to analogy, we certainly shall not be denied its advantages, considering how indispensable it is held to be in the humoral pathology.

If a mechanical irritant, applied to the skin, produce inflammation, we have an indication of the manner in which insensible poisons, or other morbific agents, derange the actions of other parts. The same principles are concerned in all the cases.

Again, a sudden change of temperature produces sporadic colds; but, now and then, the same condition of disease sweeps over a continent,—over the world. Intelligent humoralists do not suspect any primary agency of the blood in the former case; but, in the latter, without any greater reason, they cry out *fer-*

mentation, corruption, vitiation, of the blood. But shall we not hold them to their own principle of analogy,—especially considering the strength of it in these particular cases?

Mr. Boyle, on the subject of humoralism, says, that "great changes may be made in the human body by any subtle effluvia. The bare *scent* of a dose of physic operated better upon a young gentlewoman, than it did upon her sister who took it; and Dr. Pfeil, when he wanted a purge, would go into an apothecary's shop, where purging electuaries were preparing, the scent whereof would work as well with him as a dose of medicine. A woman used to purge herself with beef-broth; but having broke her leg, used no other cathartic than the *scent* of it. Acosta relates, that on some of the mountains of Peru, the air produced excessive retching, vomiting, and purging. A still greater proof of the power that effluvia have upon the body arises from the propagation of infectious diseases." "The subtle effluvia which float in the air, before any change of weather, are felt by those valetudinarians who have formerly received bruises, wounds, and other injuries; and that, too, *only* in the very part, where they happened." (1)

M. Lemery states, that he knew two people that purged and vomited in a most violent manner, after being confined some four or five hours where there was a large quantity of damask roses. (2) But the mind itself will effect the same thing. Thus, it is related by Dr. Fairfax, that "Mrs. Raymond, whenever she hears it thunder, even afar off, begins to have a bodily distemper seize on her; she grows faint, sick in her stomach, and ready to vomit. At the very coming over of it, she falls into a downright *cholera*, and continues under a violent vomiting and looseness as long as the tempest lasts. And thus it hath been with this gentlewoman from a girl." (3) Until the nature of lightning was understood, it was supposed that it corrupted the blood in such cases. "At any moment," says Beddoes, "inflammation may be kindled in any part by some causes which we can distinguish, by others too subtle for our senses, as, perhaps, by a thunder cloud passing over head." (4)

(1) Philosophical Works, vol. 1. pp. 89, 91.

(2) Hist. de l'Acad. Roy. 1695, p. 69. On the other hand, Lord Bacon says, "Odores ad confortationem cordis præcipue utiles sunt." (De Vit. et Mort.)

(3) Philos. Trans. Lon. vol. 3. p. 287. Ab.

(4) Researches on Fever and Inflam. p. 103.

Now, if any one of the foregoing effluvia, and thunder and lightning, operate primarily upon the solids, which we suppose will not be denied of most, we see not why the others should not follow the example. Thus we should bring those humoral diseases, small-pox, measles, scarlatina, &c. within the same rule.

Coincident with the foregoing examples is the peculiar irritating effect of ipecacuanha upon the lungs, in certain constitutions, whether inhaled, or taken into the stomach; of the nitrous oxide, and various other gaseous substances. They are all followed by their full manifestations as soon as they come in contact with the lungs. Take any substance capable of immediate effect, and nobody can doubt that the phenomena depend upon some direct impression upon the vital forces of the solids. Absorption is out of the question, for the results follow before the process can have begun; and what farther proves the local action is the entire subsidence of the phenomena, as soon as many of the exciting causes are withdrawn.

If then, the vital forces of the solids may be thus suddenly affected by a thousand different agents, — often resulting immediately in inflammation, — can there be any doubt, that other agents of different properties, and slower operation, affect the same forces, only more gradually, — altering them in a different way, and establishing various modifications according to their peculiarities? And yet are we told that the vital powers are only the dream of the visionary man.

The whole theory of the action of foreign causes upon the living organization, whether they consist of morbific or remedial agents, is inadvertently shown, in many places, by Dr. Stevens himself. Thus, in speaking of the opposite effects of cold water, when drank under certain circumstances, he shows us that its action, in both cases, is wholly upon the solids, — first upon the stomach, then by sympathy upon the extreme vessels, heart, brain, &c. (1) But such is the force of hypothesis, and notwithstanding the violence of the contrast, in immediate connection with the preceding illustration, Dr. S. supposes two examples of individuals that are affected in different degrees by an aërial poison, *according to their different susceptibilities*, when the solids are instantly lost sight of, and the effects ascribed to "the injury sustained by the blood from the noxious agent."

(1) On the Blood, &c. p. 128.

Again, we may advert to causes of a purely mechanical nature, which will serve as an ample illustration of the principles through which many of the morbific agents operate, of which we are yet to speak. Thus, it is stated by Mr. Travers, that "Sir A. Cooper relates in his lectures the case of a man of middle age, and robust frame, who had suffered much agony for several days, from a thecal abscess occasioned beneath the nail of the thumb, and who, a few seconds after the matter was discharged by a deep incision, raised himself by a convulsive effort from his bed, and instantly expired." Other instances of the same nature are recorded by this author.(1) "I have seen a man die," says Mr. Hunter, "almost immediately upon the loss of a testicle. I have seen convulsions immediately attend the operation for the hydrocele, so that I have almost despaired of recovery. I have seen a most violent sympathetic fever, delirium, and death, follow in consequence of dividing parts in the leg, and searching after a bleeding artery." (2) "I have known," says Bichat, "the operation of cutting away the prepuce immediately fatal." (3) "I have known," says Andral, "the *bite* of a single leech produce symptoms of tetanus." Roux, (4) and many other authors relate analogous cases. "A man," says Mr. Travers, "had been bitten in the finger by a cat, and in whom symptoms resembling hydrophobia had been present for twelve hours, being in perfect possession of his mind, died in three minutes while the excision of the bitten part was performed." Is it an antecedent corruption of the blood which destroys life at the moment a blow is inflicted upon the region of the stomach? Or, if the subject did not perish in consequence, would you think it a humoral result if there were an immediate loss of sight, or cerebral inflammation? But, since these effects will not be ascribed to any deterioration of the blood, may it not be very probable, that, when the action of medicine or other agents produce impressions upon remote parts by the medium of the stomach, it is through the same principle of sympathy? (5) The philosophy is the same as in the following case. "On dropping water upon an inflamed eye, the sound eye became red on the spot; and the trial was resumed some time after with the same result." (6)

(1) Inquiry concerning Constitutional Irritation, vol. 1. pp. 22, 25.

(2) On the Blood, &c. p. 397. (3) Patholog. Anat. vol. 1. p. 16.

(4) Med. Chir. Rev. July, 1833. p. 195.

(5) The foregoing are cases for the consideration of those who deny the existence of vital powers.

(6) Beddoes, Ut. Cit. p. 103.

Again, four years had gone by, according to Dr. Joerdens, since a boy of 14 years, trod on a piece of glass, that penetrated the foot. This was removed, and all was well again. At the end of the 4 years, however, this boy "began on a sudden to talk in a very strange, and wild way." He became turbulent, and blows were inflicted upon him, "but all was in vain." Delirium set in, and the straight-jacket was applied. But nothing appeased the patient. The whole house was in perpetual uproar, and the doctor, more than once, fled for his life. "The patient cried, stamped, foamed, and tore till I was driven away."

Such was the state of the case, and there was no doubt the boy was full of bad humours. The case was apparently a strong one for humoralism. But the physician at last bethought him of looking at the boy's foot which had been impregnated with glass four years before; when there was found near the ball of the great toe, a small reddish elevation. "The moment I made pressure upon it, the seizure returned with violence, and to appease the phrenzy in some degree, I was obliged to retire." An incision being made, "a very trifling portion of glass presented itself, and was removed. Much as the patient had raved, during the operation, with equal suddenness did all the symptoms vanish; and he was surprised on being told of all the senseless things he had uttered, and begged pardon." (1)

Were all the reputed cases of humoralism as thoroughly sifted as this,—"usque ad pedem,"—we had not taken up our pen in defence of solidism.

SECTION IV.

ALTHOUGH, admitting that morbific agents enter the circulation, it might be reasonably inferred that the sanguiferous organs are mainly concerned in all the morbid alterations of the blood, in consideration of the dependence of this fluid upon those instruments for much of its formation, and the rapid changes which they are well known to exert in its composition, as also, the fact

(1) Joerdens ap. Hufeland Journ. vol. 4. p. 227.

that the agents must pass the highly sensitive vessels whose vital properties are capable of receiving and maintaining impressions that may not be manifested till some distant time; yet a controverted point of this nature requires a variety of proof and illustration.

We shall have no difficulty in finding all the proof that we desire; and, although varying more or less from the foregoing exemplifications, its application is exactly the same. We constantly meet with analogous examples in the secreted products, where the powers of the secerning system are suddenly altered, along with those of the sanguiferous, and the secretions as suddenly changed.(1) Thus, we are told by Mr. Hales, that a dog, exposed to a temperature of 146° Fh. began immediately "to drivel a great quantity of *red* foam which did stink so intolerably, that a labouring man, who went near it, was almost struck down *in an instant* with the stench." Here, there is nothing in the exciting cause that can generate disease of the blood, or the morbid secretions, excepting through the agency of the solids. The example is also pregnant with information, — since the first result led immediately to a coincident phenomenon; whilst others, again, "die of a rose in aromatic pain."

Anger, according to Hildanus,(2) and others, imparts a poisonous quality to the saliva, — so that a bite under the influence of this passion has been frequently attended with fatal consequences. It is precisely the same process, the same primary modification of the forces and actions of the solids, which starts a redundant secretion of tears, or of urine, when the passions operate; only, in the former case the vital forces are more profoundly affected than in the latter. The same parallel holds as well in the lower animals as in man. Thus, "fear and anxiety of mind," says the able Percival, "have a remarkable effect on the secretion of urine in the human subject, and the same influence is equally striking among animals. It is by no means an uncommon occurrence upon a race-ground, to meet with a racer and his jockey, who are said to be *leaky* before starting; on which occasion I should argue, that the feelings of the horse and man were not widely

(1) We may say, once for all, that we do not intend to imply that the vital forces of the solids have any direct influence upon the blood; but that the changes are produced wholly by the action of the organized matter, which those forces animate and by which their action is conducted.

(2) Cent. V. 75, and Cent. I. 85.

different." "Who has not seen a cur do the same thing at the sight of a horse-whip?" (1)

So readily may the secretions be influenced by slight impressions upon remote parts, that Helmont affirms "he could tell by the taste of the milk, whether the ass had been curried that day;" and Pelletier, Broussais, and others, have remarked that the milk of a passionate nurse makes a child sick. (2) But, notwithstanding this notorious fact, it is an universal doctrine in humoralism, that cathartics affect the milk by entering the circulation; whilst it is, also, equally known that their operation arrests even the absorption of chyme. It is plainly a matter of sympathy; and, we may add that the milk of the mother is more frequently affected by indigestible food than by cathartics.

From the foregoing considerations, it becomes more and more apparent, that no conclusions, as to the antecedent state of the blood in disease, can be drawn from any changes in its appearance, or its composition. The changes, as we have seen, may begin the moment there is any departure from natural action, and this, too, where it may be inappreciable without the closest scrutiny. Indeed, the very application of bloodletting supposes the existence of a morbid state of the solids, which it is designed to remove; and we do not believe there was ever a humoralist who has abstracted blood upon any other principle. How untenable the assumption, then, that, "in tropical fevers every drop of the blood is deranged before the attack; and that after this, every fibre is affected." (3)

But humoralists, overlooking the fact that disease already exists in the solids when blood is abstracted, if this fluid happen to be dark, assume that such was its condition antecedently to any lesion of the former. It is universally known that various phenomena distinguish the dormant stage of fever, which are abundantly indicative of a general disturbance of the solid parts of the body. But, Dr. Stevens shall confirm our views upon this subject, and show how entirely it has been assumed that "the morbid changes of the blood are the first in the train of symptoms."

"During the dormant stage of marsh fever," says Dr. Stevens, "there are certain premonitory symptoms which warn us of the danger; for immediately before the attack, there is a decrease of

(1) Lectures on the Veterinary Art, Part. 3. p. 24.

(2) See, also, Simson de Re Med. Dissertationes, 3 and 4, 1726.

(3) Dr. Stevens on the Healthy and Diseased Properties of the Blood, p. 17.

action, particularly in the vascular solids. The pulse is less frequent than in health, there is less animal heat evolved, the temperature of the blood, and, of course, of the whole body, is reduced sometimes as low as to 94°. There is generally great lassitude in the whole system, the mind is dejected, the appetite fails, the bowels are costive, and the fœces which are passed are less bilious, the tongue is foul, with a bad taste in the mouth, particularly in the morning, there is a tendency to headache, wandering pains in the bones, uneasiness in the back, the face is pale, the countenance is dejected, the eyes become heavy, the skin is sallow, and when the patient sleeps his rest is frequently disturbed with unpleasant dreams. *These are the premonitory symptoms* which, in this fever, precede the attack."(1)

Now if there be any thing valuable in absolute proof beyond the Berkeleyan hypothesis, "these premonitory symptoms" should settle unequivocally the doctrine of solidism. Whatever may be the condition of the blood, when abstracted under the foregoing circumstances, though it is rarely done till the disease has advanced still farther, the facts which we have hitherto stated do not permit a question to be raised as to the dependence of any alteration in that fluid upon the diseased state of the solids. A different conclusion would be clearly a mere matter of assumption, whilst it would violate all that is known in relation to the dependence of the blood upon the solids, and the rapid changes which we have shown to arise in the condition of the former, from any modification of the latter. And yet Dr. Stevens brings forward a case where he "bled a sickly looking sawyer, who was working in the marsh," and who "had most of the premonitory symptoms," which distinguish the foregoing fever, to prove by the "peculiar appearance of the blood" at this stage of the disease, that the sanguineous lesion had been the cause of the premonitory symptoms.(2) So, also, Dr. S. bled a patient in this city, and arrived at the same conclusions. The subject had been supplied by Dr. Ludlow, who informs us that he broke out with the eruption of scarlet fever on the same day. This, then, is the whole amount of proof which humoralism has ever produced to illustrate the origin of disease in the sanguineous fluid; the whole weight of which, however, is directly, and we may again say conclusively, in favour of solidism. Equally to our

(1) On the Blood, &c. p. 217. (2) Ibid. 312, &c.

purpose is the altered state of the secretions which has been alleged with the foregoing view.

What we have now said may be also affirmed of the experiments of Dr. Potter, of Baltimore, (often cited by humoralists,) during the prevalence of yellow fever. The solids were already in a state of disease when the blood was abstracted. In his own case, *immediately* after leaving a house on his first visit, where a whole family were sick, he was seized with *nausea, great lassitude*, and other symptoms, denoting the access of fever; the full developement of which he thinks was prevented by the violent operation of an emetic. (1) Here the very nature of the remedy concurs with the history of the case in showing that the morbific impression had been wholly sustained by the solids.

It is stated, by Dr. Rush, that "a peculiar sallowness of the complexion was observed to be general in Philadelphia, and at Alexandria, previous to the yellow fever of 1793." This was quite anterior to the irruption of the epidemic, and is an important historical fact. Again, Dr. R. mentions an instance where a person was so highly under the influence of the miasmatic poison when he left Philadelphia, that his breath "created sickness at the stomach in four or five persons who sat at the same table with him in the country;" (2) and yet he escaped an attack of the fever. Now, it will not be doubted that all the foregoing phenomena depended upon a diseased state of the solids; and admitting that the blood may have been "black," does not all fact, analogy, and philosophy, teach us that this alteration depended as much upon the condition of the solids, as the "sallow countenance" and the "fœted breath"? Besides, can it be entertained that the *pabulum vitæ*, that which circulates in every part, excites every part, and sustains the whole, can have been so diseased as to produce, *per se*, all the "premonitory symptoms" of fever, without, in all instances, urging on the disease to a violent explosion?

It is well known that chronic diseases, and important disorganizations, may be going on for months, and even years, without being suspected to exist; and their local nature, by the way, is abundantly demonstrative of the doctrine of solidism. But the severest grades of acute disease may be equally latent. Thus, it is stated by Lord Anson's surgeons, Lind, and others, that the

(1) Memoir on Contagion of Yellow Fever, &c.

(2) Medical Inquiries and Observations, vol. 4, pp. 176, 177.

sailors have often dropped dead, with a sudden invasion of the scurvy, upon deck, or from the mast, when on duty. The solids are found to be greatly diseased in all these cases, especially the sanguiferous organs. "Those who died suddenly, and without any obvious cause," says the Philosophical Transactions, "had the auricles of the heart as large as the fist;" and in a case where a young man "died suddenly, and to all outward appearance not very ill, he had his pericardium eaten up, and his heart covered with deep ulcerations."(1) "A person," says Sir G. Blane, "may be said to labour under the scurvy before it betrays itself by any obvious symptom; the chief mark of which is the incurable state of ulcers, whether they appear spontaneously or in consequence of slight accidents. Soft, indolent tumors, also, rise under the skin where small blows have been received,"(2) &c. Moseley states, "if a person, newly arrived at the West Indies, has subjected himself to any of the causes which may produce fever, previous to its attack, he has sufficient warning given him, if he will attend to it, to cure it by anticipation."(3) And so Dr. Rush of the yellow fever, of Philadelphia.(4)

It is, therefore, palpable, if we admit what we shall show is not true, that the blood is always "black," or otherwise greatly altered, in fever, scurvy, or under any conditions of disease, it affords not the slightest proof of a primary deterioration of that fluid. The humoral conclusion is a mere assumption. The moment morbific agents have affected the solids, we may reasonably look for some change in the blood. This will hardly be objected to, considering the foregoing facts, by those who believe the blood to be so constituted, that the poison shall first affect its composition and properties. As disease advances, so *pari passu*, may there be an increasing alteration of the blood. This is an essential doctrine of solidism. It admits all that the humoralists are attempting to ascertain by analyzing the blood. It is contented with what existing knowledge suggests, without departing from an observation of nature; and we have many strong grounds for believing, that no chemical researches into the varying conditions of the blood can either add to our knowledge of the subject, or become of the slightest practical advantage.

(1) Vol. 26. p. 223. (2) Diseases of Seamen, p. 505.
(3) On Tropical Diseases, p. 444.
(4) Medical Inquiries, &c. vol. 4, pp. 29, 182, 183, 189, &c.

But let us examine some of the important facts of the humoralists,—look at the logic, and weigh the conclusions. We will first take up Dr. Stevens, since, as we have said, he is of oracular authority.

"The dark colour of the blood," says Dr. S., "which we observe in the beginning of pestilential fevers, is the effect of the poison on the vital fluid; but the blackness in the last stage of these diseases is produced by the loss of the saline ingredients; which I can prove beyond all question the true cause of the red colour of the blood."

Another fundamental principle consists in the statement, that there is a diminution of the saline principles of the blood in febrile affections, and that this is the cause of the dark colour of the blood. But on other occasions, it is said to be owing to the presence of carbonic acid; whilst in a third instance it is occasioned by a poisoned state of the blood. These conclusions probably depend upon our author's statement, that "we know that when a powerful current of electric fluid passes through the body, it *destroys the saline matter* of *even* the arterial blood, and makes it perfectly black." (1) Indeed it is in this way that our author annihilates the salts in "climate fever." "There is no bowel complaint, &c., to drain them off," "yet they disappear as certainly, and almost *as suddenly as in those* that are killed by the electric fluid." (2) But, again, he founds his belief that the blood loses a proportion of its saline constituents, in intermittents, upon the assumption that the salt boilers in a miasmatic district of the United States escape the fever, whilst the inhabitants of the village, (Salina, New-York,) in which the salt-works *are situated*, are particularly liable to invasion. Again, the blood of the villagers was dark-coloured, oily, &c., whilst that of the salt-boilers was florid and healthy. The inhabitants, also, are not liable to fever, whilst the marshes are impregnated with salt; and at other times they employ salt food and saline medicines successfully as prophylactics. It is also affirmed that our neighbours, in New-Jersey, who live near

(1) In the yellow fever, Mr. Jennings thinks, that "whether or not it (the loss of the saline constituents of the blood) be the *first* change induced by the morbific poison, and a *cause* of the subsequent diseased actions which we call fever, future observations must determine." (*a*)

(2) On the Blood, &c. p. 209, &c.

(*a*) On the Chemistry of the Blood, in Trans. of the Provincial Med. and Surg. Association, vol. 3. 1835. p. 60.

the salt water marshes, are especially exempt from fever. These statements, which are often quoted by authors, are utterly without foundation, as is also the strange affirmation that "persons who have been long under the influence of the marsh intermittent, are cured by the saline atmosphere of the salt-works, more effectually than they are either by bark, purgatives, or even by large doses of the sulphate of quinine."(1) We speak upon this subject with a distinct knowledge of the facts, having investigated the diseases in the western region of New-York during a period of nearly two years. Indeed, so exactly otherwise is it, the presence of salt upon the marshes has evidently contributed to that peculiar decomposition of vegetable matter which forms the predisposing cause of the fevers in question. And this construction is sustained by the special prevalence of marsh fever in the saline districts of the south of France, along the saline coasts of the Mediterranean, and in many parts of Great Britain; where, it is thought by all observers, that the impregnation of the marshes with salt imparts greater activity to the morbific exhalations. Lord Bacon, always cautious as to facts, remarks that, "quodque mirum videri possit; paludes *aqua salsa* per vices inundatæ, *minus salubres*, quam quæ aqua *dulci*."(2)

But, the humoral hypothesis upon this subject, which supposes the absorption of saline matter from the atmosphere, may be at once put at rest by the fact, that the air is incapable of suspending muriate of soda, unless in almost inappreciable quantities. The air of the ocean differs not in this respect from that of the land.

And then, as to the imputed deficiency of salts in fevers, Craigie remarks that, "in tropical countries it has been observed repeatedly that all the secretions are more saline than in temperate latitudes; and if there be a difference in the saline constitution of the blood, it is rather augmented than diminished in the regions in which yellow fever prevails."(3) Malcolmson makes the same affirmation of the saline and alkaline character of the urine in beriberi; it being most so in proportion to the severity and duration of the disease.(4)

(1) Stevens on the Blood, pp. 310, 312, &c.
(2) Hist. Vitæ et Mortis, in Op. t. 3. p. 349.
(3) Craigie's Practice of Physic, vol. 1. p. 249.
(4) Essay on Beriberi, pp. 138, 293, 294.

Our author has, also, a double hypothesis as to the cause of tropical fevers; the same affections being the result, at one time, of a poisoned state of the blood, and at another, of the loss of the saline ingredients. But neither of these will suit the cases of instantaneous seizure which we shall relate.

"The sickness of the stomach," says Dr. Stevens, "which is so generally met with in the commencement of all those fevers that are produced by the specific aërial poisons, is probably the effect of the poison itself, which is *thrown out of the circulation*, and causes irritation in the gastric organs, in the same way that tartarized antimony produces nausea and vomiting when we inject a small portion of that agent into a vein." At a later period, the gastric irritation "is evidently, in these fevers, produced in a great measure by an excess of acidity in the stomach. This may, perhaps, arise from the decomposition of the saline ingredients of the blood by the nervous or electric fluid which appears to exist in excess in all fevers, but particularly in those of a malignant character." (1)

The work, from which the foregoing extracts are made, abounds with similar doctrines. We give them as received examples of humoral facts, and of humoral pathology.

Dr. Stevens takes to himself the credit of mainly ascertaining and settling, by experiments, "that all acids give a dark colour to the colouring matter of healthy blood." "That the pure alkalies have a similar effect with the acids." "That the whole of the neutral alkaline salts immediately change the venous blood to a bright arterial colour." That the neutral salts maintain the fluidity of the blood. (2)

There is also a general disposition to admit the importance and novelty of the foregoing facts; at least, of their application in the treatment of disease. We have already stated Dr. Latham's opinion as to their merit, and originality. And thus the English editor of Dr. Edwards: "Stevens has the merit of greatly extending the experiments with acids and alkalies, as well as applying them." (3)

So, also, Dr. Tweedie: "If the premises advanced by Dr. Stevens be correct, the saline treatment he has had the merit of

(1) On the Blood, &c., pp. 53, 441, 445, &c.

(2) Op. Cit. p. 360, &c. But it is affirmed, that in proportion to the advanced stages and malignancy of fevers, the blood loses its salts. Why, then, does it often remain permanently fluid, in these cases, after death?

(3) Edwards on the Influence of Physical Agents, &c. p. 484.

proposing, appears the most likely to correct the vitiated condition of the blood." (1)

Thus, too, Mr. Pereira: "Dr. Stevens has shown that the red colour of the blood depends on the presence of its saline constituents, and that when deprived of them, this fluid is black." But we have less objection to this, than to the next step which is taken,—viz. "the obvious indications, therefore, in the treatment of cholera, are to restore the water and saline matters to the blood." (2) And so of fevers. Will their transfusion restore the blood to that unique and homogeneous state which is the ordinary product of the actions of life? Or, if so, will it re-establish the functions which are now verging on extinction? Why should not "the water and salts," thus artificially introduced, be carried off by the same morbid process which originally exhausted the blood?—especially as the same action now exists in an aggravated degree? (3) Have you done any thing to subvert that process?

Again, we are told by Dr. Elliotson, that "Dr. Stevens seems to have proved that the colouring matter of the blood is really black, and acquires redness only by the action of the salts upon the hematosine;" that "if blood is black from the want of salts, very little of them will make it florid; if black from the presence of carbonic acid, &c. the quantity of salts requisite will be proportionate to the quantity of the blackening agent," &c. (4)

"Dr. Stevens," says Dr. Hodgkin, "has paid great attention to the changes of tint which this colouring matter (of the blood) exhibits by a mixture with different chemical re-agents. He states that both acids and alkalies occasion it to assume a drab colour;" (5) &c. "Dr. Stevens, in his Treatise on the Blood,"

(1) Cyclopædia of Prac. Med. Art. Fever, p. 213.

(2) Pereira's Elements of Materia Med. part 1. p. 312.

Here is another specimen, in a work on *Physiology*. On mixing carbonate of potash with blood, Müller says: "I was thus able to give rise *to the process* by which the *inflammatory crust* is formed."! (*a*)

(3) Dr. Elliotson says, there was, in consequence, "generally a greatly increased discharge of fluid from the alimentary canal." (*b*) So there was, also, from the skin.

(4) Ut Infra, p. 150.

(5) Lectures on the Morb. Anat. of Serous and Mucous Memb. vol. 1. p. 299.

Dr. S. can hardly be said to have paid much attention to the "tints." It was all "black" with the acids and alkalies, and all "red" with the neutral salts. Dr. S. was intent on other objects than the *shades* of colour.

(*a*) Elements of Physiology, vol. 1. p. 117.

(*b*) Human Physiology, part 1. p. 161.

says Dr. Turner, "has the merit of proving the saline matter of the blood to be essential to the phenomenon" (arterialization);(1) Müller, also, appears to consider these observations of recent date, and to be quite unsettled.(2)

Thus might we go on through many other authors, showing the novel light in which this matter is regarded, and the extraordinary applications which are made of these experiments in a porringer to the philosophy of life,—to the mysterious ways of nature in her vital actions,—her inscrutable processes in disease,—and the substitutions which they enable us to make for the products of the living organization. We have been drawn into this inglorious subject by the necessities of the case, as it forms one of the fundamental grounds of humoralism.

We know of no better method of shaming this pretence, than by showing that all the experiments had been long since and far more extensively made,—all the inductions drawn, in all their aspects,—all their absurd applications made, and finally denounced by the humoralists themselves. Had this been fairly stated, and the subject divested of the fascinating disguise of novelty, and the credulous thus put upon their guard, we would have been the last to complain of any modern imitations of the alchemists.

Nearly 200 years ago, Robert Boyle, and Dr. Slare, made these experiments, and came to all the recent physiological conclusions. Acids, Mr. Boyle says, give the blood a *dark colour*, and produce *coagulation;* whilst the urinary salts render it *florid*, and *preserve its fluidity*. Dr. Slare, as we have seen of Dr. Stevens in respect to the salt marshes, supposes, from his experiments with salts, that the arterial colour of blood depends upon saline matter in the atmosphere. Of "the volatile salt," he says, "I have observed it to advance the tincture of *arterial* blood, and which is very curious, if you dissolve it in your blood whilst you are bleeding at one of your veins, that blood will become very florid, and like arterial blood. Therefore, since nitrous salts produce none of these tinging effects, this corollary seems much to favour the notion, that the effects of the air upon the

(1) Elements of Chemistry, "respiration."

(2) Elements of Physiology, vol. 1. p. 321. See, also, Dr. Gregory, and Mr. Irvine, in Monthly Archives of Med. Sciences; and Lon. Med. Gaz. 1834.

blood may be due to such salts as are of a volatile-alkalisat nature." (1)

This subject finally attracted universal attention. Experiments upon the blood with acids, salts, and various other substances, were multiplied in countless numbers; and with the same results as to colour, fluidity, &c., and the same practical applications were made, or opposed, as laid down by Dr. Stevens; but were finally more or less abandoned, in practice, by the projectors themselves. The most distinguished men were engaged in this crusade. We refer to some of them, that our statement may not want its authority. There was F. Hoffman, (2) Boerhaave, (3) Petit, (4) Boissier, (5) Francassiti, (6) Malpighi, (7) Baglivi, (8) Bœcler, (9) Schwencke, (10) Eichel, (11) Eller, (12) Vieussens, (13) Lancisi, (14) Rutty, (15) Quesnay, (16) Pitcairne, (17) Friend, (18) De Heyde, (19) Magnet, (20) Floyer, (21) J. M. Hoffman, (22) Colbatch, (23) Ettmuller, (24) Harris, (25) Coward, (26) Bohn, (27) Fuller, (28) et multi alii.

Many of these, and other experimenters, not only exhibited their re-agents by the stomach, to be thence taken into the circulation, but they took the more philosophical course of endeavouring to neutralize poisons, of cleansing and otherwise restoring the blood, by injecting the counter-agents into the veins. This was the great age of experimental transfusion, by which the absurdity of the principle which had suggested the chemical practice was fully established. Now it was, too, that the mechanical and mathematical doctrines cut the greatest figure; and now it is, that we are again looking with favour upon Cockburn's table by

(1) Philos. Trans. vol. 2, p. 551. Vol. 17, p. 898.

"A standard of volatile salts," says Dr. Slare, "should be settled. At present, I can think of none better than water."

Mr. Boyle afterwards pursued his experiments, which may be found in his Apparatus ad Historiam Sanguinis Humani.

(2) L. 1, De Acidi et Viscidi pro Causis Morborum et Alkali, pro ils Debellandis Insufficientia, p. 52. (3) Element. Chem. t. 2, p. 378. A great variety. (4) Lettres d'un Médecin : let. 2, de Sang. Phenom. cum variis Liquidis Commist. p. 35. (5) Sur l'Effet des Médicam. p. 39. (6) Philos. Trans. vol. ii. p. 490. (7) Diss. de Vis. Struct. et de Polypocord. p. 132 Cons. Med. Cent. (8) Exp. circa Sang. in Op. p. 347. (9) Disp. an nitrum Sang. solvat. (10) Hæmatologia, pp. 106, 188, 190, &c. (11) Exp. circa Sang. human. passim. (12) Mém. de l'Acad. de Berlin. t. 7. (13) Trait. de Remot. et Prox. &c. pp. 20. 21, 272, 273; and Philos. Trans. vol. 20, p. 224. (14) Ibid. vol. 22, p. 599. Lancisi's answer to Vieussens. (15) Synopsis; passim. (16) De Œcon. Animal. t. 1, p. 103. t. 2, p. 66, et Præf. (17) Element. Meth. Med. p. 36, &c. (18) Emmenologia De Remed. Viribus et Operat. p. 160—185. Various and circumstantial. (19) Exp. circa Sang. chem. mist. Various. (20) Theat. Anat. t. 2, p. 169. (21) On the Preternatural State of the Animal Fluids, &c. p. 158. 1696. (22) Disquisit. Patholog. p. 178. (23) On the Cure of Diseases by Acids and Alkalies, 1698. Also, Collection of Tracts Chirurg. and Medical. (24) Med. Hippoc. Chym. &c. (25) On Chymical and Galenical Remedies, 1683. (26) Inquiry into the erroneous Philosophy of Alkali and Acid, 1698. (27) De Acido et Alkali, 1675. (28) Pharmacopœia Extemporanea, pp. 65, 101, 210. 1701.

which he adjusted the doses of medicine, at every period of life, "according to the squares of the constitution." (1)

Some of these writers praised, whilst others condemned the saline treatment. The able and learned Hillary, who practised in the West Indies, and was a thorough humoralist, speaks of it after the following manner. "It is *well known* to the judicious, both from observations and experience, that *all alkaline salts*, both volatile and fixed, when taken inwardly and carried into the blood, do both attenuate, *dissolve*, and increase the putrescent diathesis of the blood and animal fluids, which *repeated experiments also confirm*." And again, in another work, "the common saline draughts ought to be ranked among the *lædentia* in yellow fever, however they may be useful in other diseases." (2) Pringle, on the other hand, employed them in "bilious fevers" after the same manner as Dr. Stevens. "The neutral salts," he says, "were given after the evacuations (bloodletting and cathartics) in order to bring the fever sooner to a crisis, &c. (3)

It is but yesterday, since the foregoing experiments were made by humoralists upon *dead* animal matter, with a variety of medicines,—salt among the number, to ascertain their antiseptic powers. The shops, in consequence, were loaded with humoral remedies, to be applied to living organized beings, because they prevented or interrupted decomposition in dead matter. But they were soon swept away by the voice of reason, and the glaring failure of their pretensions. In the very midst of the experiments, Pringle, and others, found that salt, nitre, camphor, wild valerian, &c. preserved meat from putrefaction longer than *Peruvian bark;* but, they were forced to the admission that bark and cob-web would cure their "putrid fevers," whilst the former were perfectly inert. During the reign of alchemy, up to the present time, humoralism has been severely tested by all the ingenuity of its defenders; but amid the multiplicity of remedial agents which have been the offspring of this hypothesis, we know not one whose operation imparts to it the least validity.

In more recent times, but prior to Dr. Stevens, we find various

(1) See Philos. Trans. vols. 24 and 26, pp. 2119, 46; and Balguy's Rectifications of the Table in Edin. Med. Essays, vol. 4, art. 5.

(2) Inquiry into the Method of Improving Med. Knowledge, p. 355; and Account of the Yellow Fever of the West Indies, p. 13, &c.

(3) Pringle on Diseases of the Army, part 3, c. 4, p. 203.

authors recommending the use of alkaline salts, upon one hypothesis or another,—generally the humoral,—as something that has been hitherto untried. Thus, our Dr. Mitchill is prompted to their use by the *septic* theory. He recommends "all such compounds of other bodies, with alkaline bases, as are capable of being decompounded by the septic poison or acid they meet with in the human body, and of furnishing an alkali to unite in such cases with these sour and acrimonious fluids, and thereby to saturate them."(1) Many other similar advocates have appeared in America, of whom we may notice Cathral, Seaman, Vaughn, Grangier, Currie, Hosack, Archer, Harris, Barker, White, Nooth, Mace, Spalding, Banks, &c.(2)

It is worth stating, also, that one of the remedies most lauded for its virtues by the present humoralists, and admired for its novelty, viz. table-salt, was employed and highly extolled by Dr. Wright, more than half a century ago, in treating the "putrid diseases" of Jamaica.(3) Indeed, the history of table-salt is quite unique in the *materia medica*. Dr. Mitchill remarks, that "the discovery of its efficacy has been supposed to be very *recent;* but I remember it is mentioned by Diemobroeck, (l. 3, c. 5, §. 21,) as an excellent *antipestilential* remedy."(4) Diemebroeck commends it in strong terms, and gives a sketch of its descent as a remedy in fevers. Riverius was so prone to the use

(1) New-York Medical Repository, vol. 1, p. 255. 1797.

Mitchill, who is said to have been a philosophical solidist in his practice, then gives a list of 43 neutral salts, (having "omitted many others,") for the cure of fevers. He says, "it is amusing enough, that they, who, in their *conversation*, doubt or deny the truth of the conclusions I have made concerning those sorts of distempers, are all the time in the habit of verifying them *in their practice*, and bearing witness for them daily in their prescriptions." "Some of the skilful and knowing ones tell me, gravely, I am prescribing for a *phantom*, for a creature of my own imagination," &c.

We have always had a reverence for Dr. Mitchill's enthusiasm in the cause of science; but the *knowledge which we possess* in relation to his decoration of one of his portraits, and its engravings, with the *Mitchella repens*, obliges us to say that the name of the genus was intended to immortalize the botanist, Dr. John Mitchell of Virginia, who emigrated from England to this country in 1700, and became distinguished as a physician and a philosopher. Possibly the peculiarity of the *M. repens* having a double flower may have suggested the division of honours.

(2) Ibid. vol. 1, pp. 1, 14, 404. Vol. 2, p. 400. Vol. 4, pp. 85, 150, 157, 162, 241, 415, 416. Vol. 5, pp. 150, 220, 267, 477. Vol. 7, pp. 129, 304. Vol. 8, p. 441. Vol. 9, pp. 100, 248, 337. Vol. 10, p. 140. Vol. 11, p. 63. Vol. 12, pp. 119, 377. Vol. 15, p. 308. Vol. 16, p. 244.

All the foregoing were able practitioners, having no regard to the hypothesis in their treatment of disease.

(3) Duncan's Med. Comment. vol. 11, p. 189.

(4) N. Y. Med. Repository, vol. 5, p. 118. 1801.

of saline draughts in fever that he calls them a "febrifuge." (1) And so Cullen. (2)

If we look back to the earliest ages of medicine, we shall find that neutral salts were amongst the subordinate remedies for various diseases. Hippocrates recommends them, in his writings, in more than fifty places. (3)

There is nothing in any part of the doctrine that can lay the least claim to the charm of novelty, either in the way of experiments, practical results, or hypothesis. The very colour of the blood has been often the basis of important conclusions. (4) Trotter's entire doctrine of the proximate cause of scurvy is founded on the dark colour of the blood, and though not a humoralist, a great object of his treatment is to restore the florid hue. (5)

We certainly concede that the experience of the past affords a guarantee for the future. Shakspeare, who knew much of everything, — past, present, and to come, — says: —

> "The earth has bubbles, as the water hath,
> And *this* is one of them."

But that future may be distant, unless there be efforts to hasten its approach. Credulity is constitutional with the human mind. We say not this, however, of our own knowledge, but on the authority of Lord Bacon, who affirms, "that the master of superstition is the people; and in all superstition, wise men follow fools." It is said, also, by another philosopher, that "a weak theory is more suitable to the indolence of the human mind, than an accurate demonstration." Or, as the *senex divinus* has it, — "facilis a negatio ad otium, quam ab otio ad negotium;" (6) a sentiment of which Armstrong has furnished the translation, "so indolent is the human mind, that it will rest even upon error, rather than be at the trouble of inquiring for itself;" (7) the whole of which is very well summed up by Dr. Stevens, in the trite saying, that, "whimsical theories creep faster into physic than useful facts." (8) As to the facts of this author, as well as

(1) Observat. de Med. cent. 1. ob. 15, 54; c. 2. ob. 66; c. 4. ob. 97.

(2) First Lines, &c. §. 153, &c.

(3) The "Nitre," or "Natron," of the ancients was Carbonate of Soda.

(4) Indeed, there is no end to the fancies which have prevailed upon this subject. Dr. Armstrong, finding the blood *florid* in a case of fever, inquires "whether the colour was the cause or the consequence of the fever." (a)

(5) Observations on the Scurvy, p. 140, &c. (6) De Dieta, l. 3, n. 7.

(7) Lectures on Acute and Chronic Diseases, vol. 2, p. 128.

(8) On the Blood, &c. p. 172.

(a) Lectures, &c. vol. 1, p. 194.

all others, we shall endeavour to treat them honestly; being much of Sancho's opinion, that, "the part of the man which swore truth ought to be allowed to pass, and that which told a lie ought to be hanged." (1) "I will determine them all; they are like these double or turning pictures; stand before which, you see a fair maid, on the one side an ape, on the other an owl; but farther examine, you shall find them wise on the one side, and fools on the other; in most things praiseworthy, in the rest incomparably faulty." "But I dare say no more of, for, with, or against them, because I am liable to their lash, as well as others." (2) "When, however, I behold one, mounted though it be but for a moment beyond the time which my love to my country has prescribed for him, then I cease to be a philosopher, and for the transport of an honest indignation, I wish the *hobby-horse*, with all its fraternity, at the ——." (3)

SECTION V.

ONCE more we approach the costive facts of our subject. We are thankful for the recognition which is bestowed upon Dr. Stevens, or we should be deficient in materials.

We have seen that the *dark* colour of the blood is an important affair in the humoral pathology, and we shall therefore give to it all the grave consideration which a fundamental fact demands. We have seen with what rapidity the blood undergoes the most surprising changes, as during venesection, and from slight variations in vascular action. We come now to consider a not less remarkable change that may arise from momentarily arresting the process of sanguification. This is best demonstrated by diminishing, or arresting, the function of respiration, since we suppose that this experiment will not fall within the pale of humoralism. We need not say, that we speedily obtain by this method as dark a colour of the blood as can be produced in the range of diseases. Still we fear that this very illustration may

(1) Don Quixotte, vol. 4, p. 167.
(2) Burton's Anatomy of Melancholy, vol. 1, pp. 104, 106.
(3) Tristram Shandy, vol. 1, c. 8.

be claimed in behalf of humoralism; that it may be said the blood becomes charged with carbonic acid, in virtue of which the organic functions are subverted. But this is not humoralism. It is solidism, from beginning to end. The change in the blood is wholly the result of a failure in the organic functions to perfect the blood, whilst the very carbon with which it is loaded is the product of vital action. And since the blood is the *pabulum vitæ*, if any modified action of the solids, especially when induced in the foregoing manner under circumstances of health, fail of perfecting the blood, it will naturally cease, in its turn, to perform its office of sustaining the solids. Carbonic acid, being deleterious to the organic forces, will also aggravate their embarrassment in proportion to the extent in which it may be generated. But even this, *per se*, is not at all humoralism. The acid is a poison simply *mixed* with the blood, and acts just as any irritant applied to the skin, only the more so from its universal application to the very seat of the vital forces. Humoralism supposes an absolute deterioration, fermentation, or some such evil, bred in the blood itself, and in entire independence of the solids. A great work, also, is to be performed in this case, before the blood can be reinstated. But in our instance, we have only to let the animal breathe again, and the blood is as immediately restored. Just so it is in all diseases. Just in proportion as we restore the natural action of the solids, so will be the restoration of the blood; and we shall see that, there is no other possible mode of converting a diseased to a healthy state of this fluid. It appears to us, indeed, that we might better undertake to fabricate blood, *de novo*, by an artificial process, than attempt its restoration, when morbidly altered, by chemical or any other agents. But, on the other hand, if the fabrication of blood be purely a vital process, a result of the living actions of the solids and the vital juices, we see not by what sort of philosophy we can undertake its restoration but through the agency of those solids upon which it depends for its being and integrity. Grant this, and there is an end of humoralism. Our remedial agents must then exert their action upon the solids; and, by parity of reason, as will be more extendedly shown, all absolute deteriorations of the blood spring from a primary derangement of the vital actions. Could it be shown that chemical agencies have any connection with the formation of blood, or with its alterations in disease, it must still be admitted that they are wholly

subordinate to the vital forces, and are brought into operation through the instrumentality of the solids, according as their actions may be determined by the vital properties. It is through these properties that healthy actions are maintained, or are supplanted by morbid ones; and all our remedial agents, even where chemical results are contemplated, must be directed by this principle, and without the slightest reference, of a direct nature, to the probabilities of a chemical action upon the blood. To effect this or that alteration in any fluid, we must still so operate upon the vital powers, that a new direction shall be given by those powers to the chemical forces, which we grant, for argument's sake, may act in subordination, and probably contribute in maintaining the combination of elementary principles. (1) If it be intended only to increase the proportions of certain constituents of any fluid, it is manifest that, in those cases, the forces of life are alone instrumental, since the constituents can scarcely be regarded as chemical combinations; for the moment chemical forces should operate, new combinations would be formed. It is plain, therefore, that practical humoralism is not only philosophically wrong, but pregnant with the most destructive consequences.

The foregoing example is more or less applicable, in principle, to all circumstances of the system in which the blood may change from its florid to a darker hue, even admitting that the proximate cause may consist in a diminution of its saline constituents. This, indeed, is the key to the whole philosophy which regards the various changes that the blood undergoes in disease.

Take the asthmatic, as another step in the inquiry. You grant there is nothing humoral in the case; but that the darkness of the blood is mainly owing to its want of oxygen gas, and of proper decarbonization, and that this fault belongs entirely to the solids. Bleed him; and perhaps before the blood has ceased flowing, it has assumed its perfectly natural character, and the patient may be nearly restored to health. The nature of the remedy, its only conceivable mode of operating, and the instantaneousness of the change, co-operate with our former inductive philosophy in proving that the solids had been alone concerned in the morbid and restorative process.

Again, the foregoing *rationale* is applicable to asphyxia from

(1) Still, the elements begin to separate as soon as death takes place.

carbonic acid gas. It is said, however, perhaps, that the gas is absorbed; but we do not believe it, and shall endeavour to substantiate this belief. It is an office of the lungs to excrete, not to absorb, this deleterious agent. The philosophy of physiology, therefore, is opposed to the construction, as well as facts. The very nature of this gas, more than anything else, should retard the pulmonary excretion of the same substance. This example, then, affords an illustration of the analogous mode in which a poisonous gas affects the vital properties of the solids when operating within, and from without. It accumulates to a vast extent within the blood from its constant generation by the solids, and from its defective excretion. On the other hand, what is external inflicts a great violence upon the vital actions of the lungs, and this deleterious impression is propagated by sympathy over the whole body. The blood, in consequence, becomes more and more deteriorated, and the progressive generation of carbonic acid gas from within alike contributes to the functional disturbance, and disqualifies the solids for appropriating the blood to its important uses; whilst the blood itself becomes more or less unfitted for its office. We had nearly forgotten to claim in our behalf, that the blood is very "black" in these cases. But get the patient into fresh air, — how is it then? We consider this the strongest example which humoralism can offer; yet the deterioration of the blood arises from direct privation of a natural source of supply, and from a primary failure of organic functions; whilst it can be in no respect restored but through the agency of the solids.

Take we a disease of reputedly humoral origin, such as the intermittent fever. What are the evidences of a fermenting or other diseased state of the blood? "It is black, black." Mainly so, however, only during the cold stage. When the stage of reaction comes, even the venous blood often acquires a highly florid arterial hue. The doctrine, then, as to the loss of saline constituents, and the morbific nature of the blood as inferred from its colour, are wholly untenable, as we shall see it to be equally so in typhus, yellow, and other fevers. The plain philosophy appears to us to be this: The morbific agent has so altered the action of the solids, that in certain conditions of the disease which are constantly varying, less oxygen is consumed, and more carbon is generated than is carried off, or less is excreted than natural. The extreme capillaries are all involved in the morbid

process, and as their action varies from one hour to another, or as remedial agents may affect their vital forces, so will the hue of the blood constantly vary. It may, therefore, pass from one extreme to the other in the course of an hour. Now is the stage of collapse, and the blood is "black," but "every fibre is diseased." Anon the hot stage supervenes, and then we have a crimson colour. This is true of the celebrated humoral fevers of the West Indies. We state it on authority; for, thus, Dr. Stevens: "In the hot stage, the blood becomes more red, and in some cases it is even florid *for a time*." With this well known fact, what, then, becomes of the corrupted blood, or the poison, or the absent salts which had so blackened the vital fluid during the stage of collapse? Or why does the dark colour return again?

But let us wait not for the natural process. Bleed the patient when shivering with cold, and his blood is "freezing," as the humoralists have it. The blood trickles from the arm, black as your hat. Soon, however, the action of the extreme vessels becomes modified, — they expand, — the circulation becomes freer, — the blood is more extensively applied to the air, — the excretory vessels are every where suddenly called into action, and what was but a moment before "black," has now become red. Carry the operation still farther, and the blood begins, again, to lose its florid hue, and may be quite dark when syncope takes place. Still we have not the least doubt that the natural condition of the blood is altered in this disease, as it is more or less so in all constitutional fevers, and in very many local affections. It would be unphilosophical to suppose it otherwise. But we shall never reach the difficulty, as we shall farther endeavour to show, by agents addressed to the blood itself. The proofs, also, of its disease are exceedingly obscure and ambiguous, as must be manifest from what we have said of the astonishing changes which are produced in its sensible appearances during the operation of bloodletting, by running to death, by mental emotions, or by various causes which appear to produce only an accelerated motion of the circulatory organs. We infer the radical lesion on philosophical grounds, and from the analogies which we have just stated, as well also from changes in its appearance, in acute inflammation, after coagulation, &c. We infer it philosophically because it is fabricated, if we may use the word, by the solids; and, therefore, when the healthy actions

become disturbed, the blood can no longer be perfected, whilst a pernicious influence must be also exerted upon it.

In the malignant cholera, the dark colour of the blood is in proportion to the severity of disease, and the intensity and duration of the collapse. The *rationale* is the same as in the foregoing cases. But suppose a part of the darkness to be owing to a loss of the saline constituents, — does this prove that a similar loss is sustained in common congestive fevers, where the blood may be equally dark, but where all the secretions have been arrested from the beginning; or, does it prove that the phenomenon, in the latter instance, is in the slightest degree owing to such a cause? We know that there are some minds that reason in this manner; and we must, therefore, put this sort of philosophy down by matters of fact. In the first place, then, we, and others, have seen the blood of cholera subjects who have lost none of its constituents either by excretions from the bowels, or kidneys, or skin, as "black" as when torrents of fluid have run off by the bowels and skin. This is exactly coincident with what happens in the worst forms of congestive fever, those "putrid" diseases of the humoral pathology, where the secretions have been locked up from the very incubation of disease, and where, beyond all question, there is actually a redundancy of saline matter in the blood. And yet humoralism, to carry its point, after assuming the dark colour, broadly affirms that "the dark colour of the blood, in the *beginning* of pestilential fevers is *the effect of the poison* on the vital fluid; but the *blackness* in the last stage of these diseases is produced by the loss of the *saline ingredients;*" though, as a *general rule*, "the blood owes its dark colour in the venous circulation to the presence of *carbonic acid.*" (1) Again, we shall see it affirmed by the same writer, and adopted from him into the humoral pathology, that even in the early stage of fevers, of which intermittents are often adduced as examples, that the dark colour is owing to a loss of the saline constituents of the blood; and it is upon this principle, that Dr. Latham thinks it not improbable we may reach the "seminal principle of the worst forms of fever by table-salt," and, by others, that "the obvious indication is to inject a solution of salt into the veins of cholera subjects." This doctrine is now generally maintained, so far as our information extends, notwithstanding the secretions are more or less arrested in all fevers, and the

(1) Stevens on the Blood, &c. pp. 103, 444.

more malignant, the more so. In these cases it is said that the morbific "poisons kill the salts."(1) It is maintained also, in defiance of the fact, that in these very diseases, the natural colour is restored by abstractions of blood, by cathartics of calomel, jalap, croton oil, tartarized antimony, and such means as establish the secretions most freely, and thus carry off, without at all adding to, the saline constituents. This is humoral logic. We hold Dr. Stevens responsible for it. The assumptions which we have just stated grew out of the obstacle to one hypothesis which exists in the presence of the salts at the early stages of the disease, which is affirmed at one time, but denied at another; whilst it settles the question both as to the agency of the foreign poisons, in imparting the dark colour at the early stage, and the saline method of treatment; although, as a general rule, "the blood owes its dark colour in the venous circulation to the presence of carbonic acid." Again, it is often affirmed by our author, that "to the saline matter, (of the blood,) the fibrin, &c. owe their fluidity, for the fibrin remains fluid only while dissolved in the saline fluid," &c. "With the exception of the colouring matter, the whole of the solid ingredients of the blood, as I believe, owe their fluidity to the circumstance of their being held in solution by a saline fluid." But mark the somerset. "When we examine the body in such cases, (the West India fevers,) after death, we find the heart flabby in its consistence, and crammed with a *thin*, black, half-putrid fluid, that can only be distinguished from the black vomit by a chemical test." "And we know that its black colour, in the last stage, as well as its *other diseased properties*, are produced chiefly by the *loss*, or, at least, by the *great diminution* of those *natural saline* ingredients which are so essentially necessary to the healthy action of the vital fluid," &c.; (2) and again, "saline medicines, we know, possess the power of preventing a dissolved state of the blood." Thus, in one case, the salts of the blood are the cause of its fluidity, and in the other, the loss of them. "In medicina decantantur tenebræ, et

(1) Cullen thought that the dark colour of the blood is owing to a redundancy of the salts.

(2) On the Blood, &c pp. 6, 283, 289, 362, 449, &c. There is a most embarrassing disagreement among the humoralists, as to the agency of the salts. Thus, Mr. Jennings thinks, in direct opposition to Stevens, that the blood is rendered "permanently fluid" in the scurvy "by an *excess of salt*;" and that this "is carried into the blood from the continued use of salted provisions," (*a*)—albeit no salt have been used, for weeks or months antecedently. Can any thing more clearly demonstrate the hypothetical nature of the humoral pathology?

(*a*) On the Chemistry of the Blood, Op. Cit. p. 79.

noctuæ philosophantur Athenis." That is to say, according to Paracelsus, philosophy can only be taught in high Dutch. Or, as Duretus has it, "fremant licet omnes, dicam tamen, quod sentio, majorem scientiæ, et praxeos ubertatem comparari a studioso Hippocratis uno die, quam ab istis pragmaticis uno seculo." (1) Nor do we mean this in the least a reflection on those who have, in our belief, adopted Dr. Stevens' facts and philosophy without examination. We do not believe that there is one intelligent physiologist who would tolerate this work after a critical perusal. Still we think, that our author's affirmation that "Mr. Hunter's book on the Blood has done less good, or, perhaps, more evil than any other that has been published during the last hundred years, unless we except Cullen's First Lines, or the Pathological Writings of M. Broussais," should have raised suspicion, and have excited an honest indignation in the minds of all liberal inquirers. It is a reckless and general denunciation of great philosophers, touches no specific point, but consigns the whole to unqualified censure, and for the manifest purpose of putting them out of the way of the most stupendous tissue of physiological absurdities which has fallen under our observation. In any event we do but exercise the right, in common with Dr. Stevens, though we hope with a greater show of reason, of inquiring into the pretensions of one who undertakes to instruct the world in matters of science, and especially when this instruction is thankfully received, and nearly concerns the health, and happiness, and life of man. We shall find no fault, should we be equally tried and condemned, — so only it be a fair exposition of our errors, or our ignorance.

Dr. Craigie is inclined to be charitable towards our author, and in respect "to the chemical and mechanical" doctrines, to think his hypothesis "the most tangible and intelligible on this subject," viz. "that in yellow fever, as in other fevers, the blood is deprived of its saline impregnation, and especially of its alkaline properties, and, in consequence, becomes darker coloured, less fluid, and less fitted for the purposes which that fluid performs in the economy." But Dr. Craigie assigns no reason for the "intelligible and tangible" nature of our author's hypothesis; whilst he expressly states, that "if there be a difference in the saline constitution of the blood, it is rather *augmented* than diminished in the regions in which yellow fever prevails." (2) It

(1) In Coacas Hip. p. 267, (2) Craigie's Prac. of Physick, pp. 248, 249.

is manifest that Dr. Craigie has no confidence in the hypothesis; but that he regards it as one of the "fancies" to which he had just before adverted. It can, therefore, be neither "intelligible" nor "tangible." On the other hand, numerous authors affirm positively that Dr. Stevens has settled, or nearly so, the whole of this question.

It is by admitting such assumptions, that "all subsequent reasoning appears so demonstrative as to make us forget where we set off." "Give but the liberty of asking what we please, and answering as we like, proof of anything may be made." Like the *fuga vacui*, certain facts are brought forward, and, without being any better accounted for, are laid down as a foundation for humoralism. It thus becomes the easiest matter in the world to believe our author's statement, that the poison of the rattle-snake, and all others that are detrimental to health, blacken the blood, and annihilate the salts. Lord Bolingbroke, like other reflecting men, considers one error a step that leads to another, and so on till we have wholly lost sight of the truth. If the first step be false, the conclusions must be equally so, however just our reasoning.

Dr. Elliotson thinks "it worth remembering, as Dr. Stevens has pointed out, that blood may be black from the presence of carbonic acid, &c. or from the absence of saline matter."(1) It is true, Dr. Stevens affirms that "I can prove beyond all question, that the saline ingredients are the true cause of the red colour of the blood;" and it is upon this assumption, that the induction is founded that the "black" colour of the blood is owing to the loss of the saline matter, whenever it may best suit the hypothesis. So "it is worth remembering," that Dr. S. has not shown that the "black" colour is ever owing to an absence of the salts; and that he has only shown, what has been long known, that venous blood becomes more florid by the addition of salts, and, in a general sense, the principle is of some interest; but there is great reason to believe, as we have said, that the natural colour of the blood depends much upon other causes than have been assigned. Müller states that he has ascertained that no carbonic acid is evolved by mixing salts with venous blood; nor does it change its colour under an air pump; whilst sugar and oxygen gas, alike convert the venous to an arterial hue. It appears to have been well ascertained by Dr. Sunderland, that

(1) Human Physiology, part 1. p. 226.

there is at least no excess of *free* carbonic acid in the blood of the patients affected with adynamic fevers; and Bichat, (1) who discoursed so much, and so well, upon *black* blood, confesses that he knows not the cause of this phenomenon, and he does not think it "capable of any positive experiment." The experiment with salts, therefore, proves nothing, except as to the abstract fact. We cannot understand the true cause of the blood's colour. It is sometimes white, when respiration is unimpaired. The causes are probably numerous, and we may be sure that they are essentially of a vital nature, as they are in the various hues of the vegetable kingdom.

But, admitting that the dark colour of the blood is owing to the loss of its salts; the fact is a direct proof of the doctrine of solidism, since they have been either eliminated by an antecedent derangement of the solids, or imperfectly supplied in consequence of a failure in the chylopoietic organs. By parity of reason, every other change in the relative proportion of the constituents of the blood, and every other alteration, should be resolved upon the same principles.

Again, if the deterioration of the blood depend upon miasma in the first instance, and the loss of the salts be only a sequel, the restoration of the salts only leaves the patient where he began. What is to renovate the blood? How shall we restore the salts when that morbid condition exists from which their loss resulted? Are the solids in any better state to assume a healthy action, than they were to go on with a healthy action before the diseased state of the blood took place, or the salts were lost?

Suppose however, that the blood, instead of being "black" in malignant fevers, were generally *white*, (as, indeed, is related by Morgagni, and others, of some of these very fevers,) what would have been the fate of solidism? Its voice would have been completely stifled. It would have succumbed before such a phenomenon. The whole force of physiology would have been brought against it in vain. And yet blood of a milky whiteness has been repeatedly abstracted where there was no remarkable deviation from health.

What especially shows the susceptibility of the blood to rapid changes from slight modifications of vital action, how greatly it may deviate from its natural aspect, how peculiar and various may be those modifications of action, and how impossible it is

(1) General Anatomy, vol. 2. p. 42.

to reason from such observations, are cases like the following: "A maid, after eating a good breakfast about 7 o'clock A. M., was let blood about 11 the same day. The *first* blood was received in a porringer, and within a little while it turned *very white;* the last blood was received into a saucer, which turned white *immediately*, like the white of a custard." "This maid was then in good health, and only let blood, because she never had her courses, yet of a very florid, clear, complexion." (1) Shall we not, therefore, be more entitled to the import of this absolute proof, than the humoralists, in the other case, to an accidental and constantly fluctuating result?

"In those fevers which arise from marsh miasmata or from contagion," says Dr. Babbington, on the authority of Dr. Stevens, "when first drawn, the blood has a peculiar *smell*, and coagulates almost invariably without any crust." We shall soon show, as also when speaking of scurvy, and in our Essay on Venous Congestion, that the last affirmation is entirely unfounded. Still, if there were no crust, the blood would then be the more natural in this respect. As to the "smell" of the blood, there are associations with this indefinite word which have given it a strangely factitious importance in its relation to this inquiry. We find that several of the former humoralists, Fernelius, (2) Vander Mye, (3) Pringle, (4) and Morton, (5) have noticed the smell of the blood in proof of humoralism. But is it not a little remarkable, that after the explosion of the humoral pathology, this "peculiar smell" should have been entirely lost sight of? That none should have observed it as peculiar to disease, but humoralists alone? Admitting, however, its existence, does it prove a primary disease of the blood? Certainly not. It proves that facts are appropriated to the humoral hypothesis without a philosophical investigation.

The facts and philosophy are briefly these: It is stated by numerous observers that perfectly healthy blood has a "peculiar smell." Thus, Dr. Lind: "Blood fresh drawn from persons in health affects the organs both of smell and taste with *sensations*

(1) Dr. Lower, in Philos. Trans. Lon. vol. 1. p. 117.

(2) De Febribus, c. 5. p. 246. (3) De Morb. Bredanis, p. 14.

(4) On Jail Fever, p. 337.

(5) Pyretolog. p. 26. Chaptal goes so so far as to say that "the vapors" of humoral blood converted chalk into calcareous nitre; (*a*) and Dr. Pascalis states that the "aqueous gas formed by the blood, is sometimes very inflammable." (*b*)

(*a*) Elements, &c. Art. Soda. (*b*) New-York Med. Repos. vol. 1. p. 565.

not easily to be described." (1) So, also, Magendie, (2) Tiedemann, (3) and Johnson, (4) who says the smell is urinous, in which Dumas (5) also agrees. This sense, we are aware, is but little to be trusted; but where it so generally concurs in its indications, we may safely take its report.

Here, then, we have the whole of this matter referred directly to the agency of the solids. If the *smell* be in any respect modified in disease, all philosophy teaches us that it depends upon the same cause which imparts the constitutional odour to the blood. It is a natural induction, that if the solids in their healthy state endow the blood with a "peculiar smell," they will modify this smell, as they do other conditions of the blood, when their action is altered by disease; though, as we have stated, there is reason to believe that the modification is not appreciable. This is also exactly coincident with the variations of smell which more or less attend the recent secretions in all diseases, and which, it would be absurd to deny, are determined by the modified action of the excretory organs. Hence we have a variety of proof, that this very fact, which is alleged in favour of humoralism, corroborates the doctrine of the vitalist.

It is an important fact in humoral pathology, that the blood is often fluid in adynamic fevers; whether abstracted from the arm, or after death; and it has been even assumed as a proof of contagion. (6) This affair is an old favourite. It is now

(1) On the Scurvy, p. 513.
(2) Physiology, p. 278.
(3) Comparative Physiology, p. 142.
(4) Animal Chemistry, vol. 1. p. 24.
(5) Principes de Physiol. t. 2. p. 33.

(6) Amongst Dr. Stevens' most mistaken assertions, is that which would propagate the belief that the yellow fever, or the "African typhus," as he calls it, in these United States, without "one solitary exception," has been traced to importation." (Ut. Cit. p. 195.) Exactly the opposite of this has been long well known and admitted. Indeed, we do not believe that there are, at this day, twenty contagionists amongst the entire faculty of North America, as it respects yellow fever. The question was so far settled in this country before Dr. Stevens wrote his book. It was said long since by Dr. Miller, that "the fabrications and prejudices, connected with the importation of yellow fever into the United States, have been so often exposed, that repetition has long ago grown weary of exposing them." (N. Y. Med. Repos. 1810, p. 53.) And yet our author states that he made "a minute inquiry into this subject." But here our author might have enjoyed the advantage of our recorded facts and opinions. Not so, however, as to those statements which he founded upon experiments in the State of New-York. (a) (See p. 431.) And here we refer the reader to a very severe, but just, commentary, by Moseley, upon this practice of hunting up dis-

(a) We neglected referring in its proper place, (p. 432,) to the important proof supplied by Lancisi, that marshes, having a mixture of salt and fresh water, are, of all the most productive of fevers. He says that Pliny affirms the same of "saline exhalations," and that "we must here take experience and not speculation, for our guide." — *De Noxiis Paludum Effluviis*, c. 5.

held that, "the black and *dissolved* state of the blood is, in reality, in fever, the true cause of the nervous, as well as the other bad symptoms." (1) It is a sufficient objection, however,

eases, "in travelling post through a country, in order to convert them to the support of some new hypothesis." — *See Tropical Diseases*, pp. 134, 136.

We shall add a few words upon the general principles which relate to this expiring crusade against the philosophy of nature. It is intimately *allied* to the subject of our text. We cannot, however, do it so well as in the language of one who wrote in the midst of the battle, and who planted his standard on the watch-tower of physiology. "In the books of physic, and in the lectures of its professors," says Dr. Miller, "it has been fashionable to call the virus that is generated among the particles of filth, that cover the bodies, clothes, beds, and apartments of the poor, *human* contagion, and its exhalations, *human* miasmata. As well might the contents of a jakes be called *contagion*, or the reekings of a dung-hill, *miasmata*. It is a poison formed by intestine workings among the elementary ingredients of excreted and inanimate matter, and as distinct from the vascular energy by which contagion is elaborated, as death is from life. There is not an atom of contagion in the case; for contagion, like other excreted fluids, is not imitable by any *artificial* process, and cannot, any more than bile, be prepared in a laboratory by synthesis." (*a*)

If there be any dependence upon the laws of nature, we may safely confide in the foregoing doctrine. The laws, and actions, and products, of living matter, as we think we have incontrovertibly proved, as well by the facts of our opponents, as by our own, are absolutely distinct from those of inorganic matter. It follows, therefore, as a consequence which admits of no question, that the decomposition of dead matter cannot result in the specific products that are generated by living organized matter, and *vice versa*. There are, also, some remarkable coincidences amongst diseases that are admitted to be contagious, and not applicable to other affections, which appear to denote the operation of specific laws; such as their regular progress and termination, — their affecting us but once, &c.

The "doctrine of contagion," as it respects epidemics, is an offspring of humoralism. It was never imagined till the time of Galen; and his crude suggestions were variously embellished, till Fracastorius (*b*) reduced them to a more artificial method. This had only the effect of turning physicians to a better observation of nature; and the subject was beginning to lose its attractions when Mead took it up, and gave to it a new impulse. But here, again, a great reaction followed. The sun of solidism, which had set with Galen, was now beginning to reappear; and as philosophers abandoned their speculations, and were again conducted by the phenomena of nature, the dream of febrile contagion had nearly vanished with its parent night-mare. But that genius of night is again inspiriting the illusion.

The Roman poet, probably taking counsel of Hippocrates, laid down the whole philosophy as to the source of malignant fevers. In speaking of the plague of Athens, he says that, "these diseases proceed either from the air, or from the earth,

—— ubi putrorem humida nacta est
Intempestivis pluviisque, et solibus icta." (*c*)

And Virgil, thus:

"—— nemorum quæ maxima sacro,
Fonte sonat, sævamque exhalat opaca mephitim." (*d*)

(1) Dr. Stevens, Ut Cit. p. 449, and many others.

(*a*) Dr. Miller, in American Med. Repository, vol. 5, p. 187. We employ the 2nd Ed.
(*b*) Op. l. 3, de Contagione et Morb. Contag.
(*c*) Lucretius, de Re. Nat. L. 4, v. 1090. (*d*) Æn. 7, v. 83.

to the humoral part of the hypothesis, that the coagulability of the blood is not destroyed, till the strongest manifestations of disease have taken place in the solids, nor is it by any means uniform in the worst cases either of fever or scurvy. On the contrary, in the former affections, "at the commencement, when the lancet is generally employed, the blood drawn does not differ materially from that *taken out of* the *healthy system*. It separates into its constituent parts, and presents frequently, we might say generally, an inflamed surface; but, it is principally as *the fever advances*, as the whole body becomes more relaxed, and disposed to putridity, (?) and as the energies of the nervous system sink, that this fluid assumes a loose, dissolved, and peculiar aspect." (1) This statement is not made in the calculating spirit of a partisan. It is founded on the universal testimony of the best observers; and being thus founded, it is utterly fatal to the humoral hypothesis as it respects "adynamic" fevers. But, so much emphasis is laid by the humoralist upon the dark col-

Hippocrates, (*a*) scouted the idea of contagion, and Galen, at most, but speculated upon it. The latter refers the plague to a peculiar constitution of the air. (*b*) And so Celsus. (*c*) Such, too, was the opinion of Rhazis, and Avicenna, and other Arabian physicians; as it was, also, of Aretæus, and Trallian. They never speak of contagion in relation to pestilential epidemics. "Contagion," says Moseley, "was unknown to the ancients, who studied nature, and, consequently, thought correctly." (*d*) We have not doubted, however, that in modern times, even medical philosophers have often found it necessary to yield to the torrent of public prejudice upon this subject, and to aid their superstitious sacrifices. When, more than a century ago, Verney, Chicoineau, Soulier, and others, undertook to put down the popular notion as to the contagion of plague, they were accused of having contributed to its desolation at Marseilles. Others, seeing that the public will not be served, have either been silent, or have taken advantage of its credulity.

But it is not alone the crippling restraints of quarantine regulations, military cordons, &c., which the public thus blindly imposes upon itself. The evil touches more deeply than the purse, or personal convenience. We may state our meaning by the following extract. "The positive rules of treatment," says Dr. Williams, "are, not to anticipate any thing in fever, but to await the occurrence of each particular symptom; and inflammation being set up, to remember that it bears a specific character, and that we might as well attempt to stop the smallpox eruption, as to impede its course." His practice, therefore, in typhus fever, consists almost entirely of enemata composed of barley-water and syrup of poppies. (*e*) We shall present a statement of some facts in relation to this subject in Sec. 10.

(1) Lon. Med. Chir. Rev. of Dr. Burne on Adynamic Fever.

(*a*) Epidem. Lib. 2, 3, &c. (*b*) De Temper. l. 1, c. 4, and Comment. in Epidem. l. 3.

(*c*) De Med. pp. 40, 41. See, also, Lancisi's luminous work on this subject, de Noxiis Paludum Effluviis.

(*d*) Letter to Dr. Mitchill, 1803. It is remarkable that the truly philosophical Moseley designed this letter to draw Mitchill's attention to his speculations about the morbific influence of the moon.

(*e*) Elements of Medicine, pp. 83, 88.

our, and dissolved state of the blood, especially in the early stages of fever, &c., that we must be full with our facts, however tedious.

"Nothing," says Pinel, "is so variable as the blood drawn from individuals affected by fevers *called putrid*. It is sometimes like that from a healthy man; sometimes it is pale, sometimes of a deep colour. It coagulates quickly, or not at all. Sometimes it is covered with a buff as in pleurisy. Déyeux and Parmentier did not perceive any remarkable difference from that which is drawn in inflammations." (1)

The observations of Hillary, a profound humoralist, upon the West India fevers, will not be rejected. He affirms, that in their worst forms, "the blood is often of an exceeding florid-red color." (2) Dr. Rush, also, says in a note to his edition of Hillary, that the same is true of the blood in the yellow fever of Philadelphia.

And so Moseley: "Perhaps the *position*, that sizy blood occurs oftener than otherwise, in hot climates, is nearer the truth." (3) And so, R. Jackson. (4)

"To investigate the nature of the fever from the appearance of the blood," says the philosopher Moore, "is equally uncertain and inexpedient; because at the beginning even of putrid fever, it shows some share of inflammatory buff." (5)

De Haen has seen the inflammatory buff as late as the twelfth day in "putrid" and petechial fevers. "Inde scilicet constitit in febribus malignis, in petechiis, in malignissimis variolis, sæpius sanguinem de vena secta prodiisse sive concretum, sive inflammatorium." (6)

"From the presence of petechiæ or vibices in fever," says Mills, "the putrescency of the fluids is usually inferred. But we learn from experience, that the blood is commonly observed here, as in other inflammatory disorders, cupped, buffed, or tenacious." (7)

"In typhoid or asthenic fever, complicated with local inflam-

(1) Nosog. Philos. t. 1, p. 195. (2) Diseases peculiar to the West Indies, p. 4.

(3) On Tropical Diseases, p. 101. (5) Medical Sketches, p. 247.

(4) Jackson says the blood was "dissolved" *after death* only in the worst forms of the West India fevers; which he calls the "gangrenous temperament," on account of the *great disorganization*. Jackson, whose almost only mistake was that of being a febrile contagionist, and, of consequence, a little inclined to humoralism, was too much a lover of facts to sustain the latter doctrine by an appeal to the blood. "*We cannot define*," he says, "*what is not visible, and there is no use in forming conjecture.*" He then allows that "the blood, as drawn from a vein and permitted to cool, separates into different parts, the serum abundant or deficient as may be, the *taste* and colour peculiar, but undefinable in words." This is all the proof that he attempts in favour of humoralism, excepting what belongs, *prima facie*, to the solidist; viz. "the aspect is dry and withered, the habit irritable, the feelings irksome, itchings, eruptions, and acrid excretions." After this, he appears not to have glanced at the humoral pathology, but writes as a philosophical solidist. (On Febrile Diseases, vol. 1, pp. 52, 53, 86, 94.)

(6) De Haen, Rat. Med. t. 3, p. 252, &c.

(7) Mills on the Utility of Bloodletting in Fever, p. 6.

mation," says Morgan, "the blood either remains natural, or, when it has coagulated, resembles a mass of dark jelly. In some of these cases, as the fever assumes more of the sthenic variety, the blood shows a tendency to form the buffy coat; a fact which, we think, forcibly points out the *vital relation* subsisting between the vessels and the fluid which moves them." (1)

Mr. Malcolmson, who bled freely in beriberi, states that the blood was "natural," or "buffed," in the favourable cases; but "generally fluid in the fatal cases." (2) Mr. Hamilton (3) found the blood buffed in this disease, as did, also, Colquhun, Christie, and others.

When we look into the writings of the best observers, and who have no hypothesis in view, we are even astonished, considering the complete subjection of the blood to the actions of the solid parts, that it does not more constantly present greater evidences of an altered condition in bad forms of constitutional disease. We are told by Mr. Evans, one of the latest authorities, that, in twenty-six fatal cases of tropical fever, he found "a change in the vital and physical constitution of the blood in only eleven." And as to the pretended fluidity of the blood, in eleven Europeans recently arrived "the blood was dissolved in *one*,"—and this appears to have been an uncommonly malignant case.

We shall ultimately see that the whole of the foregoing statement is true of scurvy,—the great pillar of humoralism; as it is equally so of purpura hemorrhagica. (4)

Dr. Rush remarks that "I have seen this dissolved state of the blood several times in pleurisy," (5) where we suppose it will be conceded to depend on the action of the solids. But if it will not, then we will go back to the cases of apoplexy, organic affections of the heart, over-driving, &c., which we have already stated. We draw upon the analogy as sound, philosophical, and especially important in medical science. And we may offer this peculiar state of the blood, in other ways, as a proof that the alteration depends on the action of the solids. Thus, it is stated by the writer last quoted, that, "blood drawn from an artery, (in fevers,) has been found to be what is called dense, at a time when that which was drawn from a vein, in the same persons, was dissolved." (6) Here, then, it is manifest, that the arteries imparted the coagulating property to the blood which it lost in the veins.

(1) Principles of Surgery, p. 79. (2) On Beriberi, pp. 128, 119, *note*, &c.
(3) Hamilton in Edin. Med. Chir. Trans. vol. 2, p. 22, &c.
(4) See Facts in Relation to Purpura, in our Article on Venous Congestion.
(5) Medical Inquiries and Observations, vol. 4. p. 326. (6) Ibid. p. 339.

Again, it is said by the humoralists, that the blood being dissolved in adynamic fevers runs out of the putrescent solids as water out of a sieve: and, what improves the elegance of this philosophy, the phenomenon is put forth as a proof of the primary disease of the blood. But a single fact like the following appears to us to settle the construction in a different manner. Thus, it is stated by Bartholine, that a malignant tertian fever, at Copenhagen, "was accompanied with petechiæ, which *came out in the paroxysms*, and *disappeared in the remissions*." (1) We have related a remarkable case of purpura hemorrhagica, showing, as in the foregoing instance, the direct agency of vascular action; the extravasation having taken place, in different parts, on each side of the body in a perfectly symmetrical manner. (2) In a very malignant and fatal epidemic at Minorca, Cleghorn states that "the blood frequently changed its appearance in the course of a few hours in the same person; — presenting a buff in the morning and none in the evening, and vice versa." (3) What was the cause of these alternate changes? Moseley says, "I scarcely remember to have seen a fever accompanied with petechial, or purple spots, in the West Indies, where the circulation had not been forced with cordials and hot regimen." (4)

Exactly in the foregoing manner does common philosophy teach us to interpret all other alterations of the blood which may turn up during the progress of fevers, or of other diseases. And, as a general principle, in proportion as the blood appears to depart most from its natural character, there has been an antecedent and corresponding, or a greater disease of the solids. Better might the vitalists contend that typhus fever is owing to one of its occasional consequences, the alteration of the glands of Peyer, than the chemicals ascribe this and other lesions, or the constitutional affection, to a diseased state of the blood.

Prior to Bichat's time, it was a favourite dogma with the humoralists that the morbific blood was concocted by the liver. Whereupon this reformer remarks, that "physicians have hazarded opinions upon the influence of the black abdominal blood in diseases. If we would express by it the frequency of affections of the liver, it is without doubt just; but if it be employed to express the influence of the portal blood in diseases, it is

(1) Hist. Anat. Rar. Cent. 11. His. 1. 6. (2) See Essay on Venous Congestion.
(3) On the Epidem. Dis. of Minorca, p. 157. (4) On Tropical Diseases, p. 101.

vague and does not rest upon any positive fact." (1) Now, if Bichat could have had any prejudices in regard to "black" blood, they should have inclined to the humoral pathology; since his great work on "Life and Death" is partly intended to demonstrate the bad effects of "black blood" in another sense.

Again, we, and others, even humoralists, have often bled patients in congestive fevers, immediately after they had drenched themselves with saline cathartics, and have constantly found the blood as dark, or, to use the humoral epithet, as "black" as in cases where no means of cure had been employed. But, in these very cases, where the blood has been "black" at the beginning of its abstraction, it has often assumed a florid hue before the operation was over.

Our own philosophy, however, teaches us that the appearances of the blood in disease should not be neglected. They may afford some knowledge, though comparatively little, of the pathological condition of the solids upon which they depend. "Color, consistentia, aliæque educti sanguinis affectiones, subindicare nobis paterunt latentem causæ proximæ constitutionem." (2)

We have now stated the most important considerations upon which humoralism reposes; whilst solidism offers a vast amount of precise and various evidence from which there can be no appeal. It is true, there are minor circumstances which are taken into account by the humoral pathology. These we shall fairly and fully state as we go along, and shall endeavour as fully to show that they are equally without foundation; or, at least, if they have an existence, their true interpretation is exactly against the humoral pathology.

"It is always to the great disadvantage of the sick," says Zimmerman, "that we deduce the first ideas of a disease from its essence or character. We every day hear of muriatic, or inspissated, or corrupted blood, without, however, seeing any proof of it. It is, nevertheless, from these arbitrary principles that the generality of practitioners judge every day of the phenomena of disease, and that they establish their indications and methods of cure." (3)

Sprengell, humoralist as he was in speculation, remarks, that "it has been the folly of a great many, upon dissecting a corpse,

(1) General Anatomy, &c., vol. 1. p. 446.
(2) Baglivi Prax. Med. l. 2. de Causa proxima.
(3) Zimmerman on Experience in Physick, vol. 1. p. 191.

to take the part that was most affected to have been the cause of the person's death, when it was only the consequence of a disease which might have been cured if the original cause had been at the first considered. Thus, dropsy is invariably ascribed to a disease of the *liver* by some, and by others to the *brain*, *stomach*, *mesentery*, *spleen*, *kidneys*, &c.; because they saw some one of these parts mostly affected. And so, again, a fever is generally ascribed to a dry thick blood, because they find none but such in the veins; whereas these *accidents* are often produced by the greater or less violence of the disease." (1) How remarkably applicable is all this to our own times.

We had intended to have reserved for our examination of the pathology of scurvy the following observations upon the supposed putridity of diseases. But that affection being now comparatively rare, whilst the foregoing are the great scourges of the human race, we shall offer them here, as preliminary to our more analytical consideration of the humoral pathology. This particular inquiry we shall submit, at present, to the actual test of experience; and we shall ultimately see that the most enlightened observation is confirmed by the soundest laws in physiology. Nor should the important facts which we have just stated as to the blood, in fevers denominated putrid, be neglected.

De Haen, in controverting Pringle's opinion of putridity of the blood in scurvy, says, "in tot morbis putridis, etiam conclamatis, dum sanguis vena tussa eductus, aut aliunde affluens, examinatus est, nunquam, ne in pessimis quidem variolis, *putridinis signa* vel minima mihi dedit." (2) When we speak more particularly of the scurvy, we shall refer to many concurring authorities.

Bancroft remarks, that "they who assert that the fevers of hot climates are usually putrid, seem either not to have been aware of the violence, and exhausting nature of the symptoms, which precede the appearances of putridity, or not to have been acquainted with the true causes of those appearances." (3)

Chisholm, the great source of error as it respects the contagiousness of yellow fever, in speaking of the "malignant pestilential fever of the West Indies," observes, that "this theoretical notion (putridity) has been the bane of thousands; but the blood of

(1) Sprengell's Comment. on Aphorisms of Celsus, sec. 9, a. 9.
(2) De Haen, Rat. Medend. t. 2, p. 440.
(3) Bancroft on Yellow Fever, p. 60.

tens of thousands cannot efface the fatal impression which theory and deep-rooted prejudices have made." (1)

"How insidious and deceitful," says Wendelstadt, in describing a camp fever at Wetzlar, "the sudden failure of every power! How easy to imagine we look upon a putrid fever, — a disease that is scarcely to be found in nature. Here we have nothing but the consequences of a cerebral lesion, depending upon inflammation, or a vascular injection of the brain." He then goes on to animadvert upon the "bark and wine treatment," comparing it to "oil poured into the fire." (2)

"Vulgus medicorum, et vulgus hominum eas vocant malignas febres, quæ variis, gravissimisque stipantur symptomatibus, ruuntque in deterius. Hoc imaginarium malignitatis nomen imperitia medicorum peperit, petulantia vulgus fovet. Credit enim ignarum vulgus, quouscumque rapit febris, perimitque, a malignitate illius semper perire, credunt plures etiam in medius. Sed hæc est rara avis in terris," &c. (3)

Mills says that from the "presence of the petechiæ in fever the putrescency of the fluids is usually inferred." But "were petechiæ to proceed from the broken and dissolved texture of the blood, the blood so dissolved would be found in all the cavities of the body," &c. "Here," he says, "is a case of the typhus gravior of Cullen, or such as is commonly denominated putrid. The petechiæ disappeared after the second bleeding, an effect I daily witness from the use of the lancet, which clearly proves that this symptom proceeds from vascular excitement." (4) We shall have occasion to refer to similar observations by many others, in our Essay on Venous Congestion.

"Hundreds," says Rush, "have perished from the petechiæ of smallpox being supposed to mark a putrid, instead of a highly inflammatory disease; and thousands have perished from an ignorance that there are several states of fever which indicate a high and more dangerous grade of disease than sizy blood, and in which the lancet has been laid aside." (5)

Moseley remarks, that "much has been said by writers concerning malignant and putrid fevers, and the tendency of all

(1) Chisholm on the Pestilential Fevers of the West Indies, vol. 1, p. 418.

(2) Hufeland's Journal, t. 4, p 416; 1795.

(3) Baglivi, Prax. Med. l. 1, de Feb. Malig.

(4) Mills on the Utility of Bloodletting in Fever, pp. 6, 75, 156.

(5) Note to Cleghorn on Epidem. Dis. of Minorca, c. 7, p. 183; and Rush on the Bilious Fever, pp. 73, 270.

fevers to putrefaction in hot climates. But such opinions are not founded on practice, however they may seem to agree in theory." He objects to the epithets "malignant," and "putrid," on account of their tendency to deter from bloodletting. (1)

As to the supposed accumulation of carbonic acid in the blood, in febrile affections, Dr. Davy thinks its tendency is to preserve it from putrefaction. (2) But we cannot imagine any such influences upon the living blood, circulated and controlled by living solids.

We profess not to understand the precise limit which the humoralists assign to *living* putrefaction. Dr. Hosack says "we do not say that it proceeds to the same extent that takes place after death." (3) This is, at least, consolatory.

SECTION VI.

THE incubation of miasmatic fever may occupy months; Bancroft says, nine or ten months. Others have known it a full year, or more, as denoted by the explosion after a residence during that period in northern latitudes, subsequently to an exposure to a southern, swampy climate. Can it be that the blood may be contaminated, "fermenting," during this long period, without producing active disease in the solids; especially since it is many times removed, totally changed, before the explosion takes place?

It is no legitimate part of humoralism to suppose that the poi-

(1) On Tropical Diseases, pp. 101, 137, &c. Moseley says that "the notions of *asthenia*, and *putridity*, prevailed so universally in Jamaica, at the time of my arriving in the island, that the word *inflammatory* was scarcely known; and copious, or repeated bleeding was in general considered an agent of death." When we come to the consideration of Venous Congestion, we shall endeavour to show that *congestive*, more than true inflammatory, fever predominates in the hot climates. This, it is true, runs into common inflammation before death. But, even after death, the venous system is generally more injected than the arterial. This is the general testimony of the best observers.

(2) See paper in Edin. Med. & Surg. Journ. vol. 34, p. 262.

McBride, Percival, &c. supposed that the "putrefaction" of the blood in scurvy, &c. was owing to a loss of carbonic acid.

(3) Hosack's Lectures on the Theory and Practice of Medicine, p. 111.

son exists merely in combination with the blood, and thus acts upon the solids, in the same way as when operating externally. This would be perfect solidism. Besides, there is every reason to suppose, if morbific agents be really taken into the circulation, (an event of rare occurrence, as we shall endeavour to show,) they are speedily elimited by the excretory organs. It has been shown, indeed, by Prof. Hering, Westrumb, and others, that when a great variety of foreign substances were "mixed with the blood, they were quickly ejected from it. The decrease is obvious in a few minutes." (1) This is an important fact, which should be constantly borne in mind, as well as that which relates to the unceasing renewal of the blood.

Indeed, our facts, and our reasoning, are, here, strongly enough stated by the humoralists, — thus furnishing a sufficient ground of objection to their own doctrine. "When any part of the body," says the learned editor of the American Medical Recorder, "becomes impregnated with a noxious material, it is evident that the blood contains component parts that do not belong to it in its natural state. There is something present that is burthensome to the animal economy, and hence some emunctory is *immediately* employed to *cast it out* of the circulation. If these substances, which are not essential to the healthy composition of the blood, exerted no influence upon the system, is it probable that they would be so soon removed again out of the system? Does this fact not accord with that wonderful power of animated bodies, which is ever ready to remove such causes as are offensive to the living body?" (2)

This is the language of a shrewd and learned humoralist, expressive of the exact doctrine which is taught by the solidist, and shown by experiment. And may we not, also, take up his excellent philosophy, founded as it is upon fact and common sense, and apply it to noxious substances before they are admitted into the circulation? If there exist "that wonderful power of animated bodies, which is *ever ready* to remove such causes as are offensive to the living economy," may we not safely conclude, — aye, in the very face of experiments where so much difficulty and illusion exist, — that this same "wonderful power"

(1) Edin. Med and Surg. Journ. vol. 31, p. 213. Also, Müller's Physiology, vol. 1. p. 247.

(2) Vol. 5, 1822. p. 16. When Dr. Hale injected castor oil into his veins, he soon tasted it in his mouth.

guards the portals of the body, and by excluding the noxious agents, preserves the consistency of nature. "Natura, suis viribus usa, quæ noxia sunt expellere noverit, quæ utilia usui servare." (1)

Returning to the long incubation of intermittent fever, besides the entire renovations of blood which may happen before the explosion of disease takes place, the subject, in the meantime, may have undergone some severe local inflammation, during which the blood has been variously altered, and, not a little of it, perhaps, directly abstracted. Possibly, too, he will have had some of those specially humoral diseases, scarlatina, measles, erysipelas, &c.; yet shall the explosion of miasmatic fever ultimately ensue. But if we admit the predisposing causes to have acted upon the *vires vitæ* of the solids, there will be no difficulty in the case; for, however completely the blood and solids may have been renewed, the *vires vitæ* are still the same. "Singular, — to be at once another and the same!" (2) And yet the paradox is less than was thought by our philosopher. The material parts of our bodies are in no respect the same at intervals of six months. It is only the soul and the vital forces that remain without renovation. These constitute our exact identity. As one particle of matter is deposited, it takes the place of another that is simultaneously removed. But there is nothing that can supply a renewal of the vital powers. There is nothing brought in by the new material that is at all analogous to them. They are always there, sending off the old material, and organizing the new, with which they associate themselves. Hence it is obvious, that the morbific impression made long antecedently to the explosion of disease must have been upon the vital forces. Otherwise, also, if upon the material of the blood, or even upon the mere solids, then should the matter of A upon whom the morbific impression was made, having passed into B, through the vegetable laboratory, produce the disease in B instead of A. This is certainly absurd enough; yet to this complexion the humoral pathology must come.

It is only necessary, therefore, to follow the indications of constant analogies, and to suppose that a peculiar alteration of the vital properties is produced by the morbific agent, and that this remains more or less stationary, or gradually increasing, till

(1) Galen de Naturæ Facult. et Idem, de Diff. Feb. c. 5.
(2) Scott's Diary, Jan. 1, 1826.

some one of a thousand exciting causes acts with violence upon the altered properties, by which a more absolute change, according to the nature of the primary modification, is suddenly determined. All this can never be made absolutely *tangible*. But, are not the vital phenomena better evidences of the nature of the causes upon which they depend, than any approximation we may attain towards their physical demonstration?

What possible relation can the successive stages of fever, the horror and rigor, pyrexia, sweating, apyrexia, and then a stated return of the same phenomena in regular order, and at exact hours, have with a diseased state of the blood? What, the periods of the quotidian, the tertian, the quartan, &c.? (1) What makes these distinctions? Is it the increasing deterioration of the blood that allows the long interval of apparent absence from disease? Or rather, is it not analogous to the antecedent incubation of the fever, and does not the return of the paroxysms depend upon the same gradual alteration of the vital properties, which become partially restored during the modified actions which they institute? The proximate cause of the original paroxysm continues to be the cause of the succeeding, unless we superadd the force of habit. Were it depending on a diseased state of the blood, this would be a never-ending exciting cause, and should, at least whilst the disease continues, maintain its unintermitting existence. Senac, after describing the terrific symptoms which attend "malignant intermittents," remarks, that "a person inexperienced in the disease would scarcely believe it possible for the patients to rise again. But, strange to tell, at the termination of the paroxysm, these terrible symptoms abate, and oftentimes entirely disappear." (2) And all this, notwithstanding the corrupted, putrid cause of the whole difficulty, (even more morbific at the end of each paroxysm, when all the symptoms have given way, with an improvement of the secretions,) still continues to pervade the inmost recesses of every part.

Again, in many instances the paroxysms recur at intervals of a year, when the subjects have removed to climates remote from miasmata, and after the blood has undergone hundreds, perhaps thousands of renewals, and the patients, in the mean time, have

(1) What determines the intermitting hemicrania in marshy countries, or establishes a similar affection in some other part, when the rest of the system seems nearly exempt from disease? Is it a morbid state of the blood, constantly pervading all parts alike?

(2) On Fevers, p. 93.

been the subject of other diseases. These paroxysms, also, so remote from each other, may be brought about by present causes that can act only through the medium of the solids; as cold, fatigue, passion, &c.; thus proving that the predisposition resides in the vital properties of the solids. And if ultimately there, surely so from the beginning. If solidism be not capable of explaining fully the philosophy of the periodical return, and the subsidence of the paroxysm, we believe there are none who will attempt it on the hypothesis of humoralism. (1)

In another instance which the humoralist claims, — the bite of a venomous animal, — it is stated by Mr. Boyle, that, "although the quantity of poison is *scarce visible*, yet, in many patients it is preserved during a great part of their lives, and manifests its continuance in the body by annual paroxysms." (2) Mr. Boyle, it is true, was a most consistent humoralist, never admitting the convenient doctrine of exceptions to fundamental laws; but will the present humoralists venture to affirm that the poison, in the foregoing instances, was preserved in the subjects "during a great part of their lives?"

Analogous to the foregoing facts is the production of hereditary diseases.

> "Multa pater materque valent infundere natis,
> Transit et in seri jura nepotis avus."

These diseases are reputedly of humoral origin. Without a show of fact or of reasoning, it is assumed that "the disposition to gout, scrofula, leprosy, the venereal, and other hereditary diseases, can only be communicated through the medium of the fluids." "In the above diseases the vitiated state of the blood is, probably, the sole cause of the morbid action in the solids," &c. (3)

> "Qui viret in foliis, venit e radicibus humor,
> Sic patrum in natos abeunt cum semine morbi."

(1) We consider it utterly chimerical to attempt an explanation either of the *modus operandi* of the morbific cause, or the nature of the morbid changes. We can only say that a morbific cause exists, and that there are certain phenomena which denote a primary lesion of the vital forces of the solids. In what that lesion consists, or *how* the actions are modified, we shall probably remain forever ignorant. We speak now of idiopathic fever. More is probably known of the local inflammations which it developes, or which are developed with it; and which the morbid anatomist, overlooking the vital signs, would often have us believe to be the essence of the disease.

(2) Philosophical Works, vol. 2, p. 144. We state it as a humoral fact.

(3) Dr. Stevens, on the Blood, p. 149. And so Dr. Holland in Medical Notes and Reflections, *ut cit.* And so Andral. This is the general doctrine.

As well might we assume that the transmitted peculiarities of the Albino, or the colour of the negro, or hereditary black teeth, (1) are owing to vitiated blood, as that the foregoing diseases are propagated in the imputed manner.

Now, as to the ground of this assumed transmission of disease from parent to offspring, by way of the fluids, the only proof which we have seen consists in the affirmation that, "in this way, God visits the iniquity of the fathers on their children down to the third and fourth generation." (2) This is humoral philosophy. But we see not why the iniquities may not be visited as well through the solids as the fluids; although the authority which we quote has fortified itself by another citation from Holy Writ, which says that, "in the blood is the life thereof." (3) Doubtless, too, it is, for the most part, on a like intimation that *salt* has become the panacea for fevers, since we are emphatically told that "salt is good."

We shall make but few direct comments, in this place, on the foregoing assumptions; much less shall we follow the author in his "step from the sublime to the ridiculous." Was not Lemnius as near the truth when he said, "coitus super repletionem pessimus, et filii qui gignuntur, aut morbosi sunt, aut *stolidi*;" or, again, "intemperantia veneris qui uxores ineunt, nulla menstrui decursus ratione habita, nec observatio interlunio, præcipue causa est, noxia, perniciosa; tum et quarta luna concepti, infelices plerumque et amentes, deliri, stolidi, morbosi, impuri, invalidi, tetra lue sordidi, minime vitales, omnibus bonis corporis atque animi destituti." (4) This was pure humoralism, — the real "vox et præterea nihil." This author also maintains that wise men beget fools; and Erasmus, (5) on the contrary, that fools beget wise men, — the reason of which is interpreted by Cardan according to the humoral pathology: "quoniam spiritus sapientium ob studium resolvuntur, et in cerebum feruntur a

(1) See New-York Jour. of Dental Science, No. 1, 1839, p. 15.
(2) Dr. Stevens, ut Supra. (3) Ibid. p. 121.
The vitality of the blood is not only thus intimated in Holy Writ, but by Hippocrates, (*a*) Aristotle, (*b*) Virgil, (*c*) and many other ancients. It was probably, however, considered by these writers in the same sense that it is by butchers. Harvey appears to have been the first who regarded it as being endowed with vital properties, — "primum vivens et ultimo moriens, sedemque animæ primariam." (*d*)
(4) De Occult. Natur. Miraculis, l. 4, c. 3. (5) In Moria.
(*a*) De Flatibus. (*b*) De Hist. Anim. l 3, c. 19.
(*c*) Æn. l. 10, v. 486. (*d*) De Generat. Anim. Exer. 51.

corde." (1) And what better is all the fact and the reasoning which we have hitherto considered? Nevertheless, what we have just recited is one of the accompaniments of modern humoralism; being even palmed upon society at large under the allurements of a licentiousness (2) that consigned Dr. B. de Mandeville to an unenviable notoriety. (3) In making these observations, we have no reference to those transmitted peculiarities which appertain to the constitution of parents.

Hereditary predisposition to disease cannot be doubted; but when all sophistry shall have been exhausted in imputing its transmission by the blood, we shall look, with renovated confidence, to some peculiarity of the vital forces,—the inalienable properties of the solids,—for the only satisfactory solution of the mystery. We shall marvel at the gleam of light which broke through an era of greater ignorance; when Fernelius anticipated the age which has been succeeded by the speculations of his own, and who, in concluding his argument upon this subject, thus limits his doctrine: "ut semel dicam, una gula est morborum mater, etiamsi alius est genitor, ab hac morbi sponte sæpe emanant, nulla alia cogente causa." (4)

Whilst, on the one hand, it is perfectly inconsistent with the constant renewal of the blood and other changes to which it is subject, to suppose that it is forever in a peculiarly morbid state, without as constantly maintaining that disease in the solids

(1) De Subtiltate, l. 18.

(2) See Alexander Walker's Intermarriage, &c. New-York ed. 1839. Something of its spirit may be gathered from an introductory recommendation by "the Satirist," and from its "Advertisement." According to this writer the sentiment of love should have little or no part in the election of a wife; but the choice should be made as you would select a good breeder from a flock of sheep. Another laudable object of this work is "to show the cause of the rapid improvement of the Turks by polygamy." Although designed as a popular treatise, we think it prejudicial to the morals even of students in medicine, whilst it abounds with error and fanciful opinions.

(3) His "Latin verse in commendation of Marten's Gonosologium Novum," which was a professional work. Mandeville, who was prosecuted in consequence, observes that "if what the reviewers say of me were true, I ought to be hanged." *See his "Apology for a Latin Verse," &c. Lon.* 1709, *p.* 5. But he made the matter worse by his "Apology;" and what is not a little remarkable, this "apology" embraces many of the curious and fundamental principles laid down by Walker. He refers (p. 27,) to Ambrose Parey *de recta methodo coeundi cum fructu*, which, as the "Satirist" thinks, is the gist of Walker's Intermarriage. "Schencke," he says, "compared women to brutes, and particularly to bitches, when they are proud; for which, I think by the bye, that the German ladies are not very much obliged to him."—(p. 14.) See, also, Walker, Parts 5, 6, 7, 8, for parallels.

(4) De Pathologia, l. 1, c. 2.

which it is said to excite only at distant intervals; on the other, the vital properties of the solids are manifestly impressed in a direct manner by a variety of extraneous agents, as the obvious exciting causes, when hereditary complaints are brought into existence. Thus, nothing is more common than to see scrofulous inflammation suddenly follow a cold, or a fit of indigestion, &c. We will take an illustration from Dr. Stevens: "When a large draught of cold water is taken by a person weakened and exhausted, the stomach is suddenly deprived of its natural heat, the *extreme* vessels lose their *power of action*, and the impression is immediately communicated to the heart, the brain, and the whole system. But in another case, if water be drank from the same pump, the impression on the stomach is immediately followed by *reaction in the extreme vessels*," &c. (1) Solidism is as good in the hereditary cases, as in the latter; both as to the predisposing and exciting causes, and the development of morbid, or the increase of natural, action. It will be here seen, also, that our author has drawn upon John Hunter, to whom he imputes so vast an amount of sin, for a great fundamental doctrine of the vitalists. But our author may have been accidentally reasoning from nature, in this instance. Indeed, we constantly find, whenever the most exclusive humoralists attempt the analysis of disease, they obtrude upon the enemy's ground. This can only be excused on the principle of "hot pursuit." It is here, as we saw it with chemistry, after its conflict with the powers of life. Thus again, Dr. Stevens, after ridiculing the doctrine of sympathy, (2) and imputing every thing to a morbid state of the blood in miasmatic fevers, speaks, at last, more circumstantially, and announces that, "in fevers from poison, there is generally a derangement of the gastric organs, such as we produce by injecting an emetic solution into a vein; and during the hot stage of *all* the inflammatory fevers, there is generally an *intense headache*, produced either by the *derangement* of the *stomach* or by *pressure* on the brain, caused by the increased quantity of blood in the head."

Returning, once more, to the supposed agency of vitiated blood in developing scrofula, smallpox, lues, &c., is it not highly probable that such an agent, so incessantly applied to every part of the body, and constantly uniting itself with every part, would derange universally, and in a corresponding manner, the various

(1) On the Blood, &c. p. 128. (2) Op. Cit. p. 170.

tissues? This is the humoral essence of the predisposition. It must not appeal to the vital properties, organization, &c., for help; for then it merges into solidism. On the contrary, however, we see that scrofula attacks, chiefly, the lymphatic glands and the lungs; lues, the nose, throat, skin, and periosteum; gout, the joints, &c. On our principles, however, it is easy to comprehend, through a thousand analogies, and the plainest physiology, that hereditary dispositions to disease, may affect a part only, or the whole body, so that occasional causes, of temporary operation, shall develope morbid action in a part or the whole. So, also, accidental causes may predispose in the same manner, and other transient ones bring out disease, according to the predisposition established by the former series. In the hereditary case, it is nothing more than a peculiar modification of the properties of life, and is exactly analogous to the peculiarities which relate to the constitution of every individual human being. It is exactly the same principle that perpetuates a fine breed of horses, or the celebrated race of Durham cows. There is just as much of humoralism in one case as in the other. The whole philosophy of the subject is easily comprehended by observing how the vital properties of the solids are variously altered by mechanical agents. But, when the circulating blood, the *pabulum vitæ*, operates as a cause of disease in the solids, in virtue of its own diseased state, it must be a predisposing as well as exciting cause; and from its long and unceasing operation, and its absolute conversion into all parts, it should, in one way or another, derange every part of the system. We do not argue, all parts alike; since their organization is various, and their vital forces variously modified by nature, though the latter consideration does not belong to humoralism. These observations are equally applicable to miasmatic fever, and other reputedly humoral diseases. (1)

Drs. Evanson and Mcunsel, in their excellent Treatise on the Diseases of Children, think that "we have some warrant for inferring that the scrofulous diathesis is nothing more than a state of general ill-health occasioned by a deficiency in the proper supply of red blood, and consequent preponderance of white fluids and

(1) Mr. Hunter has considered the subject of humoral diseases upon other grounds than our own. They appear to us highly philosophical, and strongly opposed to the humoral pathology. (a)

(a) See his Lectures on the Principles of Surgery, lec. 11.

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tissues." (1) But are we not, at least, equally warranted in the conclusion that this assumed (2) deficiency of the red globules is owing to some primary defect in the vital forces and actions, upon which sanguification depends, and that such defect constitutes the entire essence of the evil? Or, what is the true primary cause of this supposed deficiency in the blood? Upon their principle, say the foregoing writers, "we can account for the hereditary transmission of the disease, and for its occasionally passing over generations." Is it, then, that this *imperfect state* of the blood is transmitted, — sometimes skipping a generation; "sæpe non eundem, sed similem produit effectum, et illæso parente transit in nepotem."? (3)

The wonderful effect of foreign causes in producing a permanent impression upon the vital properties is demonstrated by the protecting power of the cow-pox virus, and the immunity which is enjoyed from repetitions of the contagious diseases. The same philosophy is applicable in the foregoing cases, as in the present. It is only an exhibition of specific impressions upon the properties of life under a different aspect; which unfolds, however, by the variety, the whole problem in respect to the organic forces, and the true philosophy of the radical cause of disease. The ever changing blood is, of course, as susceptible of the corrupting effect of the virus of smallpox, measles, and scarlatina, at one time as another; and from circulating in every part, and being the *pabulum vitæ*, should produce the foregoing diseases at every exposure to the virus.

(1) Evanson and Maunsel on the Diseases of Children.

Upon this hypothesis, the practice has sprung up of feeding scrofulous patients with *dark* meats, in preference to *white!* We lost one of our best families because we would not submit, in consultation, to this popular philosophy.

(2) We say assumed; because the blood abounds as much with the red globules in a vast number of scrofulous subjects, in the absence of the disease, as in the most robust constitution; whilst we constantly meet with other constitutions in whom there is habitually a diminished proportion of the red globules, but no scrofula. If the tissues of some scrofulous subjects are lighter than natural, there are others in which they are darker, — all of which goes, in our mind, to the doctrine of solidism.

(3) L. Mercatus, op. t. 2, l. 5.

Why are we forever baffled with cancers and consumptions? We are near the time when Dr. Rush predicted that "they would be struck out of the list of mortal diseases." (*a*) But are we any nearer the event? Cancer is still a perpetual prop of humoralism; whilst the solidist, seeing nothing in the blood to rectify, and witnessing the vain attempts at its purification, regards the irresistible march of the disease as one of the best illustrations of a general principle. Or, again, with a sweep of the knife he exposes the visionary nature of the humoral pathology.

(*a*) Medical Inquiries and Obs. vol. 4, p. 405.

In either of the foregoing cases, the theory of the vitalist is the only one which can stand for a moment before the constant mutations of the body, especially of the blood, — laying aside all other facts, and all analogies. In the same manner, impressions from morbific agents, as we have already said, may remain for an indefinite time in a latent state. "The impression may be latent for half a century, for example, but which circumstances shall distinctly call forth." (1) The cause once applied, although withdrawn in an instant after, may be followed at a remote period by a protracted series of morbid phenomena. "When a cause is applied," says the philosophical Fordyce, "which produces fever, it produces it *uno ictu*, although the cause be no longer applied; neither is it increased, diminished, or altered by the farther application of its cause.

Will humoralism explain how it is that smallpox is sometimes "cut short by the violent action of medicine"? (2) Can the process be at all analogous to that suppuration which is said to be necessary to carry off the offending part of the blood, to concoct, and to clarify it? How, then, does the medicine transmute the whole mass, *instanter*, from its profoundly altered, morbific state, to a sanative agent? If you combine the cathartic with the morbific blood "in a porringer" will you get healthy blood in consequence? But perhaps smallpox, like the scurvy, "putrid" fever, &c., has been "cut short" by the abstraction of blood, and the poison being diminished a thirtieth part in this manner, the hypothesis is satisfied? Then, if you pour off an ounce of the thirty abstracted, the remaining twenty-nine should be equally changed, in consequence, to healthy blood. If you call in the aid of the solids, you abandon the speculation, and become a solidist. Why did Malouin, (3) Rossen, (4) and Sulzer, (5) succeed in preventing the eruption of smallpox upon the face by covering it with mercurial plaster? Why is the secretion of purulent matter diminished by opening the pustules? Or, how is it, as related by Hunter, Cruikshank, and others, that measles will supplant the smallpox, run their course, the smallpox reappear and observe its usual progress? What becomes of the blood, as originally contaminated, whilst it is subjected to the contamination of measles? Dr. Russell (6) has related two

(1) Beddoes on Fever and Inflammation, p. 135.
(2) Dublin Hospital Reports, vol. 4, p. 532.
(3) De Mercurio. (4) De Functione Cutis. (5) Hist. Morborum.
(6) Trans. of a Soc. for the Improvement of Med. and Chir. Knowledge, vol. 2.

cases where the smallpox and measles existed cotemporaneously, and went on their courses together. Glehn, and Hufeland (1) have known scarlatina and smallpox to progress together. Again, it is in these specific diseases, that we see especially manifested the spontaneous agency of the solids in recovering their own normal condition, and re-establishing the healthy state of the blood; since it is in these formidable diseases, particularly, that our remedies are often absolutely powerless. There can be no cleansing of the blood in such cases, excepting as it is the work of those solids which had brought it into a deteriorated state. And with what exact precision does not the work of restoration progress in the great humoral affections which we are now contemplating? The humoralist must explain this spontaneous termination exclusively by the doctrine of "coction," "mechanical expulsion," &c.; without the possibility of offering the least intelligible rationale of the process, whilst he is absolutely contradicted by the "cutting short" of smallpox, and pestilential fevers, by a single bloodletting. When venereal patients are attacked with smallpox, the eruption, it is said, has never been known to appear upon the copper blotches; and others have been known to be simultaneously affected with common syphilitic, and scrofulous ulcers; each having its specific discharge. Is the blood specifically altered in three different ways in these cases? The two first are great humoral diseases, and the discharge is said to be a drain from the blood of its impurities. Does vaccination cure hooping-cough by contaminating the blood in a different manner; or why does it protect the ever changing blood against all contamination from the virus of smallpox, or again leave it susceptible of various degrees of "fermentation"? We pause for a reply.

Again, when the predisposition to the very worst forms of tropical fevers is produced by concentrated malaria, or other causes, the explosion of disease may follow with the most surprising instantaneousness. "Two boatmen," says Mr. Evans, "became suddenly enveloped in a small cloud of vapour, when one of them fell down, apparently in a state of asphyxia, and the other was so affected as to be apparently unable to render him any assistance. The vapour passed away quickly, and the one who had been least incommoded recovered sufficiently to

(1) Brit. and Foreign Med. Rev. No. 7, p. 219.

look after his companion, whom he found lying in the mud, apparently insensible, but gradually became sufficiently well to be led home. In the course of the night, Dr. Chevalier was called to see him, and found him suffering from an intense ague. The surface of the body was cold, the countenance expressed great anxiety, the pulse small and scarcely perceptible; the patient in a state of coma, convulsions, &c. Reaction took place in about three hours. This was followed by a remission for a few hours, when another paroxysm succeeded, equal in violence to the first. The patient died in about 48 hours from the period of exposure. The body was examined whilst warm. The blood was *fluid*, (certainly an important fact for the humoralist,) a little serum on the surface of the brain, the lungs somewhat gorged, and the stomach intensely inflamed, containing about two or three ounces of blood." (1) The rapid effects of concentrated malaria are illustrated by other cases.

Here the exposure was momentary, the intense operation of the cause instantaneous; and we presume it will not be contended that the symptoms were owing to any primary lesion of the blood. And yet the foregoing case is a perfect type of the fevers which are considered of humoral origin. The cases are exactly analogous in principle to those which we have stated in a former section, where moral emotions, odors, &c., produced corresponding results. And if, in these instances, the action of the morbific agent have been upon the vital properties of the solids, by parity of reason the same philosophy is applicable to those cases in which the explosion of the same disease is delayed for many days, or months, after the morbific agent is withdrawn.

Nor is Mr. Evans alone in the relation of these extraordinary coincidences, in the application of febrile poisons and the development of disease. They are noticed by many authors in different countries; and Dr. Marsh has collected numerous cases of instantaneous seizure upon the application of the febrific agent. (2)

Lancisi relates an instance where thirty individuals, composing a party of pleasure upon the Tiber, suddenly encountered a wind from a marshy district; when twenty-nine immediately fell sick of a tertian fever.

(1) On the Endemic Fevers of the West Indies, p. 22.

(2) Dublin Hospital Reports, vols. 1 & 4. See, also, Whytt, on the Nature, Causes, &c. of Nervous Disorders, p. 14; and Monro's Experiments on the Nervous System.

"Miasma," says Dr. Johnson, "is capable of concentration, or rather accumulation; for when it is detained amid woods and jungles, and especially during the rainy season, when there are no regular breezes to dissipate it, and when the beams of the sun are obscured, except at intervals, by dense clouds, it becomes exceedingly powerful, as the annual mortality too plainly proves." "The poison will be found in some places so powerful that a man in perfect health, by remaining on shore during the night, in marshy situations, and wet or autumnal seasons, shall have the fever violently the next day, and die on the third or fourth."

We have introduced the foregoing remark of this profound observer, partly for the purpose of stating an example in illustration, which occurred in his own person, and which, like those we have already cited, corroborates the truth of solidism.

Sitting upon the deck of a vessel at a late hour in the evening, near the shore of the Ganges, "he perceived, all at once, a faint, *heavy odour*, which was immediately followed by a sense of faintness, giddiness, and, at length, nausea." Fully persuaded that he was seized with fever, he took a dose of calomel, and opium, and next morning, castor oil," &c. "The same," says Dr. J. "is often felt on crossing the Pontine marshes in Italy; and Dr. Moseley remarks that he felt a shiver while passing the swamps to the west of Kingston, before the sun had dispersed the vapour." Dr. Johnson is inclined to the belief that the primary effect of aerial poisons is upon the olfactory nerves, and is, by them, transmitted to the brain. (1)

Facts of the foregoing nature are of great force in illustrating this momentous inquiry. We cannot have too great a variety. Dr. Russel states, in his Treatise on the Plague, that "the effect of the pestilential virus, in some instances, seems to be almost instantaneous." (2) Dr. Rush "met with several cases of yellow fever, in which the poison acted so as to produce a fever on the same day." (3)

Dr. Geary "visited a child in an ill-ventilated cabin, without windows, who was attacked with maculated typhus. He had her brought to the door of the outer apartment. Immediately after examining the case, he was attacked with nausea, vertigo, and a general chill. Fever set in that night, and he was leeched

(1) On the Influence of Tropical Climates, &c. vol. 1, pp. 93, 101, 103.
(2) Russel on the Plague, p. 195.
(3) An Account of the Bilious Remittent Yellow Fever, p. 27.

and blistered next morning. The attack lasted 21 days,—singultus 72 hours." (1) and so, Dr. Potter. (2)

"In some plagues," "says Dr. Mead, "persons have been struck dead as with a blast of lightning, without any precedent fever, or even indisposition." (3)

SECTION VII.

It is not only important in a physiological, but practical sense, to understand the principles through which morbific agents establish their impressions.

Many vitalists, like the distinguished Dr. Johnson, whom we have just quoted, believe that the primary effect of poisons is exerted upon the nervous system. We have stated our reasons in another place for supposing that it may be either through this system, or conjointly upon the organic forces of other parts. We are now most interested with an opinion, entertained by able and sound philosophers, that morbific agents may act upon the blood through the nervous influence, without any intervening agency. This doctrine is entertained by Musgrave, (4) Philip, (5) Thackrah, (6) Sommé, (7) Barthez, Dupuy, Bordeu, Mayer, and others, as we understand them; but not in the humoral sense.

Since the doctrine of reflex nervous action, which was so well expounded by Prochaska, has been again taken up, and farther illustrated by Müller and Hall, we see a disposition amongst some, (and, strangely enough, even some humoralists,)(8) to ascribe every thing to the nervous system,—overlooking the independent organic forces. These properties are constantly receiving the greatest variety of impressions which appear to

(1) Dublin Journal, Sept. 1837. (2) Ut Cit. (3) Essay on Poisons.

(4) Speculations and Conjectures on the Qualities of the Nerves, *Passim.*

(5) Exp. Inquiry, &c.; and, more recently, Philos. Trans. Again, "the sensorial power can not only excite, but impair and instantly destroy all the powers of the living blood." (In London Med. Gaz. March 25, 1837, p. 961.)

(6) Inquiry into the Nature and Properties of the Blood. "Nervous Influence is the source of the blood's fluidity, and its loss the cause of coagulation."

(7) Etudes sur l'Inflammation.

(8) With the humoralists, however, it is always "*the nervous fluid,*" alias, *galvanism.* Dr. Macartney not only engages the nervous system primarily, but ascribes a "consciousness" to the properties of life. — *Treatise on Inflammation,* 1838.

have little or no connection with the nervous system, whilst they are well known to sustain the most powerful influences from the latter. This has been shown, in a variety of ways, experimentally; and the natural phenomena of life abound with illustrations.

It appears, then, to us that the nervous influence is only concerned so far as it propagates the impression to the vital properties of the great instruments of organic action. We believe, also, that our numerous facts warrant the conclusion that the primary morbific impression, so far as the nervous system is concerned, is more especially exerted upon the irritability ("organic sensibility") of that system, than upon the sensibility. We do not admit that this is hypothetical; but refer to the premises which we have stated, and those which are to follow, for its rational deduction. Sensation is not often involved. C. H. Parry, we believe, denied that pain is a cause of disease. He "utterly and forever disclaims all reliance on the neuro-logical systems of pathology hitherto extant. He considers them calculated to debase the moral character of mankind, to produce or perpetuate disease, and to discredit the medical profession." (1) Still it is manifest, that sensibility may be sometimes interested, as shown by the effect of many odours, which can operate in no other conceivable way, and would probably have no effect if sensation were not concerned, along with the organic properties of the nerves. If the blood remain fluid in cases of instantaneous death, it is easily comprehended, through analogies already stated, how the violence of the shock sustained by the organs of sanguification should exert a pernicious effect upon the fluid which they hold in subjection; whilst there is no immediate or known connection, analogous to that of the vascular system, betwixt the blood and the nerves, through which changes are likely to be determined in that fluid. If certain impressions, however, upon the nervous system so modify the organic fevers, that the blood remains fluid after death, we find, on the other hand, when syncope takes place from the loss of blood, that which flows last coagulates with great rapidity. These differences in the effects of the nervous influence are not more remarkable than many other phenomena arising from the same cause, where they appear quite contradictory, according to the manner in which the nervous influence is made to operate. Thence we understand

(1) Elements of Pathology, vol. 1. *Preface.*

how the blood should be fluid after death from a charge of electricity emitted from a battery of 50 or 100 acres, whilst it coagulates partially when an animal is killed by a battery of a few square feet. (1) True, it can scarcely be said, that there is an action of the vascular system upon the blood in cases where death is produced by lightning; but the altered condition of the vessels is plain enough. It is not improbable, however, that the electric agent may then be the direct cause of the blood's fluidity, acting, perhaps, chemically upon it in very large quantities.

The cases communicated to the London Medical Gazette, by Stanley, and Robinson, and the direct experiments by Magendie, and Camerer, as stated in another place, showing that disorganizing inflammation may take place after the abolition of the communicating nerves, are also calculated to shake our faith in the full extent of the reputed agency of the nerves in the incubation of fever. So, also, paralytic limbs have participated in febrile action. This fact is noticed by Dr. Stevens; and whilst he variously argues that miasmata, &c. can have no operation upon the nervous system, (2) he thinks "there are many reasons for believing that the nerves, like the electric fluid, can produce a sudden change in the quality of the whole circulating current;" (3) and this, without the agency of the blood-vessels. We speak of it to show the expedients of humoralism.

That the nerves, too, are less concerned in the morbific effects of foreign agents, than supposed, appears from the action of various poisons upon the leaves of vegetables, whether operating from without or from within. (4) Whilst, however, it is abundantly manifest, that impressions produced upon the nervous system may be powerfully determined upon the *vires vitæ* of other tissues, we are apt to overlook the extreme susceptibility of the latter, from the more obvious phenomena of sensation. Nothing convinces like feeling. Upon irritability, however, all the phenomena of organic life primarily depend. It is perpetually alive to the stimulus of blood, and must, therefore, be exceedingly susceptible of impressions from many other causes. This is familiarly shown by the pulsation of the heart, when removed from the body; its action being maintained by air, heat, me-

(1) See Scudamore on the Blood, p. 53, Exp. 52.

(2) On the Blood, &c. pp. 234, 242, 259, &c. (3) Ibid. p. 260.

(4) See Experiments of Christison and Turner in Edin. Med. and Surg. Jour. vol. 28; and Marcet and Macaire in Biblioth. Univer. t. 31, and Ann. de Chimie, t. 29. Also, Lindley's Botany, c. 13.

chanical irritants, &c. (1) The same principle pervades every part, and is more or less liable to the same impressions.

The independent vital properties of the capillary system supply, therefore, an intelligible medium upon which foreign agents may operate. But none can doubt, whilst the integrity of the nerves is preserved, that their influence contributes more or less to the early modifications of the vital forces of other tissues, whether in fever or inflammation. Sensibility is the peculiar property of one; irritability common to both, though independent in each system. But they are so blended together, that they must exert reciprocal influences; though it is certain that the presence of the nervous system of "non-vital parts," or, at least, its connection with the brain, is in no respect necessary either to natural or morbid action. Billing is clearly wrong when he says, "that while parts are supplied with the nervous influence, they retain their power of action, and not longer." (2)

The various experiments that have been performed upon the nerves illustrate the sympathies which they establish betwixt remote parts, and the probable agency they have in this way in the incubation, as well as in the subsequent phenomena, of disease. It is still important, however, to distinguish the nervous power from the organic forces. It is the latter which carry on the actions. The former can only influence the other by the impressions it receives from foreign causes, mental emotions, &c. The operation of remedial agents is to be greatly explained upon this principle. The salutary impressions are especially propagated from the alimentary canal, through the medium of the nervous system, to other parts. That the nervous centre may

(1) We refer to a practical commentary on this subject, in the examples of vital action in dried hearts, at page 17.

Dr. Parsons, in his Crounean Lectures, states an experiment which he performed upon the heart of a viper. When laid on paper, it beat 24 pulsations; then, in the hand from 37 to 48; and, afterwards, in water at 100° F. 87. Whereupon he remarks, "it is amazing to think that such very small portions of the nerves, as were cut out with the heart, should contain matter sufficient for so many contractions; and that it did not all of it instantly fly out from the wounded ends, even before it was put into warm water." (a) This reflection is also well adapted to the hypothesis of galvanism.

Lord Bacon says he has seen the hearts of criminals, when thrown into the fire, leap up half a foot, and to continue leaping up and down, "gradatim ad minus," for seven or eight minutes. (b)

(2) First Principles of Medicine, p. 22, 1838. That influence, in conformity with the philosophy of the day, he considers galvanism.

(a) Philos. Trans. Abg. vol. 11, p. 1205.

(b) Hist. Vitæ et Mortis, in Op. t. 3, p. 374.

be suddenly and powerfully irritated merely from its sympathy with other parts, and the sympathy be variously reflected upon the organic forces is strikingly shown in nervous aploplexy, and in the convulsions which attend the dentition of infants.(1) De Haën justly states that a nerve may be so irritated as to produce a derangement of the entire functions of the brain.(2) Again, we have a long catalogue of diseases which refer themselves to moral causes, — so fatal to the physical theories of life. Surgery constantly affords corresponding examples, some of which, as instant death from trifling operations, we have had occasion to state. Sympathetic pains are forever springing up in parts remote from each other, and the disease, which often follows immediately, shows how the nervous influence is perpetually affecting the organic forces. In our Essay upon Bloodletting, we have endeavoured to illustrate by various facts, how the nervous influence is concerned in modifying organic actions, and thus affects the constitution of the blood, during the progress of the operation. In the same way we have some intelligible account of the remarkable changes of colour in the skin and hair, which have been noticed by Le Cat, Fardeau, Rostan, and others, as having suddenly followed violent emotions of the mind. And, in respect to the incubation of miasmatic fevers, the agency of the nervous system appears to be well established by many accurate observers. Thus, it is said by Cheyne and Barker, in their account of an epidemic fever in Ireland, that "we are not of opinion, that the time between exposure to contagion, and the formation of the disease thereby caused, is a period of health. The *nervous system* was affected previous to any disorder of the circulating system." Still it is probable that whenever manifestations of disturbance of the nervous system begin, there is, also, not only an alteration of the organic properties of other tissues, but that a critical examination will always detect an alteration of functions.

(1) "The probability is," says Dr. Armstrong, "that there is some important discovery to be made with reference to the nervous system; for there is unquestionably a subtle something, — a *tertium quid*." (*a*) True; something intermediate between mind and matter, and probably as little cognizable by the senses as mind itself. We may imagine galvanism; but it will never help to explain a phenomenon of life. We apprehend, however, that this *tertium quid*, in its relation to the nerves, is only a part of what we have regarded as the vital principle.

(2) Rat. Med. t. 3, p. 121.

(*a*) Lectures, &c. vol. 1, p. 169.

All the changes in living organized matter, whether they relate to the blood or to the solids, depend upon the action of vessels. We certainly except those unnatural experiments in which chemical agents are injected into the circulation, when they affect the blood much in the same way as caustic affects the skin. But this is no part of medical philosophy, and nature has no analogies with these preposterous contrivances of art. It is by the vessels, through their organic properties, that the blood and all other parts are formed. These forces exist in a certain independence of the properties which are peculiar to the nervous system, and no physical change can happen but through their direct agency. But the nervous influence may exert impressions upon them, modify their actions, and more or less alter their natural character; just as foreign agents act upon them in an analogous manner. It is, however, no more the nervous influence that maintains fluidity, (see p. 474,) or produces alterations in the blood, than it is the point of the lancet that occasions the results of phlebitis. In either case, the effects are owing to modifications of the vital forces of the extreme vessels; and the nervous influence, and the lancet, are, nearly, alike the agents by which the organic powers have been impressed.

All conclusions upon the foregoing subject, as in other physiological inquiries, have been generally not a little embarrassed by leaving the phenomena of nature for artificial results, and the contradictory statements of experimenters. Thus, Brachet gave to a dog six grains of opium, after dividing the par vagum. He manifested no symptom of narcotism; whilst another dog, to whom he gave the same quantity, fell immediately into a profound state of coma.(1) So it is positively affirmed by different physiologists, that nux vomica, which will destroy a rabbit in one second, produces none of its peculiar effects in a far greater dose, if the nervi vagi be divided; or, at most, only at a late period. And so of other poisons. Here, it would seem, that there can be no possible room for mistake. But such results absolutely contradict the whole humoral doctrine of absorption, fermentation, &c. Whereupon, Müller procured Wernscheidt to repeat these experiments, and the following is the result: After affirming that, "before narcotic poisons can exert their general effects on the nervous system, they must enter the circulation," he proceeds to say that "Dupuy and Brachet, indeed, maintain that

(1) Recherches Exp. du Sys. Nerv. c. 3, p. 177 — 227.

animals cannot be destroyed by narcotic poisons introduced into the stomach, if the nervus vagus has been divided on both sides, or, at least, that they do not die so soon. But in thirty experiments on mammalia, which M. Wernscheidt performed under my direction, not the least difference could be perceived in the action of narcotic poisons introduced into the stomach, whether the nervus vagus had been divided on both sides or not, provided the animals were of the same species and size." (1)

But, what appears to establish the credibility of Brachet and Dupuy, (for the question is resolved into this by the simple nature of the facts,) is the well known integrity of those philosophers, their great experience in the experimental art, and the incontrovertible fact that emetics and cathartics have but little effect upon dogs if there have been a previous division of the nervi vagi; and we may also refer to the analogous condition which is often attendant on apoplexy. It is clear that Müller's experiments had in view the humoral hypothesis of absorption.

Admitting, however, that Brachet's and Dupuy's experiments are entitled to no credit, and that Müller's should be received without suspicion, the latter proves nothing in the way of absorption; for even after the division of the nervi vagi there are still other channels of nervous communication betwixt the stomach and the brain, and the organic properties of the stomach still exist, though certainly impaired.

We shall resume this inquiry in our 11th section.

SECTION VIII.

We have now seen two sources through which external causes may readily establish morbid actions, and induce all imaginable changes in the blood, without resorting to the violence of supposing that morbific agents may pass the highly susceptible properties of the nervous and vascular systems, and be taken into the circulation, without inflicting an injury upon those forces, whilst it is assumed that they exert a deleterious effect upon the blood; and having corrupted, or produced a ferment in that fluid, the vital forces no longer escape with impunity.

If, therefore, in all the cases to which we have hitherto adverted, it appear certain that the morbific agents exert their perni-

(1) Elements of Physiology, vol. 1, p. 246.

cious effect upon the solids, and considering what we have stated in relation to the susceptibilities of the nervous and organic properties, and how impressions upon the former may be determined upon the latter, before proceeding farther with our argument, we again feel entitled, by the rules of sound philosophy, (so much insisted upon by the humoralist,) to the conclusion, that all diseases are brought about in a corresponding manner. It is indifferent to us, at the present moment, whether the poison is circulating with the blood, or operates from without. In either instance, it is the poison, not the blood, which produces disease. But we shall endeavour to show that it is purely a matter of assumption, with very rare exceptions, that poisons are taken into the circulation, — unless put there by the agency of art, or brought into contact with abraded vessels.

In the bite of the viper, the effects are so instantly consequent upon the application of the cause, and such a progressive series of phenomena arise, that they can only be explained upon our principles. The rapidity, too, with which remedies afford their relief, and their nature and operation, supply the strongest proof that the solids are the immediate subject of the morbific and the remedial agents. As this is a favourite example with the humoralist, it is proper that a case should be fairly presented. But this case is mainly intended to establish our principles through the curative agents. Humoralism fails whenever the means of cure act exclusively upon the solids, as we shall ultimately demonstrate. "A countryman was bit by a viper on the right hand. On the following day, the whole extent of the arm was enormously swollen, and its surface was of a livid-red colour. The face and trunk had a jaundiced hue, and the extremities exhibited diffused patches of redness. There was great prostration of all the vital powers; the pulse was scarcely perceptible; the body was bedewed with a cold clammy sweat; the face convulsed, pupils dilated, breathing laborious, and frequent efforts at vomiting. Dr. Butazzi prescribed three grains of quinine every hour. On the following day the arm was as much swollen as before; the pulse was scarcely to be felt, &c. Dose of quinine increased to four grains. On the next day, there was a decided amendment; a copious warm perspiration had taken place; the discolouration of the surface abated; and from this time, the swelling of the affected arm subsided." (1)

(1) Med. Chir. Rev. Lon. vol. 29, p. 509.

Here, then, we have in this favourite example of humoralism, a manifest illustration of the primary action of the poison upon the solids, in the progressive, but rapid, and "enormous swelling of the arm;" and if the chemical pathologist chooses not to consider the other developments as sympathetic, he can only regard the poison as having been taken, in part, (if susceptible of division,) into the general circulation, and as existing there in a free state, and producing its deleterious action upon other parts of the solids, as upon the arm. And this would be pure solidism. The reason for this conclusion we shall soon show. But, in the mean time; we may say, that the poison was not carried into the torrent of the circulation, since there was no inflammation produced in any part corresponding with that of the arm; and this proof may be affirmed of most other analogous cases. It is, to our mind, conclusive, and should be well considered.

But the quinine! what had the quinine to do with the re-establishment of the blood? How did it arrest the "ferment," or otherwise stay the progress of the corrupting influence of the poison, and the concurring deterioration of the blood which the morbid solids were fast inflicting? You abandon it. Then we ask how it be possible for the quinine so to impress the solids as to enable them to destroy the morbific condition of the blood which had thrown them into such a state of violent disease? If, in their natural condition they could not resist the action of the morbific blood, is there any philosophy in supposing, that when their powers were subverted, and the morbific cause constantly increasing in intensity, they were in a better state to surmount the evil which had nearly laid them in ruins? If not, then it is plain enough, that the poison had produced its effect solely upon the vital forces of the solids; by which we arrive at an easy solution of the manner in which the quinine, producing its effect like the poison, brought the properties of life with triumph into their natural state.

A single case of the foregoing nature settles the philosophy, at least, in relation to all other viper-bites.

In another case of poisoning by the viper, the symptoms were not less violent, and the arm swollen to thrice its natural size; the swelling having advanced progressively up the arm. The treatment consisted of warm poultices to the arm, and twenty leeches to the stomach, when the patient speedily recovered.(1)

(1) Bulletin de Thérapeutique, &c. 1834.

In the Recueil Périodique d'Observations de Médicine, &c.(1) are two cases of the same kind, where the patients recovered after having suffered violent symptoms, and where the treatment consisted of embrocations with volatile liniment, and *d'eau de luce*, in doses of six drops. In the Philosophical Transactions(2) are records of several experiments made upon human beings in presence of the Royal Society, &c. with the bite of the viper. The progress of symptoms, and their rapid subsidence, are clearly indicative of the local action of the poison, and its propagation by sympathy. "William Oliver said that he immediately felt a *violent pain*, and shooting from the wounds from the thumb up his arm, even before the viper was loosened from his hand." "In a few minutes his eyes began to look red and fiery, and to water much." Very violent symptoms soon afterwards supervened,—as, also, in the other cases. The cures were attributed to olive oil, applied locally; but the whole credit is, doubtless, due to nature; as, also, in the instance quoted from the Recueil. This, therefore, is still more to our purpose.

But, as to any or all of the foregoing remedies, we take it that no one will assume that their action is in the least exerted upon the blood; and since it would be opposed to all fact and reason to suppose that the solids may be brought into a condition to speedily convert the imputed morbid state of the blood, which they had no agency in producing, and which they could not prevent, into its natural state, so that it shall become sanative instead of morbific, we must unavoidably conclude that the solids had been alone concerned in the morbific process. It is a fundamental doctrine of the modern humoralists, as we shall have shown by various quotations, that the remedies for humoral diseases must be such as act directly upon the blood; however true it may be that their practice, like that of the homœopathist, is constantly at variance in all severe affections.

As the poison of the viper may be taken with impunity into the stomach,(3) Dr. Stevens, and other humoralists, endeavour to get over the objection of its not being absorbed, by assuming its decomposition. This, of course, cannot be admitted; so that

(1) 1756, p. 412.

(2) Vol. 39, pp. 312, 394. Vol. 40, p. 26, Abdg. vol. 11, p. 60—66. See, also, vol. 7, p. 5060.

(3) Stevens on the Blood, p. 137; Meckel's Archiv. t. 3, p. 639; Froriep's Notiz. Sept. 1823, p. 170; Fontana on Poisons; and others. So, even, of the smallpox and hydrophobic virus, the woorari poison, &c.

both parties must be content with the simple facts, that it is not absorbed, and that the stomach resists its action. The surface must also be abraded before its deleterious effect can begin. It must be applied directly to the denuded blood-vessels, where the vital forces are most strongly pronounced, and where the nervous extremities are unprotected. The fallacy of the foregoing assumption is at once shown by the fact that the poison exerts no action when applied to any part of the external surface. Is it "decomposed" by the tongue, the lungs, or when applied to the skin? The worari poison manifests its effects as soon as inserted under the skin. Is it decomposed by the stomach on the instant of its contact with that organ? (1) And is it not opposed to reason and physiology, that instant decomposition of the foregoing, and many other poisons of analogous peculiarities, should have happened in most of the thousand cases in which the experiment has been made? The proof is conclusive; from which it also appears, as is true of most other deleterious agents, that the absorbents do not take it up. Poisons will act, or not, upon the vital forces of the stomach, &c. according to their specific properties; but, in the latter case, it constantly happens, that the moment they come in contact with a denuded spot, their action is violent, and may be exclusively upon the part.

We know not by what authority, but it is perfectly new to us, that, as Dr. Stevens asserts, "when the muriate of soda is immediately applied to the wound that is made by the rattle-snake, it is a complete antidote against the poison." (2) This looks like a prop for the "saline treatment;" but if it be true, the neutralizing property of salt should be shown, or the solidist will be very apt to appropriate the fact to his own use. It is, however, palpably too great an assumption, not only in a practical but scientific view, to pass without this notice. Nor should we omit saying, that precisely the same thing was once affirmed in Italy of *ammonia*. "Miracles," they said "had been done with it." (3) Thereupon, Fontana made more than six hundred experiments to prove that ammonia is no remedy against the bite of a viper.

(1) Dr. Stevens "has given the Wauralli poison internally, in large doses, to dogs, monkeys, and other inferior animals, yet they did not suffer, even in the least." (Op. Cit. p. 142.) Müller, and other humoralists, endeavour to show that poisons may be absorbed by the stomach in one minute. And yet, strangely enough, Müller maintains the decomposition of "the poison of serpents," &c. by the stomach. Physiol. p. 478.

(2) On the Blood, &c. p. 142. (3) Fontana on Poisons, vol. 2, p. 77.

"If, however," says Girtanner, "he had made a single comparative experiment, and had applied ammonia to a wound made by a lancet that was not poisoned, he would have found that ammonia itself, applied in this manner, would have produced a disease exactly analogous to that caused by the venom of the viper; and, consequently, so far from removing the malady, must necessarily increase it." (1)

Modifying properly the affirmation of Girtanner, the same may be made of muriate of soda, a very striking example of which is recorded by Dr. Bardsley. A woman having been bitten by a dog not rabid, "immediately applied salt to the wound." This irritating application, apparently, kept up an inflamed state of the wound, and prevented its healing for more than four months, when she fell into a state completely resembling hydrophobia; but, from which she slowly recovered, *as soon as the wound was healed.* Dr. B. justly regarded it as having no direct connection with the dog. (2) (See case at p. 425.)

But let us suppose, instead of a poison, that wine has been injected for the cure of hydrocele; or let it be injected into the cellular membrane of the scrotum, and there remain till inflammation be well established. Should we not have symptoms scarcely less violent than those which we related of the bite of the viper? And where is the difference in the progress, and general phenomena of the cases, or the principle on which the curative agents operate? The understanding, not the imagination, must be brought to their contemplation. Or take a less ponderable, perhaps, immaterial substance,—heat, for example. Burns and scalds may produce results not less extensive and violent than the poison of the viper, and observe the same progress from local to constitutional developments. It will, of course, be admitted that the primary action in these cases is upon the skin. And, in what other intelligible way can the constitutional disturbance be explained than through the principle of sympathy? We might go on with an endless multiplication of analogies,—taking them from the humoral ranks. Thus, whilst a blister relieves a gouty foot, it may suddenly bring the stomach into a similar condition of disease. Can humoralism account for the phenomenon? True, to this day, and in high quarters, its ele-

(1) Memoir on the Laws of Irritability.
(2) Edin. Journ. of Med. Science, vol. 1, 1826, p. 250.

gant diction is, that it drives the peccant matter to the stomach. But why not attack other parts on its way? Why the stomach almost always in preference to any other organ? Does humoralism also still maintain that "repelled eruptions" excite internal disease by "a translation of the morbific matter;" and if so, why is the new disease almost instantly removed by a sudden reappearance of the cutaneous affection? Do interchanges of the virus take place in these cases through the medium of the blood? And if so, is it not strange that the "peccant humour," whilst passing from one part to the other, should confine its malign influence to the skin and intestines? It is universally affirmed that an attack of urticaria, after a debauch, is owing to a morbid state of the blood. But, then, is it not unaccountable how the blotched and swollen face is often restored to its natural appearance during the operation of an emetic?

We wish not to leave our viper-bites without their full effect. There may be an attempt at circumventing them. Here is an instance whose rapidity seems to put a vitiated state of the blood out of all question. There was not time for absorption before the violent symptoms began, even by way of the injured vessels. Thus, Bengger relates the case of a child, who died in about ten minutes after the bite of a rattle-snake. (1) Still the blood may have been corrupted? Then regard the rat upon which a bite has been inflicted. The constitutional symptoms are often manifested on the instant. And here is an *experimentum crucis*. "I have seen," says Dr. Johnson, "the ear of a rabbit exposed to the bite of a Cobro de Capella, with a pair of scissors kept across the ear, ready to cut it off the moment the bite was inflicted; yet the animal died quickly in convulsions." (2) Just so "a slight prick in the nail, at the extremity of the finger, has brought on the most horrid convulsions;" (3) and the opening of a thecal abscess has produced instant death. "It has been long acknowledged that violent poisons may destroy life before there is a possibility of their entering the circulation." (4) The experiment just related clearly settles a principle which must apply to the analogous cases which preceded, and from which we had deduced the same philosophy by another process. We refer, also, to the ex-

(1) Meckel's Archiv. &c. t. 3.
(2) Med. Chir. Rev. Lon. vol. 29, p. 145. 1836.
(3) Zimmerman on Experience, &c. vol. 2, p. 85. See other examples in Sec. 3.
(4) Johnson, ut supra.

periments of Dr. Addison, and Mr. Morgan, for a confirmation of our views.

Even so great a humoralist as Dr. Mead is compelled to abandon the true humoral ground in accounting for the effects of the foregoing poisons; and although he invades the philosophy of the vitalist, he mixes with it a spice of his own. "It is done," he says, "by the great activity of the nervous fluid, one part of which being infected immediately taints all the rest." Indeed, Dr. Mead had the sagacity to see that nothing could be done with "pestilential fevers" in the way of the "ferment," without some previous agency of other materials than the blood. For this purpose he calls to his aid the nervous system; yet, so strongly wedded to humors, he devotes an inquiry into the nature of the nervous fluid. From all which he concludes, that "the first impression of the poison is made on the nervous spirit, yet the whole mass of blood very quickly partakes of the hurt; not, I suppose, *from any mixture* of the venom with it, but from such a corruption of it, as an *irregular circulation*, interrupted secretions, and stagnations in the smallest vessels, the consequences of a vitiated nervous fluid, may naturally produce. Nor will this seem strange, since such a compounded liquor as the blood is, may certainly undergo any imaginable changes by alterations made in its motion only." (1)

Substituting nervous influence for "nervous fluid," we have, in the foregoing extract, a complete exhibition of the doctrine of solidism; and it is remarkable that so great a practical philosopher as Mead, with such a luminous view of his subject, should have so constantly glided off into the speculations of the humoral school. And so it is, indeed, with all the able humoralists of the present day. In explaining the phenomena, and in the application of curative agents, they are vitalists; or rather, like Mead, suppose some movable fluid in the nerves.

That nothing may be left unsaid upon this important subject, it may be stated that Dr. Mead combined "five or six grains of the viperine poison, newly ejected, with half an ounce of human blood received into a warm glass; but the blood was not visibly altered in colour or consistence. It then was, and remained undistinguishable from the same blood taken into another glass, in which there was no poison at all." (2) And, again,

(1) Introduction to Essay on Poisons, pp. xix, xxvii.
(2) Medical Works; Essay on Poisons, p. 35.

"Dr. Stannius was unable to recognise, by the *microscope*, any alteration in the appearance of the blood in frogs poisoned by strychnia." (1) The same symptoms, also, take place whether they are deprived of their lungs or not. (2)

Mitscherlich, it is true, performed a series of experiments by mixing different *chemical* agents with the blood of frogs, and, as might have been expected, the globules of blood underwent a change. But when he came to the destruction of these animals by immersing them in acrid solutions, he could detect no such alterations. The blood accumulated under the skin, and presented exactly the phenomena which the vitalist would infer from the violent changes induced in the action of the cutaneous vessels. (3)

But suppose that every thing which may be directly mixed with the blood, whether in a "porringer," or by injection into the veins, may alter the constitution of that fluid, what more does it prove than the simple fact in relation to each substance, when thus mechanically combined. On the other hand, in every patient that is bled, and every inflammation that is produced by mechanical causes, we have the most palpable demonstration that the action of the blood-vessels produces, almost instantaneously, a greater change in the blood than rarely falls to the lot of the foregoing experiments.

It is astonishing with what rapidity organic changes may take place from the action of poisons which exert no chemical effect. Here is a case by Sir E. Home, exactly parallel with that recently quoted from Dr. Johnson, and which shows, in other aspects, that poisons produce their effects directly upon the solids. He caused a rat to be bitten by a snake. It died in one minute. The cellular membrane beneath the wound was wholly destroyed, the muscles separated from the ribs and from a

(1) British and Foreign Med. Rev. vol. 5, p. 222.

(2) And yet the humoralists adopt the statement of Dr. Stevens, that these, and other poisons, blacken and otherwise corrupt the whole circulating mass of blood. Chemistry having reflected no light upon the subject, Dr. Johnstone says, "it is much to be regretted that we are so *so little acquainted* with the precise nature of the chemical changes which the animal fluids undergo when a poison has been introduced into the circulation. Animal chemistry *has done much*, but much yet remains to be accomplished." (*a*) On the other hand, it is stated by another humoralist, that "certain poisons appear to produce some change in the blood," "though in what that change consists, we are, *at present*, unable to decide." (*b*)

(3) Med. Chir. Rev. Lon. Oct. 1838, p. 561.

(*a*) Trans. of the Provincial and Surg. Association, vol. 5, 1837, p. 233.

(*b*) Ibid. vol. 3, 1835, p. 76.

small extent of the scapula. The bitten part was greatly inflamed. (1)

Enaux and Chaussier state that the symptoms, at first, are always local after the bite of a viper, if the animal be not killed by the poison almost on the instant. (2) Sir E. Home compares the action of arsenic upon the muscles of a frog to that of the poison of a snake. (3) And Orfila shows the primary local action of many vegetable poisons when applied to wounds, — as elaterium, colocynth, &c. (4)

It is true, in many cases of poisoning by wounds, especially dissection-wounds, there may be no apparent affection of the injured part. But here we must suppose that the morbific impression was made upon the nervous system as in Johnson's experiment upon the rabbit's ear; since, also, in many of the instances the wound has been so small as to be only seen by the microscope, and sometimes not at all. It is too much, therefore, to ask of the human understanding to admit that such an inappreciable quantity of virus will so contaminate the entire mass of blood as to develope the frightful effects of these injuries. The quantity, indeed, is only capable of the sub-division supposed by the humoralists, in a mathematical sense. In most of the cases of poisoning in dissections related by Travers, Colles, Stafford, Duncan, the French Reporters, and all others, the wounds have not been generally larger than a puncture by a needle. (5)

In opposition to the foregoing facts, and the experiment which we have related from Mead, (p. 487) it is maintained by Müller, and others, that "the poison of serpents, when introduced into the blood, produces decomposition of the animal fluids." (6) It is the constant occurrence of affirmations like this, which renders a tedious investigation of facts indispensable. There is no other method of putting them down, and our readers must submit to it. It would be an easy matter to respond, that it is not so; but this would be neither respectful, nor very satisfactory.

Let us consider the bearing of the last mentioned affirmation

(1) Philos. Trans. Lon. 1810, p. 86.

(2) Method. de Traiter les Morsures des Anim. enragés, et de la Vipère, &c. p. 109.

(3) Ut Supra, p. 86. (4) Toxicol. Gén.

(5) See, particularly, Travers on Constitutional Irritation, p. 154—247; and Duncan in Edin. Med. Chir. Trans. vol. 1, p. 494—547, and p. 594—596.

(6) Elements of Physiology, vol. 1, p. 478. In this way the "peculiar effects" are supposed to be exerted on the solids. Müller even supposes the same of many "narcotic poisons"! an induction founded mainly on their alkalescent nature. So, therefore, soda and potass act by decomposing the blood.

upon the humoral pathology in other aspects. If morbific agents penetrate into the circulation, and there produce their effects either primarily upon the blood, or upon the solids, we should certainly meet with more undoubted examples of disease from putrid animal matter, considering the violence of its effects when applied, in almost inappreciable quantities, to the vital forces of the solids by inoculation. The same matter, which is thus so destructive, is applied to the mouth, stomach, and skin, and respired, with impunity. If other morbific agents are taken into the circulation through these avenues, we do not understand the exception in favour of the foregoing and many other fatal poisons, which act by inoculation.

Precisely analogous to the foregoing cases are those related by Sir B. Brodie, in which severe erysipelatous inflammation of the hand "followed the handling of sheep's offal," and wounds received from the bones of hares. In these cases, the affection was entirely local. (1)

Again, let us observe the progress of the symptoms in a humoral disease of more gradual formation; as, after the deposition of the syphilitic virus upon the glans. Trace it from its incipient stage of chancre, along the lymphatics till it terminates in bubo. All is yet strictly local; and the progress of the disease may be now arrested by remedies that are equally local in their effect. We will not say, generally so; but if in a single case, it is enough for the principle. (2) Or, surely, were the

(1) Lon. Med. and Phys. Journ. vol. 57, p. 342.

(2) As to the opinion of the humoral nature of syphilis, it has hitherto rested, in a measure, upon its supposed dependence upon a certain specific for its removal, originally surmised by Beringarius, (*a*) or J. de Vigo. (*b*) But it was early shown by David Abercrombie, (*c*) and afterwards by many well known English physicians; and more recently, by Chaussier, Caron, Larrey, Jourdan, Dublad, Keraudren, Brouissais, Devergie, Willaume, Desruelles, Desjardins, Delatour, Barthélemy, Kaisser, Handschuck, Ruft, Cullerier, Lucas-Championnière, and others, that syphilis may be removed by a simple dietary treatment. There is, then, no poison to be neutralized, no humor to be expelled, no "leaven" or "ferment" to be arrested. Whatever makes a full impression upon the vital forces of the solids enables nature to take up, through these agents, the process of restoration. Mercury is shorn of its magic powers, and its mode of action, may come, at last, to be considered in something like a philosophical sense.

Bleeding, purging, bathing, &c. were the remedies for lues, till Berenger, about 1500, introduced the practice of salivating by mercury. The blood was then, as now, supposed to be purified, renovated, and the poison carried off, by way of the salivary glands. But this remedy will cure lues without salivation. Again, it is

(*a*) Commentaria, 1521; and Astruc de Lue Venerea. (*b*) Practica, 1516.

(*c*) Tuta Cur. Lues Ven. absque Mercurio. Lon. 1684.

blood contaminated, we should, at least, have buboes, and other developments springing up simultaneously in various parts of the body. Gonnorrhœa, which never affects the constitution, illustrates the philosophy of lues by its coincident local results. The buboes, in either case, are evidently the result of continuity of inflammation along the absorbents; or, if there be hernia humoralis, it is as clearly the result of remote sympathy, or of an extension of inflammation along the mucous membrane of the vasa deferentia. In a process of time the morbific impression becomes extended, through the same principles, over the system.

In the same manner, local phenomena are first presented after poisoned wounds, whether from dissection or the bite of snakes; and it ought to be recollected, that, in the former case, "previously to the disease terminating either in death or in recovery, swelling or inflammation seize upon the portion of the limb interposed between the original wound and the first seat of pain." (1)

When constitutional symptoms appear, they are the result of the same laws of the constitution which operate in developing the general symptoms of smallpox, cow-pox, hydrophobia, &c., the latter of which will be a subject of analysis hereafter. These laws, especially in local diseases, are certainly liable to great modifications in their results. But, when we consider the close analogies which prevail amongst those intermediate cases of disease which connect the constitutional phenomena of simple inflammation with the more specific developments of smallpox, hydrophobia, &c., whatever may have seemed paradoxical disappears before these analogies, the specific nature of the poisons, the different organs affected, and the modifications to which the vital properties are liable.

Dr. Stevens has furnished humoralism with a case which re-

well known to have often failed, and even to have exasperated the disease, and, this in proportion to the excess of its action. For this reason, a resort was soon had to guaiacum, china-root, sarsaparilla, &c. all of which appear to have removed the complaint. Now is there any intelligible mode in which all the foregoing remedies, including diet alone, (*a*) should coincide in directly purifying and restoring the blood, were it the fountain of this specific disease. ? (*b*)

(1) Dr. Colles, in Dublin Hospital Reports, vol. 4, p. 246.

(*a*) We have, however, no doubt of the superior efficacy of mercury, and would not trust alone to other remedies. We employ them more or less in combination; especially diet.

(*b*) It is, also, not improbable that lues has undergone essential changes. Fracastor says that in his time, the disease differed greatly from what it was when introduced into Europe forty years before. (*Opera; et in Aphrodisiac. p.* 200.) The bubo and gonnorrhœa were first noticed by Fernelius about 1560. *In Aphrodis. sive de Lue Ven.* p. 613.

quires some notice; since, not only the fact, but the philosophy, has been appropriated by others. It is a striking exemplification of the spirit of hypothesis when it aims at nothing but its own success.

It appears, if only a small wooden needle be dipped in the "Wouralli" poison, and a puncture made with it in the flesh, "almost the *very moment* that the poison *mixes* with the living blood, it *destroys its vitality* in the part to which it is applied." But our author appears to be aware that the quantity of worara poison is not sufficient to contaminate, and "destroy the vitality" of the whole mass of blood. He therefore resorts to the leavening principle, and asserts that "this loss of vitality in one part of the blood, *almost instantly extends to the whole*, and the animal dies, generally in less than fifteen minutes." He then boldly affirms that there is "no increased action, or organic disease;" that, "we know that this poison does not possess the power of producing any direct impression on the nervous system;" "and there is not the slightest trace of disease to be seen in any part of the solid structure;" but says nothing of the symptoms. The conclusion therefore follows, "that the poison causes sudden death by the immediate destruction of the living principle of the blood."(1) It is the general opinion that "fermentation" is induced in the blood; since "a little leaven leaveneth the whole lump," and because "it is known to every housewife and fishmonger, that one spoiled herring will taint the whole cask." (See p. 419.)

What is to be done in cases of this nature, where the entire proof is thrown upon the respondent? Submit, or resist? Did our author show the truth, in the slightest respect, of any one of the statements in relation to the blood, any more than he shew that the solids sustained no injury from the poison? But what if there was "no increased action, and no organic disease," still "the solids perished;" and the affirmation is at least as good that they "perished" from the direct action of the poison, as from the morbific blood. Besides, the constitutional symptoms begin as soon as the puncture is made, and the "animal is generally dead in less than fifteen minutes." The experiment, therefore, is perfectly conclusive against the doctrine of absorption; and of this our author was probably aware, since he resorts to the expedient of assuming that "the loss of vitality in one part of the blood,

(1) On the Blood, &c. p. 143.

(the part which is pricked) extends almost instantly to the whole body."

The cases which we have formerly recited of the instant invasion of fever on exposure to its predisposing cause, the experiment of Johnson upon the ear of the rabbit, the instant death from a drop of hydrocyanic acid, or of strychnine applied to the tongue of an animal, afford a proper ground of interpretation for the foregoing case. Patients die at the invasion of fever, and in the stage of depression; but neither here, nor in the case of the hydrocyanic acid, &c., may there be any "increased action" or the least appearance of "organic disease." But "the solids perish,' and it may be, *prima facie*, as well from the direct action of the poison as the blood.

But, from the experiment, which we have related, by Sir E. Home in the case of the rat bitten by a snake (p. 488) we are warranted in the belief that the solids must sustain some organic lesion during the violent action, for fifteen minutes, of the worari poison.

The experiments of Sir B. Brodie with the foregoing poison, even admitttng its absorption, show conclusively that it produces its destructive effects, not upon the blood, but directly upon the vital forces of the solids. (1) We have, also, 26 experiments with two species of vegetable poison, similar to the worari, by Dr. Horsfield, at Java, which demonstrate the humoral assumptions as to the action of poisons upon the blood; and we shall state them in a note below. (2)

(1) Philos. Trans. 1811.

(2) These experiments show, 1st, the direct action of the poison upon the solids, since the symptoms began as soon as the puncture was made. 2d, by the development of organic lesions peculiar to each poison, in different parts; which also illustrates the different relations of the organic properties of different tissues to the properties of different species of vegetable poisons. 3d, the same is demonstrated by the activity of one upon the stomach, and the comparative inertness of the other; whilst this experiment is one of the thousand proofs that the worari and other poisons are not innoxious to the stomach from being digested, and tends, therefore, to establish the fact, that poisons are not absorbed by the stomach.

The poisons employed were the antshar and the tsbettik. The latter "is much the most rapid in its effects, more so than that of the upas. It acts by a sudden impulse, which, like a violent apoplexy, prostrates at once the whole nervous system." "A relative comparison of the appearances, on dissection, demonstrates, in a striking manner, the peculiar operation of each." "In animals killed by the antshar (which died in from 5 to 15 minutes after the prick,) the large vessels in the thorax, the aorta and vena cava, were, in every instance, found in an excessive degree of distension." And so the vessels of their neighbouring viscera. The brain but little or not at all changed in appearance. "It was very different with the tsbettik." "In many dissec-

The vitalist may bring an endless variety of parallel cases to show the improbability of the humoral nature of the foregoing. Thus, "a young woman had the nerve of her tooth touched with a heated iron. The pain became agonizing by evening, the cheeks swollen, great fever set up, and delirium supervened. Next morning the effects were dreadful; the cheeks and head were swollen to an enormous size; the nose and mouth scarcely perceptible, and the eyes closed by inflammation, never again to open." (1) Now we ask, what difference is there betwixt this case, and the first which we related of the bite of the viper? (p. 481.)

It will be admitted, we suppose, that when inflammation is excited by the puncture of a lancet, it is not done by irritating, or otherwise affecting, the blood; but that all the attendant phenomena are due to an impression made upon the solids, and their consequent morbid action. The inflammation, thus excited, may be extensively and violently propagated along the part, as in phlebitis, &c.; and it is but reasonable to suppose that the same condition is owing to a similar cause at one or six inches from the wound, as at the eighth of an inch. True, remote sympathies sooner or later come into play; but as we have no morbific blood in these cases, they strongly illustrate the power of sympathy, and serve as an index to the philosophy of analogous developments in diseases of reputedly humoral origin. We see, also, the blood first affected in the capillaries, — then the whole mass. But, besides the common effects of inflammation, the prick of the lancet may convulse the whole nervous and muscular system. And we suppose that traumatic tetanus will be also credited to the solids.

Let us now add to the point of the lancet sulphuric acid, or the virus of putrid animal matter, of the small, or cow-pox, or, the poison of the viper, of the worari, &c. Here are other irritating causes superadded to the mechanical. The general results, it is true, are more or less unlike in the several instances;

tions, the viscera of the thorax and abdomen were found nearly in a natural state. But the brain was in the highest degree injected, and showed marks of a most violent and excessive affection." These animals often died in a minute. This poison produced the same results when taken into the stomach, except in fowls, who then resist its operation. The antshar had not a fatal, though a severe effect, when taken into the stomach. The effluvia, like that of the rhus vernix, produces a severe cutaneous eruption, which is another evidence of their local action. (a)

(1) Waite on the Gums, p. 27.

(a) Trans. of the Batavian Society of Arts and Sciences, 1814.

but there are strong resemblances in the local phenomena, and in the progress of symptoms. The specific products, also, as well as other circumstances, denote specific modifications of a common pathological state. If, then, the mechanical irritation, in one instance, have acted directly upon the solids, is it not a proper conclusion from the progress and analogy of symptoms, that the several varieties of poison have done so in the others? It cannot be said that certain differences in the results imply a difference in the principle, since all those results where life is sufficiently prolonged, are purely secondary, and will be admitted to be consequent on the morbid affection of the solids. But all the primary phenomena in such instances are analogous, and have the same order of development. If the poison of the viper destroy life with great instantaneousness, this is conclusive against absorption, and is exactly allied in principle to the fatal operation of a blow upon the region of the stomach, or of surgical operations which produce instant death, or of the prick of a needle that is followed by tetanus.

Again, if we apply to the skin other irritants, and compare their effects with those which we have just considered, the principles upon which the former operate may be more apparent. The effects of cutaneons irritants are not less diversified than the foregoing poisons when inserted by wounds; yet they may all produce conditions of disease which belong to a common genus. Potass, for instance, operates with sudden intensity, and is destructive, *per se*, of organization; acids act with rapidity and violence, but each one presents some peculiarity in its results; mustard, cantharides, ammonia, tobacco, &c., modify the vital properties of the skin in a different manner, more gradually, and each in a way peculiar to itself; antimony, croton oil, &c., offer more remarkable peculiarities. These last agents carry us forward towards smallpox and cow-pox; and by their local and constitutional effects, which it will not be doubted, result from their action upon the solids, we arrive at the true philosophy which respects the operation of the virus in establishing those reputedly humoral diseases. We must look for it in the relations which subsist betwixt the peculiar properties of foreign agents and the nervous and organic forces.

In the same way might we go on with various other illustrations, all of which are only examples parallel with those morbific agents whose mode of operation is considered more doubtful,

because less obvious to the senses. But the understanding, of which a large sha physiological inquiries. In all the instan od of morbid developments, and the parti sults, depend mainly upon the specific causes. We have adverted to numerous a powerful action upon the nervous sys their application, — with such rapidity th the question. From these agents we desc poisons, which operate at more and mor we arrive at the hydrophobic virus, whi may occupy months in its incubation. mark of hydrophobia, that there is the success which attends the removal of th distant period from the infliction of the bi tained, if retained at all, at least during th where it is deposited. Or, what is far mo ogy, the virus, being a fluid and foreign structure of organized parts, is taken up, the circulation and is soon thrown out nothing remains but a specific modificatio ties of the part, which, as in miasmatic predisposing causes have been withdrawn, intensity, and silently extending its perni the whole system; till, having in either ca found lesion of the great properties of life, an explosion if we may be allowed the comparison, like the bursti boiler from the gradual accumulation of steam. The vir hydrophobia should be taken into the circulation near the t of the bite, since it is brought into contact with a thousand lacera- ted vessels, and must be even forced, more or less, into them. Or, surely, if not thus removed, or by the more natural process of ab- sorption, it will be thrown out during the frequent suppuration of wounds, which, indeed, are often artificially, though abortively, maintained in this state for some weeks. The occurrence of in- flammation in the bitten part is no proof whatever of the presence of the poison, no more so than the inflammations which spring up in other parts, or than the local developments which attend the invasion of miasmatic fevers, prove the continued presence of the morbific agents, or of the lancet when a vein inflames. But admitting the presence of the hydrophobic poison till the disease

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appears, shall it be assumed that after a long, though indefinite time, it is suddenly carried into the circulation, and then produces its effects by vitiating the blood; or shall we not rather be guided by those analogies which exhibit the operation of causes upon the living solids in a corresponding manner?

The history of sympathetic derangements removes all ambiguity as to any apparent exemption of the poisoned part from disease. We constantly witness, as we have already shown, violent constitutional disturbances from local injuries of a mechanical nature, where the affected part may exhibit few marks of disease; and although it be rationally supposed by many, that miasmata establish their primary impression upon the mucous membranes of the lungs or stomach, whence it is sympathetically propagated over the system, those membranes may remain exempt from all sensible marks of disease, whilst inflammations spring up in distant parts.

We see, too, that in mechanical injuries of the same kind, as in the prick of a lancet, different tissues sustain, at different times, the burthen of morbid action. At one time it is the vascular or cellular, and then we have the greatest marks of inflammation; and this may be propagated from the cellular along the absorbents to their glands, by continuous sympathy, just as the humoralists say that poisons are conducted along the same vessels. At other times, mechanical injuries are mainly felt by the nerves of a part, and there may then be an absence of inflammatory appearances, except, perhaps, a moderate vascularity if the nerve be large. But here the constitutional symptoms may be far more violent than in the former cases. (1) These last injuries generally lead to very speedy results; but again, varieties occur which throw no little light upon diseases of humoral notoriety. Thus, a dexterous phlebotomist, says Prof. Dunglison, wounded a nerve at the bend of the arm, in the month of October. The following April, the patient "had ever since suffered more or less from neuralgic pains of excessive severity, and from *modified nutrition* of the fore-arm, giving occasion to *irregular attacks* of hyperæmia." (2) We have stated numerous analogies in our 2d and 3d Sections, &c.

Just so is it with the poisons which we had been considering.

(1) See Sir B. Brodie's Lectures on Nervous Affections, p. 58, et seq. Travers on Constitutional Irritation, vols. 1 and 2. Both highly philosophical.

(2) American Med. Intelligencer, vol. 1, p. 22.

They relate strictly to the *vires vitæ* of the solids, and they appear to us to have no conceivable connection with the blood, unless they be sometimes mechanically intermixed.

In the foregoing cases, beginning with the mechanical causes, we pass from one to the other as a series of analogies, and along our way we come in contact again with hydrophobia, as produced by the canine virus. And here we find parallel examples which depend on causes which humoralism will not claim. The affinity has been shown in numerous instances to be strong betwixt this affection and traumatic tetanus; whilst, on the other hand, we have tetanus from causes which are said to vitiate the blood. Nor may the coincidence be neglected, that three eminent observers, Rush, (1) T. Percival, (2) and Girard, (3) without knowing the sentiments of each other, endeavoured to prove the identity of hydrophobia and tetanus. The last author has a variety of facts which demonstrate the local origin of hydrophobia, to which we shall only make this general reference. Again we have cited several authorities (p. 409,) (4) showing that symptoms resembling hydrophobia have been produced by affections of the mind, mechanical injuries, drinking cold water, fatigue, &c. (5) The best writers, however, as Hunter, Mease, Trolliet, Ferriar, Hamilton, discard the opinion that true hydrophobia can arise from any cause than the virus of a mad dog; and as this opinion is probably well founded, the simulating disease brings the reputed humoral one under the philosophy of solidism. It is also worthy of remark, that neither of the affections have any pathognomonic sign, either vital or anatomical.

(1) Med. Inq. Tetanus, vol. 1. (2) Essays Med. & Exp. vol. 2. (3) Ut Cit. infra.

(4) See, also, Rush's Obs. on Gout and Hydrophobia, p. 213; Cullen's Clinical Lectures, p. 173; Mease's Obs. on Rush's Arguments, &c.; Hamilton's Remarks on Hydrophobia, &c. 1798; Pinel, in Nosog. Philosoph. Girard Essai sur la Tetanos Rabien; and Réflexions sur le non-existence du virus rabique; Mr. Hunter in Trans. of Soc'y. for Improv. in Med. and Chir. Knowledge, vol. 1, p. 308; White's Doubts of Hydrophobia as a specific disease; Parry's Cases of Tetanus and Hydrophobia; Ward's and Reid's Treatises on Tetanus and Hydrophobia; Ferriar's Med. Histories and Reflections; Trolliet Nouv. Trait. sur la Rage, part 4th; also Cases in Lon. Med. Repos. and Rev. Jan. 1825, p. 69; Four fatal cases of Hydrophobia from bite of cats "not mad but greatly irritated," in Lon. Med. and Phys. Journal, vol. 23, p. 459; other cases in ibid, vol. 17, pp. 271, 457; vol. 19, p. 118. Rossi proposed a distinction between hydrophobia and canine madness.

(5) Moseley proved conclusively, that hydrophobia occurs spontaneously among dogs, foxes, and wolves. (*a*) And so Bouriat. (*b*)

(*a*) On Tropical Diseases, p. 48, &c., and his Treatise on Hydrophobia.

(*b*) Recherches et Réflexions sur la Rage, 1809. See, also, account of this epidemic in our New England States, in N. Y. Med. Repos. vol. 1, p. 370.

In hydrophobia, from the bite of a mad dog, as in many cases of traumatic tetanus, the poison may induce but a slight appearance of disease in the bitten part, but expend its force upon the nervous system. Still, it generally happens, when hydrophobia supervenes, that marks of inflammation or congestion spring up in the bitten part; (1) thus proving, conclusively, with other signs stated in our notes, either the local presence of the virus, or the incubation of disease in this particular part since the infliction of the wound. We cannot but repeat, however, that we do not believe that the fluid virus can have been retained any more than febrific miasmata, during a long interval of many months,—and therefore not for a shorter period of a few weeks. Nor can it be doubted, from the analogies thus supplied, that in other cases, if the nerves or adjacent tissues of the part were properly examined, there would be marks of injury at least equal to many cases of tetanus. This, indeed, was long ago observed. Sallin found the ganglions of the nerves in a state of inflammation and high vascularity: ("dans un état d'inflammation et d'engorgement sanguin.") (2) Multiplex experientia," says Autenrieth, "quæ docet hydrophobiæ semper quandam titillationem vel dolorem cicatrices præire, quæ ex vulnere tantum nec alio modo hydrophobiam oriri probat; et lymphatica vasa vel conglobatas glandulas non ut in alio contagio locali conspicue tumere. Casus in quibus 2â, 3â demum die, quum 5â post vulnus acceptum amputatio adhuc cum felici successu instituebatur. Omnia hæc probant, etsi contagio, variolarum ad instar, tempus quoddam necesse sit ad prodiendam insignem mutationem e vulnere tali venenato, tamen nec vasa sanguifera nec lymphatica, nec cellulosa, sed, sine dubio, *nervos ipsos viam* per quam letiferus effectus in universam agat corpus." (3) And thus Aurelian: "Præpatitur enim ea pars, quæ morsu fuerit vexata, unde initium denique passionem sumere nemo negat." (4)

(1) "The part that had been injured," says Dr. James, in his Review of Hydrophobia, "generally becomes painful, sometimes inflames and swells prior to the appearance of the dreadful symptoms which soon follow. The pain is commonly described as shooting from the part that had been wounded, towards the heart;" &c. (a) Dr. James considers it "a certain symptom of the presence of the poison, if the wound remains any time covered with a scab."

(2) Sur la Nature et la Siège de la Rage, 1783.

(3) De Prætervisa Nervorum Lustratione. Sec. de Hydrophob. p. 21. 1802.

(4) De Morb. Acut. l. 3. c. 14.

(a) Lon. Med. & Phys. Journ., vol. 21, p. 137.

So, also, Trolliet, (1) Sauvages, (2) Labonnardiere, (3) Girard, (4) and some others affirm, that pain in the bitten part is always the earliest symptom.

"About the same time," says Marshall, "that the irritation and pain broke out in the *hand* and *arm*, the heart began to be affected with *corresponding irritation*. I know not all the little intervening steps by which the irritation reaches the heart; but about this time, the action of the heart became quicker, and more frequent; and the irritation spread to the whole arterial system." (5)

But, who shall say, that in hydrophobia, dissection-wounds, &c. it is necessary that there should be some inflammation of the affected part to explain the constitutional results upon the principle of sympathy? The assumption cannot be defended on the ground of analogy, since it is fully proved that many causes establish violent inflammations, through the principle of sympathy, in parts that are remote from the seat of their application, though no mark of inflammation attend the seat of their more direct action.

A very remarkable case in exemplification of our views, as it respects the removal of the poison, the incubation of disease according to the analogies supplied by intermittent and other

(1) Nov. Trait. de la Rage, pp. 59, 70, 74. 1820.

(2) Diss. sur la Nature et Cause de la Rage, p. 52.

(3) Dis. sur la Rage et l'Hydrophob. *passim.* (4) Ut Cit.

The very valuable dissections by Mr. Surr are worth consultation. We refer to them, also, as nature is here seen free from all mental influence. Five horses were bitten by the same dog, and all died. The bitten part of the lip of the first horse was immediately cut out, and healed readily. When, however, the disease came on, "he rubbed his nose incessantly against the manger, or amongst the litter, and in the latter case seemed desirous of retaining his nose immersed in it. *This was a prominent symptom.* After rubbing a while, he jerked his head forward with great violence." "The rubbing continued during the disease, and to the last moment of his life." There was no examination of the second horse. In the third case, the bitten part of the lip "was cut out in about three quarters of an hour, and healed sound, without hardness or callus." On dissection, "the bitten part was thickened, and the disposition of the horse to rub his nose had been incessant." In the fourth case, there was "only a scratch" on the nose, which was washed and treated with caustics. The disease came on the 26th day, and, as in all the other cases, was very violent. "The bitten part was thickened." Of the fifth case there is no circumstantial account given; but the writer, who appears to have had no theory in view, remarks, in conclusion, that "the incessant rubbing of the bitten parts would appear to demonstrate some connection of the uneasiness there, with the other symptoms." (a)

(5) On Hydrophobia, p. 107.

(a) Lon. Med. & Phys. Jour. vol. 23, p. 1—13.

fevers, especially as it respects their periodical return for many years after the removal of the cause, and that the morbific influence is propagated from the bitten part over the whole system, (though we say not for what period of time,) may be found in a late number of the London Lancet. (1) In this instance hydrophobia supervened seven years after the bite. The time is long; but the facts leave no room for misconstruction. They only show us the necessity of abandoning speculation, and the contrivances of art, for the philosophy of nature. "The patient had not complained of illness until three days previous to his death." "A scar remained on the right hip from the bite of the dog. *The hip became excessively painful, and shooting, spasmodic twitches pervaded the whole leg.*" "Sixteen hours previous to dissolution, the most decided symptoms of hydrophobia were manifested," &c. This case, as we think all others should, from what we have said of the constant renewal of the blood and the rapid elimination of foreign substances from the circulation, settles, also, the hypothesis as to the early contamination of the blood by the hydrophobic virus. And since the humoralists dwell so emphatically upon the appearance of the blood in other diseases, we should not neglect stating, that, in hydrophobia the blood retains its natural colour, coagulates as usual, and has its natural proportions, &c. (2)

In another well attested case, where the bite was inflicted upon the median nerve, hydrophobia took place three years after; (3) and in another in two years. (4)

Whilst, therefore, we would make a positive contradistinction between hydrophobia from the virus of rabid animals, and from other causes, as well as tenanus, there are remarkable analogies

(1) February 3, 1838.

(2) See Liston, in Philos. Trans. Lon. vol. 13, p. 162, and later observers. Neither inoculation with the blood, or any other secretion than the saliva, has ever produced hydrophobia; and never the saliva of the human subject, and, probably, of the canine and feline tribes alone.

(3) Philos. Trans. Lon. vol. 91.

(4) Lon. Med. and Phys. Journ. vol. 20, p. 209. Philos. Trans. vol. 40, p. 5; Lon. Med. Journ. vol. 8, p. 156.

Analogous instances as to the intervening time may be found in Galen, l. Prorrhet s. 2, com. 17. Actuarius, Method. Medend. l. 8. Salius, de Affect. Practic. p. 360, several instances. Diascorides, de Venenis, l. 6, c. 3, 8. Al. Magnus, de Hist. Animal. l. 17. Act. Norimberg, t. 1, ob. 7. P. Salmuth, Obs. Med. Cent. 1, obs. 96, etc. Morgagni, de Caus. etc. Ep. 8, a. 21. Ephemerides Neo-Cæs. an. 7, Obs. 148. And so of later times. Some of these instances are probably fabulous; whilst others are well authenticated.

amongst them, which can only be interpreted upon a common principle. We have seen, indeed, that the analogies are so great as to lead eminent observers to confound the diseases. (1)

We have endeavoured to indicate the distinction, perhaps sufficiently obvious, betwixt those agents which affect the vital forces, as of the stomach for instance, by their superficial action, and such as are innoxious till they are brought into contact with the intimate organization of a part. Of the latter description is the virus of hydrophobia, which exerts no effect, either directly, or by absorption, when introduced into the stomach. This, as we have said, is equally true of many other virulent poisons; and furnishes a strong proof against the general doctrine of absorption and the humoral pathology. (2)

It is said that the hydrophobic virus "may enter the circulation still more rapidly by means of the veins than the lymphatis;" and it is then suggested that "the lividity described about the cicatrix at the *commencement* of the disease seems to point out this as its actual course. (3) We cannot help thinking that the solidists are especially entitled to this admitted proof of the local state of the disease at its "commencement." The virus is at least conceded, by the force of facts, to remain in the spot where it was deposited, till the explosion takes place. But there is not the slightest proof offered of its absorption at this period; whilst we have seen that it is opposed to all analogy to imagine that a fluid virus may remain in the interior of a thousand minute vessels, — veins and absorbents, — for months, and perhaps

(1) It should not, however, be neglected, that from immemorial time, it has been said that "the bite of any irritated animal has some venom in it." *Celsus, l. 5, c. 27, p. 307.*

(2) We are aware, indeed, that the humoralists once endeavoured "to maintain that the *volatile parts* of the saliva, being carried off with the breath of the animal, have been capable of producing hydrophobia, when received into the stomach or lungs of any person." But this only shows that the facts of our friends will not bear examination. The opinion has probably been handed down from Aretæus, (*a*) as a matter of oral tradition. J. Palmarius goes a little farther; having stated that an entire family took the hydrophobia by kissing their father when he was expiring of that disease. (*b*) Such is the origin of many of our medical prejudices. We cer-

(3) Cyclopædia of Prac. Med. Art. Hydrophobia, p. 502.

(*a*) De Caus. et Sig. Acut. Morb. l. 1, c. 7. "Quinetiam a rabida cane, qui in faciem, dum spiritus adducitur, tantummodo inspiraverit, in rabiem homo agitur."

(*b*) De Morb. Contagios. p. 185, 1578. Matthiolus, (*Comment. l.* 6.) Aurelian, (*de Morb. Acut. l.* 3, *c.* 9,) Hildanus, (*Cent.* 1, *ob.* 86,) Cardan, (*Contradict.* 9, *tract.* 5, *l.* 2,) Hamilton, (*On Hydrophobia, p.* 22,) endeavour, also, to show that hydrophobia may follow the application of the virus to the skin. But, it is well ascertained that the poison may not only be taken into the mouth, but swallowed with impunity.

for years, and then start off on a sudden into the system. We cannot doubt that it is partly in consequence of absorption by the lacerated vessels, and removal of the poison from the system, that only about a twentieth (some say a twenty-fifth) part of the human species, that are bitten by rabid animals, suffer an attack of hydrophobia. (1) The poison, in its transit through the circulation, is so largely diluted as to be rendered innoxious. The impression, however, in susceptible constitutions upon the nerves and the organic properties of a part, which forms the predisposition, may be the work of a few hours only; and this may be analogically, and most philosophically, supposed to remain in a state of incubation for a long and indefinite time; certainly with much more reason, than that the poison is neither carried off by absorption, nor discharged by suppuration. And it is equally correct to infer, that the morbific influence is early propagated from the part over the whole system; so that the process of incubation is simultaneously going on in every part. This will account for the remarkable variety of lesion, scarcely in two successive cases alike, which subsequently and suddenly springs up. (2) This gives us hope, that art may yet come more efficiently to the aid of nature, and that constitutional means may not be unavailing during the calm, but portentous period of incubation.

But we have something still more in point about this question of ultimate absorption. "A case is quoted," says Beddoes, "of

stainly trace farther back some of our best opinions, that we had been once apt to ascribe to a more modern date.

(1) That the exemption is owing to the alleged removal of the poison by the dress, is clearly disproved by the very minute quantity which has been known to produce the disease. Thus, Hildanus relates a case which fell under his observation, where the virus was communicated by a very slight scratch of a cat, ("a fele rabiosa scalpturam in dextro pollice accepit; ubi *epidermis vix offensa* fuit.") (*a*) Many months afterwards the patient was attacked with all the symptoms of hydrophobia, and died in three days.

Again, a boy died four months after receiving "from a little dog only a rasure not larger than the scratch of a pin." Moseley on Trop. Dis. p. 49. See, also, a similar case in New-York Med. Repos. vol. 1, p. 550; and another, where there was "a mere scratch near the toe;" (ibid. vol. 5, p. 292;) and, again, a case of about as slight an injury, in which hydrophobia supervened at the distant period of fifteen months. (Ibid.)

(2) It also explains the reason why we look in vain for inflammation and soreness of the auxiliary glands; since there is neither absorption, nor any particular affection of the lymphatic system.

(*a*) Obs. Chir. Cent. 1, obs. 86.

double absorption! the bite immediately festering, and an axillary gland swelling on the fifth day, with considerable fever. But no hydrophobic symptoms occurred till the usual period, and after the signs of a *second* absorption; which signs are said to be *pains about the wound rising towards the head or body.* The fact seems to me to concur with others in rendering it probable that the lymphatics are not the agents in generating hydrophobia. We have here a strong show of absorption, but no disease whatever like hydrophobia in consequence. If the part must be first changed, why go farther than such *local* change?"(1)

Although we believe the poison to be early carried off, we will not so far renounce our philosophy for the sake of any advantage, by supposing, that in the foregoing case, the virus produced the "swelling of the axillary gland, and the considerable fever on the fifth day." We believe that those results were a mere matter of sympathy, proceeding originally from common inflammation of the wound. But we must take the humoralist upon his own ground; and if he contend for the absorption in explaining the axillary inflammation and fever, he must give up absorption as the cause of hydrophobia; or, if he maintain the latter, he concedes our proposition as to the former; and we shall then want no better analogy for establishing our whole conclusion.

An instructive and well authenticated case is related, where the bite upon the face was severe, and the subsequent suppuration was large till the 11th day of the injury. On that day hydrophobia came on. Now mark the symptoms attending the wound. "Wednesday (11th day) morning, on removing the dressings, the *wounds were dry without any discharge, notwithstanding the plentiful discharge which appeared on the dressings the preceding morning.* There was *pain* in the wounded eye and in the head." In the afternoon hydrophobia appeared.(2) On the third day the patient died with all the

(1) On Fever and Inflam. *Note.* And Trans. of a Soc'y, &c. vol. 1, p. 318—319.

(2) New-York Med. Repository, vol. 1, p. 327. 1797.

In a case by Professor Willoughby, "the whole catalogue of symptoms came on in about 48 hours after the bite."—(*Ibid.* vol. 12, p. 135.) This and the foregoing instance illustrate what we have said as to the difference in the susceptibilities of the vital properties of different individuals in relation to the hydrophobic virus; which is exactly coincident, in this respect, with what is known of the operations of miasmata. Women and children are most susceptible, and are earliest affected; and animals more so than the human species. Again, it is affirmed by Dr. James that some people are absolutely insusceptible of hydrophobia; and that is coincident with what is

horrors of the disease. We may also refer to Dr Johnstone's *remitting* case of hydrophobia, (1) as illustrating the views of the solidist under other very remarkable aspects.

We need not say that hydrophobia has never been cured, or mitigated, by any remedy which can have any possible action upon the blood; and that in all cases where relief has been obtained, it has been through the agency of means whose operation can be alone explained on the principles of the vitalist. In respect to bloodletting, whoever agrees with us in our philosophy of its operation, will have, as we presume to think, an intelligible manner in which it relieves hydrophobia. We shall also have occasion, when speaking of the action of opium, to adduce an instance of relief, "in scarcely one minute," of hydrophobic symptoms, by the application of acetate of morphine to a blistered surface; thus proving our principles in relation to the disease, and the local action of the remedy. But, in passing along, we may refer to a case by Southwell, where a man, affected with hydrophobia, from the bite of a rabid dog, was tied to a tree and cured by 200 buck-

known of small and cow-pox, measles, &c., all of which facts we hold to be fatal to the humoral pathology. But, perhaps, in nothing is this more impressively shown, or the humoral pathology more severely rebuked, than by the case of the distinguished Dr. Robert White; who, after having been bitten by a mad dog, and refusing all means of prevention, inoculated himself with the virus with impunity, and was still willing to become the subject of farther experiments. (*See Op. Cit.*) Mr. Hunter mentions an instance where 20 people were bitten by the same dog, and only one had any disease. This is common. At other times the proportion is vastly greater, and apparently more so from the bite of wolves than of dogs. Thus, we find, of two persons, and a vast number of cattle bitten by the same wolf, all died. Again, of 5 persons, 4 died, and all the oxen, cows, and dogs, bitten. Again, of 3 persons, all died. Again, of 5 persons, 4 died. Again, of 10 persons, 9 died. Again, of 15 persons, 8 died. Again, of 24 persons, 18 died. Again, of 21 persons, 16 died. Again, of 19 persons, 12 died. All were bitten, in each instance, by one wolf. Probably not more than 1 in 18 or 20 persons contract the disease from dogs. We think the intelligent reader may apply the facts to our purposes. Constitution, and activity of poison, are manifestly concerned; whilst as we have said, in our text, the exemption from contagion is, doubtless, in part owing to an early removal of the poison by suppuration, &c. If we suppose Dr. Willoughby's case not to have proceeded from the specific virus, as we are rather disposed to think from the great brevity of the *time*, the *recovery* of the patient, and the sickness of the dog being probably *spontaneous*, (neither of which, however, alone would form an objection,) (a) with this view solidism is equally contended; and the case will arrange itself along with those at pp. 409, 498.

(1) Edin. Med. & Surg. Journ.

(a) Hamilton says the disease does not occur before the 19th day, nor after 18 months; but he is clearly mistaken as to both periods.

ets of cold water poured upon him. No other remedy was employed. (1)

After all, however, such have been the demonstrations of high morbid action in different parts of the body, that humoralists have constantly overlooked the general hypothesis when they have approached the morbid anatomy of hydrophobia; and as one or another have happened to find different parts the seat of organic lesions, they have made various, though individual, parts the seat of the pathologic cause; just as many solidists suppose the glands of Peyer to be the seat of typhus fever; or diseases of the liver, or, again, of the kidneys, &c. to be the universal cause of dropsical affections.

Müller, like all humoralists, affirms that "it is certain that the general effects of poisoning depend, not on nervous communication but wholly on the noxious substance entering the circulation." (2) But he supplies no proof that renders it even probable. It is a matter of conjecture, not of demonstration, even by any well ascertained analogies; whilst we have specific facts, and analogies without end, that disprove the general doctrine of absorption. Most of our author's evidence consists in a detail of experiments with the prussiate of potass; and allusion is made to Emmert's oft recited experiment, "in which, after tying the abdominal aorta, he introduced prussic acid into a wound of the foot, when no

(1) Medical Essays, vol. 3, pp. 93, 94.

(2) Elements of Physiology, vol. 1, p. 242.

Christison, who believes that when "vegetable acrids act only in large doses," they enter the blood, considers the "inference correct, that the impression is conveyed to distant organs, by the nerves, in regard to such species as act in small doses," whether through the stomach or a wound. But whilst all analogy, as well as the local and early manifestation of symptoms are opposed to the reservation, there is no other ground for the conjecture, than the results of chemical analysis, which, in respect to vegetable principles, are entirely worthless. But, admitting that small quantities are absorbed when the dose has been large, the detection of the poison in the blood only proves its mechanical mixture with that fluid, and brings, therefore, a greater strength to solidism.

When Orfila applied vegetable poisons to a wound, "in every instance active inflammation was found in the wound, extending to the limb above it and even upward on the trunk." "The appearances, in short, were precisely those of diffuse inflammation of the cellular tissue, when it proves fatal in its early stage." "There is no doubt," Christison continues, "but many plants of this order produce violent spreading inflammation of the subcutaneous cellular tissue, and acute inflammation of the stomach and intestines, without entering the blood; and death is the consequence of a sympathy of remote organs with the parts directly injured." (a)

(a) Christison, on Poisons, pp. 438, 439.

effects had ensued even in seventy hours; but, on loosening the ligature on the aorta at the end of that time, the symptoms appeared in half an hour." The tying of the abdominal aorta could certainly have had no possible tendency to exclude the poison from the circulation. It would only prevent its distribution to comparatively unimportant parts,—not at all to the brain, the heart, lungs, &c. "These anomolies should be defalcated." Müller furnishes several examples of his opinion of the propriety of doing so. Besides, experiments like that of Emmert prove nothing in the way of absorption from external surfaces, since the poison is introduced directly into wounded vessels. But we shall examine this subject more analytically, and in proportion to its practical importance.

Again, Müller, and others, have endeavoured to show that poisons must enter the circulation before they can exert their general effects upon the nervous system, by comparative experiments upon the trunks of nerves and upon organs where the nerves are expanded. For instance, "the spiritous solution of extract of nux vomica, introduced into the mouth of a young rabbit, produces immediate death; whereas, when applied to a nerve at some distance from the brain, as to the ischiadic nerve, it produces no general symptoms. Concentrated prussic acid, also, as Wedemeyer observed, does not exert its poisonous influence when applied merely to a bare nerve." (1)

Here, then, it is entirely overlooked, that the properties of the nerves are especially seated in their extremities; nor is a due consideration given to the manner in which the sympathetic nerve is involved in the intimate organization of a complex part, and the very different relations of such a part to the whole system, when compared with the mere trunk of a nerve. The whole philosophy is manifestly imbued with our author's hypothesis about the presiding genius of the nerves in the organic processes. Nothing is allowed for the action of poisons upon the independent organic forces of the most vital parts, (2) upon which,

(1) Müller's Elements of Physiology, vol. 1, pp. 242, 246, 247, 627, 628.

Fontana went farther with this experiment than Müller admits of his own; having inserted the poison into the texture of the nerves. The results only confirm the principles in our text. The absence of violent symptoms shows there was no absorption, whilst it corresponds with what is known of the physiology of the trunks of nerves.

(2) It is important to remark here, that irritability and other *organic* properties are much less strongly pronounced in the large nerves than in many other parts; whilst

with every deference to our distinguished opponents, we cannot doubt that primary impressions are even more determined than upon the peculiar endowments of the nervous system. And we believe when pathological anatomy shall have exhausted its day, and philosophers shall have again returned to the consideration of the properties of life, the science of physiology will soar higher and brighter than ever; prostrating in its way the exclusive neurological systems, the grotesque combinations of vitality and physics, and, above all, will plant an indelible mark upon the profane attempt to identify the powers and processes of life with those which distinguish the inorganic world.

But the foregoing experiments prove exactly what they were not intended to. For, it is quite apparent, that if "a small quantity of a solution of nux vomica" produce "immediate death" when introduced into the mouth, there is no time allowed for absorption. The constitutional symptoms begin the moment the solution touches the mouth, and with all the apparent instantaneousness of a shock of electricity. Prejudice cannot disguise the fact, that absorption, intermixture with the general mass of blood, &c. require an interval of time that shall enable the mind to distinguish between the moment when the application is made, and when the symptoms begin.

Fontana observes, "how little certain are we in things we believe ourselves the best skilled in, and in which we have the least apprehension of being mistaken! I know but one class of people who never err; those who do nothing, observe nothing, and make no experiments. All others are led into errors; and the more experiments they make, the more are they deceived."(1)

We agree with Fontana in the spirit of the foregoing propositions; confining ourselves, however, in respect to the last, to the majority of physiological experiments. Of this truth Fontana furnishes in himself a most remarkable example; and as it is exactly applicable to our present object, we shall give it in the words of Girtanner for the sake of his comment.

"Mr. Fontana," says Girtanner, "has made more than 6000 experiments upon the poison of the viper; he employed more than 3000 vipers, and caused to be bit more than 4000 animals;

they are exquisitely developed in the nervous filaments. Or, they may acquire in the trunks of nerves, when disease takes place, especially inflammation, a very exalted activity; just as in tendon, &c.

(1) On Poisons, vol. 2, p. 73.

and the conclusion he drew after this truly enormous number of observations was, that the poison of the viper kills all animals, and *produces the disease by its action on the blood.* It is well known that frogs, and many animals with cold blood, live a long time without the heart, and entirely deprived of blood. If, therefore, the poison of the viper kills animals by its action on the blood, it will not destroy frogs without blood. But experiment contradicts this reasoning. *The poison of the viper will kill frogs without blood in as short a time as it kills those animals who have not lost their blood.* Thus does a single experiment frequently overturn all that 6000 other experiments had apparently established." (1)

Here, perhaps, we ought to stop. The foregoing experiment is, of course, of universal application, and sets aside all observations and inductions of the nature of Fontana's, or where the stomach, skin, &c. have been the medium of operation. But the revival of humoralism admonishes us, that "e'en tho' vanquished it can argue still."

Another experiment by Emmert has been often brought up in proof of absorption, and of the humoral pathology. He amputated the leg of an animal, leaving it connected with the body by means of the nerves only; when he found that poisons applied to the limb or nerves produced no effect. But what was the condition of the nerves, and what sort of vitality, or vital action, existed in the limb thus severed from the body? The experiment, also, proceeds upon the hypothesis that the nerves are alone interested in receiving morbific impressions, which we think we have shown to be a capital error. All such observations are clearly of the nature of Magendie's, which led an eminent reviewer to say of them, that "they have tended to confuse the plainest truths, and to establish the most extravagant fancies." (2)

If Viborg failed of producing any effect by applying a drachm of prussic acid to the brain of a horse, it only confirms what we have just stated as to the nervous system. But this experiment, which is quoted by the humoralists, proves too much for their purposes; since it shows that poisons, in a general sense, are not absorbed by the natural surfaces, and that when inserted under the skin, &c., they are mechanically introduced into the circulation. We will not, however, for our own advantage,

(1) On the Laws of Irritability. (2) Med. Chir. Rev. Lon. vol. 32, p. 133.

countenance what we believe to have been an oversight in Viborg. The experiments of Wilson Philip, Brachet, and others, with a variety of applications to the brain, show the probability that Viborg's acid was not good. But admitting it to have been genuine, the experiment is utterly fatal to the humoral doctrine, and beautifully demonstrates the theory of life, and of the modifications of organic properties, as entertained by the vitalists.

But we have enough of absolute fact to sustain our whole ground, and to rescue some of the most important principles in physiology. Müller, and others, as we have said, affirm that narcotics applied to the trunks of nerves, incised or entire, produce no constitutional effects. The reason of this we shall have explained on physiological grounds; so that the experiments establish the great principle that poisons operate according to the relation of their properties to the peculiarities of the organic forces of particular parts. Secondly, they show that poisons do not operate by absorption. Thirdly, they prove the error of physiologists in supposing that the poison of serpents, of hydrophobic animals, &c., is innoxious when swallowed because it is decomposed by the mouth and stomach. (See p. 484, & note.) Upon exactly the same grounds might we affirm that the urine is decomposed by the bladder, because this part is exempt from its corroding effects, whilst it is destructive to all others; and, upon precisely the foregoing principles, which apply in all the cases, do we interpret the immunity which appertains to the bladder. All the experiments and inductions drawn by Müller and his school, as to the absorption and action of poisons, are of the most conflicting nature.

The celebrated experiment in which Magendie completely amputated the limb, and substituted quills for the vein and artery, to show that upas poison is carried into the circulation, (1) is liable even to greater objections than what we have just offered of Emmert's. What will the nervites say to the necessity of nerves to absorption, and other organic actions? Admitting, however, the perfect accuracy of the experiment, it can only be regarded as proving the mechanical introduction of the poison into the blood, — not of its otherwise contaminating that fluid. (2)

(1) Précis Phys. p. 239. And Journ. de Physiol. t. 1, p. 18.

(2) As the poison was inserted "enfoncé dans la patte," it was directly, and mechanically forced into the circulating mass of blood. In whatever light, therefore, the experiment may be regarded, it proves nothing but the fallacies upon which humoralism is founded.

We will not, however, dwell upon this experiment, after what we have stated of Magendie in our first section, and what we shall say of other experiments of this remarkable man hereafter. (1)

"It is one of the most difficult things in physiology," says Dr. Hall, "to devise an unequivocal and unexceptionable experiment." (2) And in Magendie's opinion, "the results of physiological inquiries have not been in proportion to the number of authors who have written on the subject. On the contrary,

(1) Again, it will be recollected that "Magendie determined by experiment that the fifth was the nerve of smell; by experiment, he proved that an animal is under several antagonizing impulses, — to go forward, backward, laterally, to revolve; by experiment, he concluded that there was a fluid in the spine and in the ventricles, which, becoming cold, produces ague. In short, there is no fallacy, no heresy, that cannot fall back on experimental facts." (*a*)

Müller, in objecting to an experiment of Magendie, remarks, that "the circumstance of an animal not being susceptible of other impressions immediately after the division of so large a nerve as the fifth, proves nothing more than that it had suffered a serious injury." (*b*) May we not affirm as much of many of Müller's experiments, in which equal injuries, at least, were inflicted. Where humoralism is concerned, the laws of physiology are sacrificed; and our author, on such occasions, quotes and sanctions Magendie's and Emmert's absurd experiments, to which we have just referred in our text; whilst there are others of his own not less objectionable. (*c*)

"I approach the labours of Magendie," says Mr. Grangier, "connected with the nervous system, with much regret; because I feel myself compelled to state, that so far from admitting that they tend to remove the veil which obscures the operations of the brain, I conceive, with some of the most profound physiologists of Europe, that viewed in the aggregate they have constituted the great barrier to the progress of modern physiology." "It is apparent that his mutilations were practised with no definite object, but with the design of wresting, from their results, some conclusions respecting the most mysterious phenomena of the animal frame." (*d*) And yet, according to Christison, "Magendie confesses and complains, that physiology is still with many a work of imagination, and that every step of the student is on debatable ground." (*e*)

Finally, our author, as we think, has received a proper, though excessively severe rebuke, in a late number of the Med. Chir. Rev. (*f*) We see not how his experiments, or inductions, can be admitted into the records of medicine. The reviewer concludes with the admonition, — "let the medical students, and the junior practitioners, beware how they embrace M. Magendie's tenets, or act on his principles. If they do not, they will sacrifice their own reputations, and, what is of more consequence, the lives of their patients."

M. Magendie has been one of the most active instruments in the revival of the humoral pathology.

(2) On the Powers which circulate the Blood. Introduction.

(*a*) Med. Chir. Rev. Lon. vol. 20, p. 324.
(*b*) Elements of Physiology, vol. 1, p. 770. (*c*) Ibid. pp. 238, 242, 245, 246.
(*d*) Observations on the Structure and Functions of the Spinal Cord, p. 68.
(*e*) Edin. Med. and Surg. Jour. vol. 35, p. 95. (*f*) January, 1839, p. 203 — 211.

you are disturbed by contradictions of the most glaring nature, or bewildered between a mass of theories and explanations which have no existence but in the minds of the inventors." (1)

The foregoing facts show us, also, by their variety and universal concurrence, how safe it may be to infer a general law of nature from a few well-determined observations. What Diascorides said of poisons, which are remedial in certain quantities, may be affirmed of them when they prove morbific; viz: "they are numerous, but the changes they produce in the body are but few, and they operate on a common principle." (2) These effects, however, although they manifest resemblances, will yet be varied according to the tissue affected, the natural and modified constitution of individuals, but certainly most so in many cases according to the peculiarities and combinations of the remote causes. An act of intemperance may produce gout in one, phrenitis in another, delirium tremens in another, fever in another, &c. Cold applied to the surface of the body may suddenly produce pneumonia, enteritis, catarrh, tuberculous inflammation, gout, rheumatism, &c. Again, when cold water is drank, in a heated state of the body, it often destroys life suddenly; and so it has when applied to the surface. (3) Just so with the poison of serpents, which may destroy instantly, or "frequently induce peripneumony." (4) The causes, which produce common inflammation in one constitution, develope scrofula, cancer, &c. in others; and the same causes produce different modifications of action, simultaneously, in the same individual; whilst we have the remarkable fact of diseases of an apparently distinct nature running into each other; all of which require for their interpretation the principles of solidism. The modifications of effect,

(1) See Wardrop on the Nature and Treat. of Diseases of the Heart, &c. p. 100.

May it not be often said of Magendie's vivisections, and his starving of animals upon a most unnatural food, that he "imitates the methods of those ancient judges, who put the delinquents to the torture to make them confess their guilt; but which delinquents, when stretched upon the rack, often give false information"?

In one of his late publications Magendie affirms, that "at the present day, no physiologist doubts that the venous system is the apparatus of absorption." Whereupon the Med. Chir. Rev. (vol. 32, p. 527,) remarks, that "the assurance of M. Magendie in many of his assertions is somewhat staggering, at first, to the cautious inquirer after truth."

(2) Ποικιλα μεν γαρ τα δηλητηρια φαρμακα, κοινα δε και ου πολλαι αι εξ αι των γενομεναι διαθεσεις. — *Alexipharm. et Theriaca*, p. 398.

(3) See Currie's Med. Reports, p. 96. And Franklin on the Art of Swimming. "Two men died on the spot," from plunging into cold water in a heated state.

(4) Laennec on the Chest, p. 225.

according to the nature of agents, are well shown by the results of different kinds of miasmata, as in intermittents, plague, typhus and yellow fevers, the malignant cholera, beriberi, dysentery, epidemic influenza, &c.; or, still more strongly in the specific contagion of smallpox, measles, lues, &c.

An infirmity of health of many years duration has compelled us to suspend our labours for a few weeks past, during which we have met with the experiments with the wourali poison under the direction of Mr. Waterton, in England. The only one which affects our inquiry relates to the exemption of an ass from the effects of the poison for the space of an hour; a ligature having been "bound very tightly around the leg" above the insertion of the poison. The ligature being then removed, the animal died in ten minutes.(1) Allowing entire accuracy to the experiment, from what we have hitherto said we must refer any immunity which the animal may have experienced to the pressure of the ligature upon the nerves supplying the superficial parts, and to the inactivity of the vital powers consequent on the entire suspension of the circulation. Magendie has some speculations in his late lectures upon experiments of the foregoing nature, and, of course, renders them subservient to his mechanical and humoral doctrines. The British and Foreign Med. Review for Oct. 1839, in adverting to them, makes nearly the same objection that we have applied to Mr. Waterton's experiment; though, for the benefit of this coincidence in opinion, as well as to avoid the imputation of appropriating the views of others, we would say that our own remarks were written before the Review had reached this country.

In respect to what we have said at p. 484 in refutation of the conjecture that the wourali and other poisons are decomposed by the stomach, it should be equally true that they are decomposed by the brain, since they are innoxious when applied to that organ; which will hardly be maintained. *See Viborg's Experiment, p.* 509.

The introductory article of the foregoing Review is an elaborate and powerful critique upon the physiological experiments and inductions of M. Magendie. The concurring testimony against them has now become so great and universal, that we have no doubt that physiology will be disenthralled from some

(1) Brodie performed nearly the same experiment. See Philos. Trans. 1811, p. 178; 1812, p. 167.

important drawbacks which it has sustained for the last quarter of a century. The habits of that distinguished man have pervaded too extensively the walks of medicine. The evil is now sapped at its foundation, and the superstructure must gradually crumble. Art will soon be supplanted by the former usage of observing nature, and we shall come to understand the reasons why, according to M. Louis, "medicine is now in its infancy."(1) See p. 511, *Note*.

(1) We shall quote a few remarks from the British Review, for the purpose, mainly, of showing that physiology is reverting to the philosophical era of Hunter and Bichat. Britain is conducting the great reformation.

"To distinguish the *physical* phenomena of living bodies from the *vital*, and to illustrate the former, and refer them to the laws from which they result, are the proposed objects of M. Magendie in the course of lectures before us." "In the course of the present work we meet with too many proofs that a man may possess great ingenuity and considerable acquirements, with very small capacity — we had almost said, a total incapacity — of sustained reasoning. Led astray by a predominating idea, M. Magendie continually loses sight of the distinction which he admits as the basis of the inquiry, and deviates from the path he has himself prescribed. Instead of carefully separating the physical phenomena of life from the vital ones with which they are so intimately associated, his chief anxiety seems to be to make out as many physical and as few vital as possible."

"The mode of investigation consists chiefly in experiments, by which it is made evident to the senses that certain phenomena take place under certain circumstances; but little pains are bestowed to ascertain what would have taken place under other circumstances. The fact auspicious to the preconceived view is seized upon, and a hasty generalization set on foot. Indeed, we feel ourselves obliged to declare, that we know not, in the whole range of modern scientific literature, a work which contains so frequent violations of the inductive principle, or such rash generalizations from isolated facts, as that before us."

"We feel convinced that an animal mortally injured, in which every action of life is fast ebbing, and the organ immediately experimented upon placed *suddenly* in circumstances incompatible with the continuance of its functions, can, generally speaking, afford no just solution of the phenomena of life, whether vital or physical." The same affirmation may be made of any individual part where the functions are interrupted in a like manner.

"From the effects of alterations in the consistence of the blood, M. Magendie draws certain conclusions, which, if established, would render null and void at least one half of the science of pathology as it now stands." This half we suppose to be the ground which is maintained by the vitalists. We shall have sufficiently shown, however, that all the work was done before Magendie attempted this *coup de grace*. Nevertheless, we rejoice in so powerful an auxiliary as our reviewer. In speaking of a certain general principle, (that "an universal cause must exist and have an universal effect,") which is justly admitted by M. Magendie, the reviewer nearly agrees with us, "if he had kept it always in view, he would have seen that no local diseased action can be attributed exclusively to the state of the blood, although the effects of a morbid state of the blood may be made to manifest themselves in one part rather than another, by particular diseased conditions of the solids."

SECTION IX.

WHEN poisons are introduced into the circulation, in *certain quantities*, they excite disease in as much, if not greater, intensity, than when applied to the surface of organs; and this is true of very many substances which are otherwise perfectly innoxious. All this has been well understood from the earliest ages of medicine. But the most indefensible experiments have been multiplied in recent times, by injecting a vast variety of noxious substances into the veins, for no other apparent purpose than to extort from their effects some justification for the revival of the humoral pathology. That they must excite disease in the solids would seem to be sufficiently obvious; and the solidist looks on in silent astonishment, that there should have been any doubt as to the result. Yet, strangely enough, the numerous experiments of this nature have proceeded upon the foregone conclusion, that the morbific agents must, in the first instance, produce a diseased

The essential principle, which is here laid down, must be admitted, as it is, indeed, by Magendie himself. It proves, therefore, fatal to the whole humoral doctrine in respect to all local diseases.

In regard to that part of Magendie's lectures which relates to THE BLOOD, the reviewer agrees entirely with what we have seen of the Medico Chirurgical, pp. 398, 511. "No portion of the present work," says the writer, "presents a more remarkable illustration of our Bacon's 'Idola Specus,' than these lectures on the blood; and we think we may venture to affirm, that saying this is equivalent to saying that no better illustration is to be found in the whole course of modern medical literature."

And so, also, Mr. Macaulay: "His" (Magendie's) "mind is, indeed, counteracted by prejudices, derived from his peculiar pursuits, so that in a coarse and illiberal manner, he affects to despise and ridicule the researches of pathologists; and he is constantly falling into errors from rash generalizations, founded upon observations of the animal economy in unnatural states." (a)

With the weight of testimony which we have now and hitherto produced against the experiments and inductions of M. Magendie, we shall regard them less than we might have thought it otherwise necessary. The facts and arguments may be found in the authors to which we have referred. They are of great moment, and will be read with interest.

In reference to what we have said of the staggers, at p. 411, we may quote from the foregoing Review that it is stated by Dr. Malin, that "a horse affected with the staggers was radically cured after it had received from its master a heavy blow on the head with a hammer, with the intention of killing it."

(a) Essay on Cruelty to Animals. 1839.

state of the blood, and that this fluid will then become the cause of disease in the solids. The results of these experiments, and the supposed rationale of their operation, are then assumed as the great basis of the humoral pathology.

But we should, perhaps, state the conclusions in the language of the most acknowledged authority. Thus, then, M. Andral, who, assuming the foregoing results as to the blood, proceeds in a manner not less remarkable for its comprehensive brevity, to declare that "those derangements of functions and organs produced by the experimenter, when he introduces different deleterious substances directly into the blood, *are likewise those* that are produced by the sting or bite of certain animals; they are also those that take place from touching the flesh of animals that die of the plague, as well as those observed in smallpox, measles, and scarlatina, of a malignant nature, as it is called. They are *the same derangements* that appear in persons exposed to putrid emanations, vegetable or animal, and to miasmata from the bodies of other persons that are themselves diseased, and crowded in confined places, &c. Lastly, they show themselves, also, in individuals, whose blood is only imperfectly or badly repaired by insufficient or unwholesome diet." "The source and primary seat of typhus fever, properly so called, is proved to be in the blood, inasmuch as it is caused by the introduction of deleterious substances into that fluid." And as to more doubtful cases, "what is the conclusion consistent with true logic and sound philosophy? Certainly this; that here, as in the preceding cases, it appears that the primary cause of the disease should be referred to the blood."(1)

Our latest and ablest writers continue to assume the same ground of induction. Thus, we are told by Professor Ferguson, as a fundamental principle, that "the phenomena of puerperal fever originate in a vitiation of the fluids;" and that "the vari-

(1) Patholog. Anat. vol. i. p. 406, 407, &c. We would here ask whether the glands of Peyer are altered in the artificial typhus? Even M. Louis, who is wholly "for facts," and allows "the point in doubt to be of extreme importance," argues, mainly, that "it is very probable that an alteration of the blood is anterior to the commencement of acute diseases, in many cases, since every day we see local affections occur in animals in consequence of the injection of certain substances, particularly poisons, into the veins; and since many viruses, those of variola and vaccina, for example, are probably transmitted by means of the blood." Our author, however, concedes, that "these probabilities are not proofs."—*Researches on Typhoid Fever*, vol. ii. p. 264.

ous forms of puerperal fever depend on *this one cause*." The experiments by Gaspard and Cruveilhier are then introduced to justify the conclusion. Our author also adopts the humoral philosophy of M. Andral, and speaks of "the very able, but somewhat exclusive doctrines of Stevens."(1) But we shall have shown that Andral and the first humoralists of the age are not less exclusive, as, indeed, may be surmised of Professor F. from the foregoing extract.

The humoralists then predicate of the foregoing facts and assumptions, that when such agents as corrosive sublimate, arsenic, colchicum, tartarized antimony, gamboge, scammony, &c. are taken into the stomach, they "are, probably, *perfectly inert until they mix with, and give new properties to the blood.*"(2) This is the general doctrine. But, in respect to calomel, we are told, that "in an hour after given to animals, it was found that every part of the stomach was inflamed which this *insoluble* substance had *touched;*" and in other places, Dr. Stevens speaks of "fretting the stomach with mercurial agents."(3) Why, therefore, may not "*corrosive* sublimate" do the same thing, and the more so on account of its solubility; and by parity of reason all other medicinal substances which are introduced into the stomach? Is it not evident, that the doctrine as to the absorption of deleterious substances and their action upon the blood, is founded, in the present instance, entirely upon their solubility?

Magendie, Gendrin, Dupuy, Gaspard, Leuret and Hamont, Ségalas, and others, have operated largely in the experimental branch of humoralism.(4) It is said, that by injecting putrid

(1) Essays on the most important Diseases of Women, pp. 53, 58, 65, 110. 1839. "The danger is," says Professor F. "not from a rational humoral pathology, but from a retrogression to the old, irrational one." We have, however, abundantly shown, by the admission of distinguished humoralists, that the present doctrine is only a revival of "the old, irrational" one; and this, we think, is clearly shown in the able work of our author. If there be "a rational humoral pathology," why are not its features exhibited?

(2) Stevens on the blood, pp. 153, 159. See, also, Andral, Müller, &c. ut cit.

(3) Ibid. pp. 281, 297.

(4) Gaspard carried his experiments so far as to inject quicksilver into the carotid arteries. (a) Cruveilhier, Key, Lombard, and others, have lately presented us with examples of pulmonary tubercles, produced by injecting quicksilver into the bronchiæ! An experiment very nearly like this was made by Moulin in 1690. (b) He injected the quicksilver into the jugular vein, and found a large quantity of it in the lungs. He then observed appearances that looked like tubercles; but on examination "they

(a) Mem. Physiol. in Magendie's Journ. t. 1, p. 165. (b) Philos. Trans. vol. 17, p. 496.

matter into the veins of animals, they have produced a morbid state of the blood, and established a disease very analogous to typhus fever, or other fevers having a close affinity to typhus. Hence has followed the induction, that marsh miasmata, putrid exhalations, &c., induce diseases in a corresponding manner. But, it appears to us that in the whole range of medical philosophy, there have been no experiments or inductions more liable to objection. There is an utter failure, in the first place, to show that the blood itself was rendered morbific by these agents; and, secondly, that the diseases were not purely the result of the action of the foreign matter, mechanically blended with the blood, upon the vital properties of the solids; whilst a local affection has been confounded with an idiopathic fever. It is kept out of view that the irritating matter of putrid injections develope the worst varieties of venous congestions, venous inflammation, or some other malignant form of inflammation, which always put on some of the symptoms of typhus, or "adynamic" fevers.

These experiments, therefore, are worse than useless. They have gone far in establishing the belief, that miasmata operate in the same way as it has been assumed of the injections; and they have been employed to stifle the important inquiry, by the only rational process, whether putrid animal effluvia will produce disease by its external application. Magendie, it is true, affirms that he has produced typhus fever in a dog, by confining him over the exhalations of putrid animal matter; but we must not forget his declaration, that "he can call yellow fever, &c. into being, at his pleasure." (1) It is well known, that thousands

were found to be only blisters, or a separation of the common integuments of the lungs, produced by mercurial globules." "I have made," he says, "some of them get out at the holes made in the vesiculæ or blisters above described."

Since the foregoing was written, we see that Mr. Blake has laid before the British Association, 1838, the results of experiments with a great variety of the articles of the materia medica which he had injected into the veins of animals. Many of them, of course, "turned the blood black," and this may have been by forming definite combinations with the blood. But, *cui bono?* They are only repetitions of what we have shown to have been done in the darker ages of humoralism, whilst they have brought no new light to the subject.

(1) We see that Magendie has lately made another important discovery. In speaking of Chlorosis, he says, "I have lately proved, gentlemen, that *bruit de soufflet, bruit de diable*, &c. depend on the nature of the blood." He admits, however, that "if we turn to the physiology and pathology of this fluid, we find that *the very foundation is yet to be laid.*" (*Lectures on the Blood, p. 51 Tr.*) What, then, must be said

of dogs have their noses in putrid carcasses perpetually, but are, notwithstanding, always in health. The parallel, also, attempted by Dr. Evans(1) betwixt the supposed effects of animal effluvia and marsh miasmata, because putrid injections produce some of the symptoms of typhus, we shall have shown to be untenable; whilst it would in no respect prove the admission of exhalations into the circulation. We cannot go over the ground so thoroughly explored by Bancroft,(2) and more recently by Parent-Duchatelet(3) for the purpose of showing that exhalations from animal putrefaction are not a predisposing cause of disease.

In respect to the foregoing analogies, precisely the same results will follow the injection of any irritant that is capable of exciting venous inflammation, or a highly congestive state of the veins of any important organ. They develope some of the phenomena which are attendant on typhus, and miasmatic fevers; but shall we so far sacrifice physiological and pathological science, as to conclude that there is no difference in the affections, or that urine, putrid animal matter, miasmata, &c. possess the same properties in their relation to vital actions, or that the latter operate by corrupting the blood, because the former excite disease when injected into the circulation? Although we do not advocate strong specific distinctions amongst malarious fevers, we yet believe there is ample proof of their modifications; and whilst urine, &c. when injected into the circulation, will produce some derangements that are allied to the local developments of the former, there is proof of their still greater diversity. In a constitutional sense, (for when urine, &c. are injected, like the causes of miasmatic fevers, they probably derange, more or less, all the vital actions, from the former being universally applied to the solids,) the distinction is far greater, and its appreciation more important. Here it is, indeed, that the great distinction resides; for whilst the local developments which follow miasmatic fevers, and the injections of acrid substances, fall under the common genus of inflammation, the pathology in which fevers essentially consist is manifestly wholly unlike that which arises in any respect from the latter

of the thousand inductions which he and others have drawn from a surmised pathology of that fluid?

(1) Clinical Treatise on the Endemic Fevers of the West Indies.

(2) On Yellow Fever, &c. To which we may add the valuable testimony of Prof. Dunglison. See American Med. Intelligencer, vol. 1, p. 161.

(3) Hygiene Publique, 1836.

causes. The modes of action form two great genera in the history of disease. So far as the artificial affections put on the phenomena of typhus, it is mainly owing, as we shall endeavour to show in our Essay on Venous Congestion, to sympathetic influences exerted upon the whole system by the venous affection. It is universally conceded that simple phlebitis simulates typhus fever.

We may also say, that if certain agents coagulate the blood, or produce other chemical changes, or any sensible alterations of that fluid, when injected into the circulation, they prove nothing beyond the fact. They supply not the least analogy for any induction as to the operation of even the same agents from without; since their absorption is not proved, or it is not shown that the quantity absorbed is sufficient to produce corresponding results, or that such results are produced; or, they operate with an instantaneousness which precludes the hypothesis of absorption. This may be equally affirmed, as we shall endeavour to show still farther, of all the experiments which have been performed by Fontana, Christison, Brodie, Trousseau, Dupuy, Mayer, Emmert, Gaspard, Viborg, and other philosophical inquirers, in administering poisons by the stomach, or by inoculation; but which have been employed as a basis for the humoral pathology, for no other apparent reason than that many of the substances when injected into the veins produce corresponding results, or because it cannot be imagined that they should produce their effects upon the solids without first deranging the composition of the blood. This doctrine has also, in part, grown out of the substitution of physical and chemical agencies for the organic forces.

When prussic acid and the nux vomica destroy life with their greatest rapidity, the animal is already dead, as we shall show, before absorption can have commenced. And yet, because some indications of its presence in the blood have been discovered in cases of its more gradual operation, the fact has been appropriated to the humoral pathology. But even in these cases, the quantity absorbed is so small, and so largely diluted by the blood, that it is highly probable that it does not contribute to the morbid phenomena after it has passed the absorbing vessels. But since, as we have shown, there is no ground for believing that it exerts any deleterious effect upon the blood, and more especially as it should affect the highly sensitive properties of the solids

before it can deteriorate the blood, it should be recollected that it belongs not to the humoral philosophy, if it have, in this way, any agency in the production of disease. The same affirmation is equally true of all other substances, under parallel circumstances. Going on with our analysis, which is of universal application, and still regarding the more gradual operation of prussic acid, we find it developing itself upon the circulatory system. And it is not a little remarkable that the veins, as in the "adynamic" and "putrid" fevers, which are supposed to depend on a diseased state of the blood, are especially the seat of its manifestations. This is particularly seen in the turgescence of the cerebral veins. But if the phenomena be not owing to a primary deterioration of the blood when the poison operates with its most rapid effect, can it be consistently said that the order of pathological changes is reversed in the other instance? In both cases, the blood may be altered in its character, and most so when death is most sudden; since, in this case, the blood may refuse to coagulate, but not in the other. The reason is made obvious by a reference to analogies; as in sudden death from apoplexy, blows on the stomach, &c., the philosophy of which we have already endeavoured to explain upon the principles of solidism.

It will be seen that what we have now said of prussic acid, &c. is exactly parallel with the explanation which we attempted of the modus operandi of miasmatic poisons in their dilute and concentrated states, as well as with the suddenly fatal or more gradual effects of the poison of the viper, &c. There is, probably, as inferred from multiplied observations, as well as from what is known of the physiology of the blood and of the solids, an universal law in regard to the primary action of morbific causes upon the living body. If taken into the circulation, it is still the same as when acting from without.

Again, Sir E. Home argues, though not in the humoral sense, that, because "an infusion of ipecacuanha thrown into a vein excites vomiting, and opium produces drowsiness, and colchicum sickness, and perhaps cures the gout," "all these remedies, when taken into the stomach, are inert till they are received into the blood, and distributed to the parts upon which they produce sensible effects."(1) Independently of the entire absence of proof in this case, as we shall endeavour to show still farther, we might

(1) British Journ. of Science and the Arts, vol. i. p. 291.

inquire how it happens that ipecacuanha, for instance, is always "distributed" to the stomach, and that there are no marks of its action upon the brain? The plain philosophy appears to be this. Every organ has its peculiar modification of irritability, and nothing proves it more clearly than the effects of emetics and cathartics when injected into the circulation. They then act, in certain quantities, apparently alone upon the stomach and intestines, although they are as fully distributed to other parts. Now, no possible reason can be assigned why they should not produce a corresponding impression upon the same organs by an external operation; whilst proof enough may be produced of the operation of many agents before they can have made any advance towards the circulation. It is also important to consider, that the *beginning* of the operation of foreign agents is not to be estimated by the *ultimate* phenomena, as is often the practice. A large class of poisons, and medicinal substances begin to develope their phenomena as soon as they come in contact with the stomach, and even the mouth. This is an important test, and settles, in itself, the question as to the dependence of effects upon absorption. However mild the earliest symptoms, they are the beginning of an impression produced by superficial action, which goes on increasing either without any farther agency of the exciting cause, or is still increased in intensity by its continual action, till those more strongly pronounced phenomena awaken our senses with a degree of vividness which leads to the conclusion that they are but the beginning of the series. This gives a convenient time for the absorption of the agent.

Before leaving the assumption, however, that tartar emetic, corrosive sublimate, opium, &c. are absorbed into the circulation, and alter the character of the blood so that this fluid is made the exciting cause, we may say that the speedy termination of the emetic, and cathartic effects, &c., is conclusive against it. Were the blood, in these, as in all other cases, the primary morbific agent, the unnatural action should go on without intermission, till the death of the patient.

Considering, therefore, the various facts which we have stated hitherto, "what, then," in the language of a distinguished humoralist, (1) a little varied, "is the conclusion consistent with true logic and sound philosophy? Certainly this, that in the prece-

(1) M. Andral's Patholog. Anat. vol. i. p. 406.

ding cases, as well as in others resembling them, it appears that the primary cause of the disease should be referred to the" solids, —not "to the blood."

Some of the agents which have excited much inquiry, and whose supposed effects upon the blood have been liberally appropriated by the humoral pathologists, are now given up by the most enlightened defenders of the doctrine. In looking for truth, as we have seen of the chemical physiologists, and all other non-vitalists, they are actually laying a broader basis for sound philosophy in medicine, than can be achieved by the exposure of error when it proceeds from the reformer himself.

We have hitherto taken strong examples, since we have little to apprehend from minor points, so only it be shown that the humoralist convinces himself of his mistake in his favourite position, or may be convinced by others. We shall venture to pursue this plan; always attacking the main fortress, — when we shall ultimately feel ourselves quite entitled to the logical induction which we have just quoted from M. Andral.

Of pus it is said by well informed humoralists, that "pure healthy pus does not exert a poisonous action upon the blood is now pretty generally acknowledged."(1) Another, M. Donnè, has fully demonstrated, that when pus is combined with blood, it exists in a state of mechanical mixture.(2) M. Gendrin has sufficiently shown that it does not excite disease even in the highly susceptible venous tissue.(3) M. Nasse has also contributed to put this question at rest by injecting several drachms of ill-conditioned pus into the jugulars of dogs, who sustained no injury in consequence. Several physiologists have endeavoured to show that pus is scarcely anything more than a proximate principle of the blood; and, admitting this to be true, it supplies another proof that "healthy pus" is not productive of disease, in virtue of any morbific property. Or, if pus be a new formation, as we believe, and if we concede all the assumptions of the humoralist as to its morbific effects, the whole subject is strictly corroborative of solidism; since its generation is effected by the solids, and its action is upon the solids alone. We shall not, therefore, consider the experiments of Leuret and Hamont,(4)

(1) British and Foreign Review, No. 4, p. 485.
(2) Archives Gén. de Méd. Août. 1836.
(3) Hist. Anatom. des Inflam. t. i. p. 21 — 25, &c.
(4) Expér. sur le Trait. des Affections Putrides, in Jour. des Progrès, &c.

and, least of all, the microscopical researches of Mr. *Gulliver*, (1) which, if admitted, would show a mere mechanical mixture of pus with the blood. As pus departs from its simple character, whether in virtue of modified actions, or from chemical changes, it may become more or less morbific to healthy parts; and there is no difficulty in supposing that when its globules are twice the size of those of the blood, by injecting them into the circulation they may produce mechanical irritation of the capillary vessels.

We had prepared some remarks on the controverted subject of secondary abscesses; but we have no room for their insertion. We may say, however, that there is no reason to doubt that a particular modification of circumscribed inflammation may, in itself, establish a general tendency, especially in the same tissue, to a corresponding development of action. We see this constantly illustrated in the mucous, serous, and cellular membranes, and in venous congestions which often spring up in rapid succession in different organs.

Coming, at last, to suppurative inflammation, the vitalist maintains that the purulent matter of smallpox, when inserted under the skin, propagates by sympathy the same species of suppurative inflammation throughout that organ. When it springs from the action of the effluvia upon the mucous surfaces, a general predisposition in the skin is more or less established by the direct effect of the primary cause. On the other hand, why is the vesicle of cowpox confined to the place of contact, whilst it impresses the system as universally as the smallpox? These questions can be only answered by calling to our aid the principle of sympathy, through which these diversified results are produced according to the peculiar influences of the predisposing causes.

Again, we are reminded by the foregoing extract from M. Andral, that he has since abandoned the ground, that "putrid emanations of an animal nature" are a cause of disease. (2) This has been a very important point in the humoral pathology, and many an experiment of injecting putrid animal matter into the veins has revolved about it.

By way of farther illustrating the humoral pathology, our author says, "when bile has once passed into the blood, is it not

(1) See Mr. G.'s Paper before the Royal Society, in Philos. Magazine, Sept. 1838. If there be no orifices to the vessels, how do the large globules of pus get admission?

(2) See his Letter in Parent-Duchatelet's Hygiene Publique, t. i. 1836.

reasonable to admit that that fluid, being altered by its unusual mixture with the elements of the bile, can no longer exercise its regular influence over the different organs to which it is distributed?" (1) Certainly, granting the entire postulate to be true. But, as we have seen of all other premises from which humoral inductions are made, the foregoing is not true in any of its aspects. This we shall endeavour to show, on an authority which humoralists will not question, — that of chemical reagents and the crucible. Nothing but the inert, colouring matter of bile is taken into the circulation. Moreover, there is no evidence that the "blood is altered by its unusual mixture with the elements of the bile;" nor is there even of such as are artificially introduced into the circulation.

But an important circumstance has been overlooked by the humoralists, viz. that the presence of the bile in the blood, or rather its colouring matter, whether it depend upon absorption, or on the failure of the liver to perform its secernent function, is a proof that disease had existed independently of the supposed cause, and that the phenomenon which is brought in favour of the humoral pathology is a sequel of that disease. How much, also, of the subsequent changes may not be owing to the sympathetic influences of that morbid state of the liver, and how much to the more general operation of the original predisposing causes?

Nor will we here lose sight of our method of exhibiting the probable grounds of the humoral doctrine, by again comparing the opinions of an individual as expressed on different occasions. We now speak of M. Andral's report upon Larroque's treatment of typhus by laxatives, wherein he endeavours to show that bile, however vitiated, is not offensive to the alimentary canal. But this is not the most important respect in which he invalidates his humoral doctrine in relation to bile. He enters directly on the very matter upon which we have been at issue, and strongly implies, at least, that vitiated bile can do no harm if it should even be absorbed into the circulation. "The old experiments," he says, "made by Morgagni on birds, which died soon after a quantity of bile collected from the interior of the intestine of an individual who had died of a violent fever, had been injected into their blood, are insufficient." (2) Here the

(1) Op. Cit. p. 414. (2) Report, &c. Tr. 1837. Compare this, also, with the quotation at p. 516.

difference lies in preconceived opinions,—the humoral pathology being a favourite object in one case, and opposition to cathartics in the other.

Again, "every body knows," says M. Andral, "what serious symptoms appear in animals when their ureters are tied, and in man when any cause suspends the secretion of urine, or prevents its excretion. The blood becomes changed, and we observe that assemblage of symptoms comprised under the generic term of adynamic, putrid, or ataxic, fever." But, that the urine, if present at all, exists in the blood in a simple state of mixture is evident from what follows in immediate connection with the foregoing quotation, viz. "in such cases a urinous smell is often exhaled from every part of the body, and the fluid itself is not unfrequently found in different parts of the system."

Here is a succession of affirmations, which, as they rest upon no proof, it is important to notice. They constitute a part of the great basis of the humoral pathology. In the first place we shall show hereafter, that, even at this late day, there is no well grounded fact that justifies the belief that any other than the watery parts of the urine are absorbed into the circulation, or, at most, only in minute quantities, unless, perhaps, where the bladder is irritated by its contents.(1) Secondly, that it has never been shown, in any way whatever, that the blood is altered in the manner alleged; and, thirdly, we need not say, therefore, that the "urinous smell" has been owing to a morbid secretion of the skin. The whole *rationale* of the phenomena comes under the philosophy of solidism. When the ureters are tied, a great natural function of the body is arrested, and no wonder that the whole system is thrown into commotion; and as to a spontaneous decrease of urine in the human subject, the very fact, like that of the bile, denotes the prëexistence of disease, which may be mild or violent. We shall resume this subject again, when we come to the specific proof, that the deleterious parts of the bile, urine, &c. are, at most, but very sparingly absorbed. But we may say now that it is stated by Magendie, and others, that when fifteen grains of bile are forced into the crural vein, it generally kills the animal "in a few minutes;"(2) and few will doubt that vitiated bile will irritate the alimentary canal.

(1) See Section 11, as to vital alterations of the absorbents.

(2) Summary of Physiology, p. 293, Tr.

In a work of this nature, no adverse testimony may be suppressed, and its force must be admitted or reasonably invalidated. We shall therefore state, what is probably sufficiently known, that many substances foreign to the blood are said to have been taken into the circulation; that Grognier found muriate of ammonia in the serum of horses which had been poisoned by it; that Tiedemann and Gmelin discovered verdigris, sulphate of iron, muriate of barytes, and lead, in the urine, and in venous blood; that Lebkuchner found camphor in the vena cava; that Mayer, Lawrence and Coates, Tiedemann, Emmert, Magendie, Jacobson, detected hydrocyanite of potass in the blood and secretions; that Jourdan and others discovered mercury in the urine; and Piorry, the sulphate of quinine in the same fluid; that Cantu found iodine in the blood, sweat, &c., and Ségalas, and Vernière, the nux vomica in the same fluids; that Emmert, Lawrence, and Coates, found prussiate of potass and nitrate of silver in the chyle, and Krimer, and others, the hydrocyanic acid in the blood of animals poisoned with it; that Wepfer, Zeller, Hamilton, Laborde, &c. have found globules of mercury in the milk, bile, bones, &c.; that Slehberger detected indigo, galic acid, &c. in the urine, and Herring, the ferro-prussiate of potass in every part of the body; and Christison, other poisons in the blood; that the effluvia of garlic, onion, mint, &c., when these substances are handled, or rubbed upon the body, will, in a short time, be exhaled from the lungs, from the surface of the body, and along with urine, where also the colouring matter of various substances presents itself, or in other parts; that nitrate of silver discolours the skin, &c.

Now, all the foregoing statements are directly in favour of solidism, could they be generally admitted with safety to an important science. They disprove, in the most conclusive manner, the humoral hypothesis that the blood undergoes an alteration in consequence of the admission of such substances into the circulation, and that the blood thus becomes the exciting cause of disease. That such an injury should be inflicted upon the blood, those substances should have united chemically with it, and, of course, have undergone decomposition. But, on the contrary, they are said to have been found not only in the secretions, and solid parts, but in the chyle, and the blood itself, in their original condition. If not, therefore, too largely diluted by the blood, they may excite disease—and this is solidism. The humoralist could

not supply a stronger fact against his hypothesis. Moreover, we will again say, as the fact is highly important, that it has been well ascertained that when a great variety of substances have been directly introduced into the circulation, they have been eliminated within a few minutes. Many of these agents, as we have said, are highly susceptible of decomposition, or of chemical combinations with the blood. We must therefore infer that the living blood is endowed, like the solids, with a principle of resistance to the chemical influences of many agents which would affect its constitution out of the body. How inconclusive, then, all those experiments in relation to this subject which are conducted in "porringers." And, if such mineral agents pass the ordeal of the circulation, and are eliminated without change, how untenable the assumption that animal and vegetable poisons, healthy and morbid, are capable of altering the whole nature of the blood?

But, as we shall see, there is much reason to believe, from direct observation, that there have been great mistakes as to the alleged absorption of deleterious substances;(1) and perhaps this should be inferred, *a priori*, from the conflicting nature of almost all chemical analyses of the blood, solids, and secretions, and from the fact that organic chemistry, in its application to physiology and medicine, is a fictitious pursuit, without the pale of science, as well, also, from the influence of preconceived opinions. We may be wrong in our most extended meaning; but the conviction having arisen from no little research, we are bound to express it. Nor shall we have failed to bring much substantial proof to its support, and not as the least of which we are dis-

(1) Some of the foregoing, and many other agents, which have been injected into the circulation, or are said to have been absorbed, and afterwards found in their original state, must have undergone decomposition. This is another indication that frequent mistakes are committed. Acetate of lead, nitrate of silver, &c. must be always converted into insoluble compounds. The former, therefore, would produce great changes in the blood, and disastrous results, if taken into the circulation, since it is well known to have been administered in doses of 10, 15, and 20 grains. But, in such quantities we often realize the most benign effects. Mitscherlich, (*a*) and Grissolle, (*b*) state their belief that it has not been known to enter the circulation. Still the fact is said to exist; that when animals were killed by this poison, the blood was found more or less altered in its physical character; an alteration, as we shall endeavour to show, manifestly the consequence of the morbid action of the solids. (See Section 11.)

(*a*) Essai sur la Colique de Plomb.

(*b*) Muller's Archives, 1836, p. 4, 5, and Essai sur la Colique de Plomb. 1835, p. 71, &c.

posed to regard the admissions of profound chemists themselves. We are not, however, actuated by a desire to interfere with the laboratory, which we hold in great reverence, any farther than to put forth our feeble opposition to the interpolation of a spurious philosophy into the sciences of physiology and medicine. These we have a lawful right to protect so far as we may by the force of truth. Success, however, requires the cöoperation of many; and we have, therefore, subjoined a note with a reference to this view.(1)

(1) Many years have now gone by since our distinguished Caldwell, like others before him who had taken for their guide the same philosophy, uttered the confident belief, "that the chemist first forms the articles in the *dead blood*, and finds them there afterwards" (*a*) Dr. Mitchill, in one of his best papers, descants upon the subject in the same manner; stating the analysis by retorts as the "first" of "the prevailing errors whereby medical and chemical knowledge have been remarkably perverted and retarded." Humoralism takes rank as "the *second* error, connected with the first, and, indeed, in some measure growing out of it." (*b*)

Bichat was of the same opinion. "Such a chemistry," he says, "is the dead anatomy of the fluids, not a physiological chemistry. The physiology of the fluids should be composed of the innumerable variations which they experience according to the different states of their respective organs." "The instability of the vital powers is the quicksand on which have sunk the calculations of all the physicians of the last hundred years. The habitual variations of the living fluids, dependent on this instability, one would think should be no less an obstacle to the analyses of the chemical physicians of the present age." (*c*)

"Every subsequent attempt," says Dr. Bostock, "to discover the ultimate elements of organized substances differs more or less from those that have preceded it." (*d*)

Now let us see what another philosopher says of the progress of animal chemistry up to the present time, and whose writings have fallen into our hands since our several essays were nearly completed. Take we the substances which, of all others, are most likely to exist in the blood, and to remain without change. "With regard to the salts," says our author, "we doubt whether any with an ammoniacal base can be truly said to be a constituent of healthy blood. Is it not much more probable that this alkali should have resulted either from spontaneous decomposition before analysis, or have been *actually produced in the process* required to determine its presence? Again, how can it be said, that sub-carbonates of lime and magnesia exist free in the blood? If we admit that the serum owes its alkalinity to the presence of carbonate of soda, we cannot extend this admission to the insoluble carbonates of lime and magnesia; and we think it a point far from being satisfactorily settled, as to how far the *different salts* of the blood have an independent existence in it. Where, as in general, the process by incineration is followed for their detection, the fact that sulphur, phosphorus, and carbon, exist in the organic principles of this liquid, must tend to render it probable that the acids of these salts may be, if not entirely, at least in part, accidentally produced." (*e*) Again, "many chemists have employed themselves

(*a*) Phila. Journ. of Med. and Phys. Sciences, 1822, p. 222.

(*b*) New-York Med. Repository, vol. v. p. 113. 1801.

(*c*) Bichat's Researches on Life and Death, c. 7.

(*d*) Bostock's Physiology, vol i. c. 6, s. 7, p. 566.

(*e*) If this be true, and probably it cannot be shown to be otherwise, what becomes of the whole fabric of the saline theory, and the "seminal principle of disease"?

Regarding then, in their proper light, all the circumstances which we have now stated, and in other places upon the same

in endeavouring to establish the identity of fibrin and *coagulated* albumen; but with singular inconsistency they have, at the same time, in other parts of animal chemistry, divided what might be fairly, and is generally, regarded as one substance, into many others." And yet again, "the author next describes the differences between arterial and venous blood, and he arranges in order the observations of different chemists, which are in some instances of the most conflicting character." And what is important to us, arterial blood and "the blood of the capillaries was not found to differ sensibly from that of the veins;" yet no one will doubt an important difference in a physiological sense.

The author reviewed, at last, speaks himself, after the following manner. "At present, we are not acquainted with all the immediate principles of the blood in health. For instance, we *know nothing* of those *numerous* substances commonly confounded under the name of extractive matter; we cannot determine exactly the proportions even of those principles with which we are best acquainted; and, lastly, *there is not a single principle in the blood which may not undergo important changes, without these becoming appreciable* in analyses."

As to another author, (M. Denis,) — animal chemistry and the humoral pathology take from our reviewer the following castigation: "M. Denis furnishes us with thirteen conclusions, in as many paragraphs, (derived from analyses of the blood,) which, for crude hypothesis and hasty generalization, we have seldom seen surpassed. It is strange that one generation will not profit by the errors of another. We laugh at the false reasoning of the older chemists, but we do not hesitate to plunge into the very system which misled them." "It appears that among other cases, he has endeavoured to cure the croup on *chemical principles*. 'The croup,' he says, '*consists* principally in a false fibrinous membrane lodged in the trachea.' M. Denis conceived that it might be alleviated directly by the introduction of substances exerting a solvent action over it, and indirectly by introducing into the blood, through the medium of absorption, other substances possessing a similar power. He asserts that he has successfully employed for this purpose salts resembling those of the blood."

Nor can we forbear another extract from this luminous writer, as tending to show, also, the uselessness for all practical purposes, at least, of animal chemistry. It is well known that Berzelius denied all but an appreciable trace of iron in the blood, and Brande, altogether; and, although its presence is now placed beyond all dispute, there were others, who, simultaneously with the foregoing negative results, like "Menghini, supposed that at some future day, nails, swords, and all kinds of instruments might be manufactured out of it. One of our own countrymen, Dr. Good, asserted that there was enough iron in the blood of forty men to make a good ploughshare; (Book of Nature, vol. i. p. 361;) and Dejene and Parmentier broached the idea that medals might be struck to the memory of celebrated men from the iron obtained from their blood!" (*a*)

Finally, Magendie concedes a very important fact, which is always overlooked in chemical analyses, viz. that even "during the short transit from the vascular tubes to your receiver, the component elements of the blood are found to affect a new arrangement." (*b*) How, then, can we look for analogies with the blood in its natural state

(*a*) British and Foreign Med. Rev Oct. 1838, p. 334 — 448.

Here, also, may be found a complete refutation of experiments that have been made to show the identity of fibrine and albumen; a proof, indeed, that the experiments could not have been performed, or that the imputed results could not have taken place.

(*b*) Lectures on the Blood, p. 52.

question, what dependence can be placed upon reports from the laboratory, when animal chemistry is the matter at issue; especially when it concerns the modifications of the blood and secretions in disease?

SECTION X.

We have dwelt long, perhaps tediously, upon the particulars which we have hitherto investigated; but it is certainly incumbent on the solidists to answer fully the most essential points of the humoralists, and this can only be done by taking them in detail. It may be discouraging to the reader to know that there are others yet before us: but the work must not be imperfectly done. Horace, who advocated brevity, allows that the most trifling circumstances should not be neglected, when they tend to multiply the points of view of any object." Besides, as Bolingbroke says, "there are some readers for whom a writer should think over all parts of his subject." If, however, we do not examine all the facts and affirmations, we hope to lay down rules of analysis which will embrace the whole, and show how the doctrine of solidism is alone adequate to explain the phenomena that have been adduced in proof of humoralism.

We now take up the facts which are advanced in behalf of humoralism in that most deservedly popular work, the Cyclopædia of Practical Medicine, when treating of fever.

"M. Gendrin," it is said, "injected one ounce of blood, drawn from the veins of a patient labouring under putrid fever, into the cellular tissue of the groin of a cat." Very violent symptoms followed, and puss ultimately died; having been manifestly killed by the experimenter. (1)

Taking the foregoing experiment in its most liberal construction, what does it prove? Does it prove, what was intended, that the fever was produced in the man by a primary lesion of the blood? Certainly not; no more than it proves that the man had taken arsenic. It simply exemplifies the doctrine of solid-

after it has passed the ordeal of the laboratory? Magendie, indeed, endeavours to prove that the composition of the blood is not the same in any individual at any two successive hours.

(1) Dr. Tweedie in Cyclopædia of Prac. Med. Art. Fever, p. 195.

ism, which is, that the blood and all the secretions may be rendered morbid, and morbific, to a healthy subject, by diseased vascular action. The mode, also, in which the diseased blood acted upon the cat was similar to that of all other irritants upon organized parts. It is, however, astonishingly rare that the blood of patients, even "in putrid fevers," will excite disease by transfusion; and there is no little reason, therefore, to think that in the foregoing case, and in the few others like it, the blood underwent some chemical decomposition after it was drawn from the patient, (1) since these changes supervene with great rapidity in instances of that nature. Indeed, it is a doctrine of humoralism that the blood becomes putrid in the midst of the living organism.

Experiments of the foregoing nature were made at an early day. Thus, Mr. Coxe transfused "fourteen or sixteen ounces of blood from an old mongrel cur, all overrun with the *mainge*, into a healthy spaniel of the same bigness." "The effect of the experiment was no alteration at all, any way to be observed in the healthy dog." (2) Professor Deidier relates that "a dog ate greedily of the corrupted glands and blood of those who died of the plague at the hospital at Marseilles; and this he did for about three months, and was always well, gay, brisk, full of play, and familiar with all comers." (3) True, when he injected the bile of those subjects into the veins of dogs, it brought on disease; but healthy bile will do this.

We regret that we have not room for many important facts which appear to have been forgotten. Some of them may be found in the authors, as indicated below. (4) Here may be seen a great variety of the most conclusive experiments. The black vomit of yellow fever was drank in large quantities by the ex-

(1) In the paragraph immediately preceding that which is now the subject of comment, Dr. Tweedie brings the following fact to the support of humoralism. We quote it not only to justify our conclusion in the text, but to show in every possible manner the nature of the evidence upon which the humoral pathology is founded.

Thus, "Sir John Pringle mentions a remarkable fact, which came under his personal observation, of a person being seized with dysentery by making experiments upon human blood, which had become *putrid by standing some months in a close phial.*" Parallels of this case are fully considered in our text.

(2) Philos. Trans. Lond. vol. ii. p. 451. (3) Ibid. vol. xxxii. pp. 20, 105.

(4) New-York Med. Repository, vol. viii. p. 70. Ibid. vol. ix. p. 429. Other volumes have important facts. Cathral's Memoir on Analysis of Black-Vomit. Phila. 1800. Valli, Sulla Peste di Constantinopli, &c. 1805. Rust's Magazine, 30, 6, p. 189. Asalini on the Plague, and Mitchill's Introduction. Wilson's British Expedition to Egypt, p. 303. Bowring's Observations on the Oriental Plague, &c. 1838.

perimenters, and inserted into the circulation with impunity. And so of the blood. (1) Here, too, are experiments and observations in relation to the plague no less unequivocal, and decisive of its non-contagiousness. Its communicability was never surmised by the ancients; and we may add, that Procopius affirms that the plague which prevailed for fifty years, during his own time, over the earth, was not supposed to be communicable. (See p. 451 — 453, *Note.*)

The experiments, to which we have alluded, must stand; and however others, like Gendrin, Namias, Magendie, Andral, may have obtained some opposite results, they come, as we shall have shown, within the pale of solidism. If morbid blood be transfused, or inserted in the cellular tissue, upon the principle of solidism it should sometimes produce disease, like any other irritant, whilst, as we shall endeavour to show, (Sec. 14,) it is perfectly innoxious in the subject from which it derived its specific changes. (2)

Next follows, in the Cyclopædia, the curious case from Duhamel, which is a standing reference in the humoral pathology — a

(1) M. Louis says that in the Gibraltar affection of 1828, there were "no especial modifications of the blood detected." On the Yellow Fever, &c. p. 164.

(2) After what has been now said, we need scarcely enter into the merits of the controversy about the communicability of glanders to the human subject. It may be so; though we think it liable to doubt. But it may be worth while to show the nature of the evidence upon which humoralism rests its claim to this disease. Thus; "Hazard says with Solleysel, Le Guirinié, and Saurier, that glanders is 'contagious.' Magendie and Dupuy, say 'no.' Le Fosse says 'yes' and 'no.' Coleman, De Blaine, and Dantz, neither say yes nor no. Chabert, after having during his whole life maintained the contagiousness of glanders, has retracted this opinion in his old age." (*a*)

Mr. Travers, who believes in the "admission of poisons into the system," declares that "there is no evidence of glanderous matter acting on the human body otherwise than as the poison of dead animal bodies." (*b*) On the other hand, Graves, (*c*) Rayer, (*d*) Elliotson, (*e*) Johnstone, (*f*) Brera, Massey, Alexander, Ferrelli, have collected some facts which appear to show that the specific form of glanders has been communicated to the human subject. Dr. Graves, however, remarks that the disease is "of very frequent occurrence in Ireland;" though in the same lecture it is said to be "of comparatively rare occurrence." Some later details appear in the French Journals; but we do not see that they vary the question.

In whatever way, however, this question may be decided, it will be seen, from what we have said in our text, that the whole philosophy belongs to solidism.

(*a*) London Lancet, Aug. 1837. (*b*) On Constitutional Irritation, vol. i. pp. 184, 296, 237.
(*c*) Clinical Lectures, lect. 8, on Glanders. (*d*) In Mém. de l'Acad. de Méd. t. 6.
(*e*) In Med. Chir. Trans. 1830.
(*f*) In Trans. Provincial and Surg. Association, vol. v. p. 225 — 238. 1837.
See British & Foreign Med. Rev. No. 11, p. 116; also, Med. Chir. Rev. April, 1839, p. 613.

rara avis, and plucked accordingly. The story runs thus: "Duhamel has related the case of a butcher who suffered from a most malignant disease, and which proved fatal in four days, from *putting into his mouth* the knife he had employed in slaughtering an ox that had been over-driven. An innkeeper wounded himself with a bone of the same ox in the palm of the hand; the arm mortified, and he died in seven days. In two women, who had received some *drops of the blood* of the same animal, the one on her hand, the other on her cheek, the parts to which the blood was applied were seized with gangrenous inflammation."

This is a strong case, and a strong one, too, for the solidists; for we suppose it will not be denied that the "drops of blood" which fell upon the skin, and produced "gangrenous inflammation in the parts to which the blood was applied," wrought that effect by a direct irritation of "the parts." Why should we then wander from this obvious fact, in explaining the operation of the same poison when applied to the mouth or beneath the skin; albeit, though the latter died, whilst the former barely escaped dying? And is it not manifest that the blood of the ox is directly opposed to the humoral hypothesis; since the "over-driving," not being of a *poisonous* nature, could have only affected the blood through the medium of the solids?(1)

Humoralism is elated with this case. We have not less than seven late and standard authors lying before us, at this moment, who either approve or put it forth as a "clincher." Besides the Cyclopædia of Prac. Med. there is Dr. Babington, in his Essay on the Morbid Conditions of the Blood;(2) Andral's Patholog. Anat.;(3) the Western Journ. of Med. and Phys. Sciences;(4) London Med. Gazette;(5) Dr. Duncan, in Edin. Med. Chir. Trans.;(6) Med. Chir. Review.(7) The case was also a great favourite with the older writers; and we doubt not that its history would form as curious a volume as that of "The Guinea."

(1) Morand has exactly a parallel case, from over-driving; only the poison was less fatal. The beef was eaten with impunity; (*a*) and Dr. Rush states that "a farmer in New-Hampshire, who had over-heated a fat ox by excessive labour in time of harvest, perceiving him to be indisposed, instantly killed him, and sent his flesh to a neighbouring market. Of twenty-four persons who ate of the flesh, fifteen died. The fatal disease, produced by this aliment, fell with its *chief force* upon the *stomach* and *bowels*." (*b*)

(2) In Cyclopædia of Anatomy and Physiology. (3) Vol. i. p. 400.
(4) Vol. ix. p. 74. (5) Vol. x. p. 501. (6) Vol. i. p. 567. (7) Vol. xv. pp. 342, 343.

(*a*) Hist. de l'Acad. des Sciences, 1766, p. 97. (*b*) Medical Inquiries, &c. vol. iv. p. 170.

We pass to the Cyclopædia's next proof, which is "the well known fact that smallpox, measles, &c., may be produced by inoculation." As we shall advert to this subject again, we shall only say now, that after Mr. *Gulliver's* discoveries with the microscope, humoralists will not contend, should the blood be capable of transferring the diseases, it is so in virtue of the specific virus which has been absorbed from the skin. But admitting that it is not so, is there anything to show that the blood may not be specifically affected by its circulation in the vessels of the diseased integument; and is there not sufficient analogy to justify either of our conclusions? There is a fact, however, which puts the humoral hypothesis entirely at rest, and it appears to us that it may be universally applied in the way of analogy. This is, the blood of an individual exposed to smallpox and measles, will not communicate the diseases to another, till the eruptions have taken place; when the blood, if specifically affected, probably becomes so from a minute absorption of the cutaneous virus; holding it in a state of simple intermixture. We quote some important facts in a note below. (1)

We may add that smallpox has been communicated to the

(1) It is said by Professor Caldwell, "fearlessly and specifically, then, do I declare, that in the blood, *carefully and properly drawn*, of persons labouring under smallpox, syphilis, measles, or yellow fever, no one can detect variolous, syphilitic, rubeolous, or pestilential matter; nor can he, by introducing that fluid into the vessels of persons in health, give rise to either of the diseases specified. I challenge the proudest of the humoralists fairly to make the experiment, and convict me of error, if I am wrong; but if not, frankly to confess his own error, and bow to the truth. I speak with confidence on this subject, having myself performed *innumerable experiments* with a view to its illustration." (*a*)

Here is an important fact, which establishes completely the independence of small and of cowpox of any affection of the blood. These diseases "will proceed together in the same person, without the smallest interruption of each other's course. If inserted nearly at the same time, in the same person, each proceeds in the same course as if they were in two distinct subjects. If inserted nearly in the same spot, the two form one common areola, but the vesications are distinct, and each preserves its own character, till that of smallpox becomes purulent. You may then take smallpox matter from the pustule, which, by the adhesive inflammation, will remain distinct from, though seated in part of the vaccine vesicle; and from the other parts of the vesicle you may take vaccine matter, and each will perpetuate its respective morbid poison." (*b*)

No cavil can be founded upon the admitted affinity between these diseases, since each is distinguished by remarkable characteristics that are in no respect modified by their cotemporaneous existence.

(*a*) Phila. Jour. of Med. and Phys. Sciences, No. 6, 1822, p. 321.
(*b*) Adams on Morbid Poisons, p. 398.

child in utero, as observed by Jenner, (1) Bland, (2) Forbes, (3) Roberts, (4) Burserius, (5) Haygarth, (6) Turnbull, (7) Lynn, (8) Pearson, (9) Hosack, (10) Laird, (11) Mead, (12) and others. This is a trophy in humoralism. But, from what we have hitherto stated, it is evident that we must look for a rationale among the principles of solidism. Mead, always a faithful observer, says that "when the child comes into the world, before matter is made in the pustules of the mother, it has no visible marks of the distemper." This, we believe, has been universally true.

It is manifest that the disease may be communicated to the fœtus in two ways. 1st. By physiological influences of the mother through the umbilical cord. The whole fœtal apparatus is as much a part of the mother in utero, as the ovum; though we have no belief that the imagination of the mother can establish a nævus, or incise a lip, or amputate an arm of her offspring, any more than it can do the like with her own person. 2nd. By absorption of the virus, (a most inappreciable quantity being sufficient,) into the maternal blood after the eruptions have taken place.(13) Or, it would be in conformity with solidism that the blood is specifically affected by the specific action of the solids. It may then propagate the disease, like the virus itself; but what has such a fact to do with the imputed corruption of the blood, in fevers, &c., by foreign causes?

Next, "in chlorotic females," says our text-book, "the blood is altered in its physical characters, and is apparently the cause of this singular disease." The nature of the predisposing causes, and the earliest symptoms must settle the question between the blood and the solids. Dr. Hall, who advocates its humoral pathology, states, that "at the first period of chlorosis there is *invariably* a confined state of the bowels, a deranged condition of the stomach, a tainted breath, a white and pasty tongue, morbid appetite, alteration of the catamenia," &c. "The exciting causes are delicate and sedentary habits; its predisposing causes, those

(1) Med. and Chirurg. Trans. Lon. vol. i. (2) Lon. Med. Jour. vol. ii.
(3) Edin. Med. and Surg. Jour. vol. iii. (4) Lon. Med. Jour. vol. v.
(5) Institutes of Med. vol. iii. (6) On Smallpox.
(7) Memoirs Lon. Med. Soc. vol. iv. (8) Duncan's Med. Commentaries, vol. xix.
(9) Ibid. (10) Ibid. (11) Edin. Med. and Surg. Jour. vol. iii.
(12) Med. Work, p. 336.
(13) But, the vital influences of the mother upon the fœtus appear to be very circumscribed; since many are born of parents labouring under the worst forms of "putrid fever" without imbibing the disease. This is more adverse, also, to humoral inductions than the experiments by transfusion, &c.

peculiarities of the constitution involved in the lymphatic temperament." "The influence of such habits," he adds, "in such a temperament, is first seen in a confined state of the bowels, a loaded colon, and the functions of the other digestive organs, with the processes of assimilation and sanguification, become deranged." Again, "chlorosis is frequently produced, and frequently aggravated, or protracted, by some *mental* cause."(1) To this refutation by the humoralists of their own doctrines, we would add an admission by M. Andral, as stated at p. 406.

It is next related that "leeches sometimes die immediately after they have sucked the blood of some persons." We have witnessed this phenomenon ourselves in leeches that have been applied to purely local inflammations; but they were nearly perishing, before the application, from previous surfeits. Acrid secretions of the skin may, undoubtedly, have the same effect. Or, in other cases, and probably in most, the leeches have been destroyed by some cutaneous remedy, which has not been duly taken into the account. We will show this in a note below.(2)

Finally, we have a statement more exceptionable than either of the preceding. It also plays a conspicuous part in Dr. Stevens' Essay on the Blood,(3) and other humoral works. It is related on the authority of Dr. Macall, Dr. Haines, and Dr. Coleman, that there grows a plant at Goose-creek near the Cumberland river, and in other parts of the Western United States of America, called by the natives *Indian Hachy;* which is so poisonous, that a *small quantity* of the milk of cows that have fed upon it, *mixed with tea*, produces most violent fever, and in some cases even

(1) Dr. Hall in Cyclopædia of Prac. Med. Art. *Chlorosis*, p. 377.

(2) We are informed by Dr. E. G. Ludlow, that forty-four leeches had died under his observation, within the few last days, in fifteen minutes after their application to a patient, (the Russian Chargé,) affected with rheumatic inflammation of the knee. The part had been rubbed with a mixture of veratrin and laurel oil; but was thoroughly washed with soap-suds before the leeches were applied. Eighteen were first put on; and on the following day, after again washing the part in the same manner, eighteen others were applied; and in two days after, eighteen more; all of which perished in about fifteen minutes after they dropped from the limb. No other application had been made to the part after the first leeching.

It is from a neglect of facts like the foregoing, that conclusions are often drawn in direct opposition to fundamental principles. Thus, M. Melier endeavours to prove the absorption of narcotics by a case similar to the foregoing. Enemata of a decoction of poppies had been employed in the case; but "irritating applications were also made to the skin." The leeches fell off dead. (a)

(3) P. 248.

(a) Archives Gén. &c. Oct. 1827.

death, although it produce no injurious effect upon the animal eating it. Sucking calves, however, which have had no food but the affected milk of the cow, show the peculiar symptoms, and often die of the disease; and they who eat of the diseased calf, or of healthy pigs that have fattened upon the milk, are said, also, to be liable to the disease.

Here, then, it is *prima facie* evident, that a great oversight has been made; namely, the immunity which is enjoyed by the cow that eats the poison, whilst the calf is destroyed by her milk. But before we go farther, we feel entitled to ask, seeing that this case is set forth in a prominent manner in a learned and popular work, why Dr. Tweedie did not accompany it by its antidote which was embraced in the American Journal from which he quotes, and which consists in the following remarks by the American editor? "The third number of the Western Quarterly Reporter," says the quoted journal, "contains a paper on the same subject by Dr. Lea, a most intelligent physician of Tennessee, who, we think, *refutes this hypothesis*, or at least renders it more probable, that the disease is owing to a *pestiferous miasm*, or *malaria*. That exhalations of this nature are exceedingly varied by local and other circumstances is now well known, and much evidence is adduced by him to show that such modifications had occurred in the case before us." (1)

The admissions of the three gentlemen, who refer the disease to the plant, show the great improbability of its imputed origin, and that it is owing to malarious causes which involve alike man and animals. The most extravagant representations, however, are made; "the true cause," says Cornelio of a similar affair, "not being known, such a one is assigned which is grounded on some public prejudice." (2)

We have heard nothing more of the marvellous in relation to this disease unless in the way of ridicule; of which Dr. Crookshank, who undertook an investigation of the facts, is abundantly liberal. (3) But, admitting all that is desired by the humoralists as to the eventful occurrences at "*Goose*-creek," the facts, as we have shown, can only be explained by the doctrine of solidism.

(1) Phila. Jour. of the Med. and Phys. Sciences, No. 8, 1822, p. 317.
See New-York Medical Repository, vol. i. for accounts of epidemics that prevailed in the New England States among several species of animals.
(2) Philos. Trans. vol. vii. p. 4066. (3) Ohio Medical Repository, 1826.

It is well known that putrescent meat, sausages, cheese, mussels, &c., will sometimes produce violent effects upon the human subject. (1) But the details of the various cases involve the elements of their own clear interpretation. In all the instances the earliest manifestation of symptoms is in the stomach and bowels, and throughout the complaint, the disease rages there. Again, the skin is the next organ which is most frequently deranged; erysipelas, and especially urticaria, being common results. We shall not go into the proof of the special sympathy which exists betwixt the skin and intestinal canal; but considering it admitted, we obtain from this relation of phenomena another fact for the solidist; whilst the exemption of the lungs, the brain, &c., are facts against the humoral pathology. Again, the immediate relief from violent symptoms, in many instances, by spontaneous vomiting, or by the action of an emetic, is a demonstrative evidence of our right, especially when connected with the fact that the cutaneous affection sometimes disappears during the operation of an emetic.

SECTION XI.

The great final object of all the putrid and other injections into the veins of animals, and the attempts to identify the operation of miasmata upon the blood with the making of bread, the fermentation of vegetable matter, and "the contamination of a barrel of Dutch herring by a putrid carcass," (see p. 419,) is the establishment of a preconceived belief that the vast range of miasmatic fevers, and other wide spreading scourges of the human race, are truly owing to a primary "fermentation," or some other not less pernicious vitiation of the pabulum vitæ. They are, indeed, the great foundation of the modern humoral pathology; and are intended to give validity to an exploded doctrine, which is admitted to have had its origin in speculation alone. We have quoted many humoralists to this effect; (2) and in connec-

(1) See Rayer on Diseases of the Skin, art. urticaria; Plumbe on the same affection; Orfila's Toxicol. t. 2; Christison on Poisons, p. 461—484; Burrows on Fish Poison.

(2) Very recently we find the following statement by an able Professor. "Gaspard's and Cruveilhier's experiments prove," he says, "that many substances will

tion with what we have stated at pp. 390, 391, and in other places, as to the practical application of the humoral doctrine, we may quote the following from a distinguished authority. "The pathology of this disease (Purpura hæmorrhagica) is involved in Cimmerian darkness." "Until we know something of the pathology of the disease, whether in the solids or the fluids, there is little chance of discovering a successful method of treatment." (1) But, suppose the fluids; what should be the next step in adopting the remedies? (See Section 14.) If we consider miasmata in reference to quantity, it may be almost inappreciable; since, as we have shown, the most malignant fevers may be established, and even produced at the instant by scarcely

produce the same fatal effects, and by the same disorganizing process; that mercury, thick unctuous substances, acrid fluids, gritty powders, and *bits of stick*, when placed within the vessels, all produce the same essential train of symptoms; while saliva, milk, *urine*, bile, cause little disturbance." "*We are certain* the cause of death is to be sought in the action of these substances, *not on the injured vein, but on the blood.*" Our author then proceeds, agreeably to the method of M. Andral, to argue mainly from these experiments that all cases of puerperal fever depend on a "vitiated state of the blood." True, he admits that "it may be said that no certain conclusion ought to be drawn (from the foregoing experiment,) unless it could be proved that putrid substances absorbed by the uterine vessels produce puerperal fever." But, "such an experiment has unfortunately too often been made; and Gordon, Campbell, and Kirkland, have most distinctly acknowledged, that retained and putrid placenta will produce genuine puerperal fever." (Dr. Ferguson, *Ut cit.* p. 67.)

Still no proof is offered, in any of the cases, of the least probability, that the action of the morbific causes is "not on the veins, but on the blood." "Bits of stick" acting on the blood! The cases of Gordon, &c., prove nothing to the purpose, or rather they belong to the solidist; since, if putrid matter were the exciting cause, its action was *on the vessels* of the uterus, or, what is equally probable, the "putrid placenta" itself was the source of irritation. The authorities quoted, though excellent observers, are no better upon this question than our author himself. The fact as it respects the placenta *per se*, no one can doubt. The imputed alteration of the blood as the exciting cause of disease is wholly conjectural, and involves the most momentous practical results. Our author's hypothesis as to the absorption of matter was laid down by Kirkland, though more in the sense of solidism; but without any show of proof. Our author's whole conclusion, therefore, rests on mere assumptions. There is also a remarkable one (p. 18,) founded on Le Galois' failure to detect pus when mixed with the blood. It may, therefore, be absorbed from the placenta. What does analogy prompt in the instance of "bits of stick"?

When we come to the treatment of puerperal fever, Dr. Ferguson wholly loses sight of his hypothesis; and regardless of the philosophy of which we have just spoken in our text as to purpura, he prescribes like a vitalist, but as without hope! The deplorable doctrine is held forth, that nothing can counteract the progressive operation of the poison, since we are ignorant of any means by which it may be neutralized. This, as will be seen in our 14th section, is exactly what we contend for in every disease of humoral origin. (See p. 516.)

(1) Medico Chir. Rev. Lon. July, 1839, pp. 305, 306.

more than a momentary exposure to the primary cause. The disease is even developed before there can be time for its intermixture with the blood; and by the soundest philosophy, therefore, — that philosophy by which the humoralists would carry up the assumed modus operandi of the venous injections, — we may infer that all miasmatic fevers are produced in a coincident manner. Miasmata, also, are too subtle for our senses, too delicate for the tests of the chemists, and are not known to increase the weight of a given bulk of air. Dr. Holland, who appears to adopt the entire humoral doctrine, considers "the virus of disease" so "minute" as to render it doubtful whether it "be ever the object of sight." (1) Mr. Boyle illustrates, variously, the extreme tenuity of miasmatic poisons, by comparing them with the odour which enables dogs to scent the game at great distances, and at long intervals of time, to the magnetic fluid, &c. (2)

It is but reasonable, therefore, to render the experiments by injections of the least validity, at the outset, that there should be some remote alliance, as to quantity, betwixt miasmatic poisons and the matter injected. A considerable amount of the latter, whatever its nature, is always necessary to establish violent diseases. This is owing to its dilution by the blood, to its existing in a free state, and to its not infecting the blood according to the humoral interpretation. Nothing can be inferred from the prick in dissections, excepting what bears directly on solidism. The numerous experiments with injections of putrid animal matter render it in the highest degree probable, that a far greater quantity of the virus which destroys in dissection wounds would be necessary to produce disease if thrown into the circulation; and, doubtless, this may be safely affirmed of the wourali, or of any other poison. Even some of the poisons, which act with great and

(1) Medical Notes and Reflections, p. 371, 1839.

(2) Boyle's Philosoph. Works, vol. i. p. 414.

"The stagnant air of the marshes of the fort of Fuentes, at the outlet of the Valteline, a place so febriferous that it is impossible to sojourn a single night without being attacked, Gattoni found in 1779, by eudiometrical experiments, to be two degrees better than that of the peak of Legnone, one of the Grison Alps, which is ever covered with snow; and the result was the same, though repeated fifteen times, and with marsh air collected from eleven different places." (a)

M. Julia, who performed, in the course of 1819, sixty analyses of the air of marshes, drains, necessaries, stables, hospitals, and cemeteries, arrived at the conclusion that the air of the several situations contains the same principles, and in the same proportions, as the purest air of the most healthy situations."

(a) Craigie's Practice of Physic, p. 73.

instantaneous power, when applied to the tongue or stomach in a concentrated state, there is reason to believe must be thrown into the circulation in greater quantities to produce the same results, however they may act with equal, or greater intensity, when introduced largely into the circulation. Much may be often owing to the relation which the specific properties of the virus bear to the peculiarities which appertain to the vital properties of different parts. Thus, a drop of prussic acid may destroy life immediately, when applied to the mucous surface of the alimentary canal, though it may produce no such effect if applied to a wound, or even to the brain. But six or eight drops injected into a vein, although diluted by ten or forty pounds of blood, may extinguish life as soon as it begins to circulate in the organized structure of the alimentary canal, or in any part to which it bears the same relations. Through this relation of the properties of medicines to the vital powers of particular parts we explain their primary effects, and their secondary through the sympathies of parts, or a more general agency of the nervous system. There are poisons like the wourali, the virus of the rattlesnake, putrid animal matter, &c., whose specific properties have no morbific relation to the vital forces of the mucous surface of the alimentary canal; but their relations subsist with those forces as animating the extreme blood-vessels or the intimate organization of nerves, especially the capillary nerves. When applied, therefore, in a concentrated state to the abraded vessels and nerves, they act with far greater intensity in an extremely minute quantity, than when a greater quantity is injected into the circulation. (See p. 522.)

Again, however little may be the reason, in our opinion, to question the origin of many epidemics in exhalations from decaying vegetable matter, there are many sound minds that oppose the doctrine; and even a large proportion of those who refer common epidemic fevers to vegetable miasmata, are either entirely undecided as to the causes of malignant cholera, and epidemic influenza, or deny altogether their dependence upon malarious agents. Bilious and intermitting fevers are imputed by many to cold, damp, or other atmospheric changes, which can alone exert a primary effect on the *vires vitæ* of the solids. The followers of Broussais and Balfour consider solar heat, by its action on the surface of the body, and the propagation of the excitement to the stomach and intestines, as the principal or sole

cause of tropical fevers; and we find Dr. Stevens more or less concurring in this opinion; whilst M. Andral attributes tropical fever to the liquifying effect of heat upon the blood. Philosophers of no less consideration than Mead, (1) Moseley, (2) Johnson, (3) J. Hunter, (4) Ramazzini, (5) Darwin, (6) Baglivi, (7) Arnold, (8) &c. believe that the very moon may act as a predisposing cause of tropical fever. Is it not a hotly contested question whether yellow fever and the plague depend upon a virus generated by living or by dead matter?

And then we have "the hypothesis of *insect life* as a cause of disease," to which Dr. Holland has devoted a serious inquiry,(9)

(1) Works;—on the Influence of the Sun and Moon, &c. p. 163—206.

(2) Tropical Fevers;—on the Influence of the Moon, p. 568—648.

(3) Influence of Tropical Climates, &c. vol. i. p. 112.

(4) Lectures on the Principles of Surgery, lect. 11.

(5) Constitut. Epidem. § 53. (6) Zoonomia, vol. i. s. xxxii. 6.

(7) De Fib. Motri. Spe. tri. c. 3. &c. So, also, Galen, the Arabian physicians, Lind, Jackson, Musgrave, Gregory, Van Helmont, Sir Hans Sloan, Hildebrand, Pitcairn, Balfour, &c. It has been supposed that Hippocrates regarded the heavenly bodies as predisposing causes of disease; but he expressly says it is the weather which is influenced by them, and that this, not the heavenly orbs, affects the constitution of men. — *L. de Aer. &c. s.* 1. *ver.* 36. (8) British and Foreign Med. Rev. Oct. 1839. p. 481.

(9) Dr. Holland is disposed to defend the insect origin of the malignant cholera. The four most important evidences are the following. 1st. "The erratic and ambiguous course of cholera is well represented by the *flight*, *settlement*, and *propagation* of the insect swarms which inflict *blight upon vegetable life*." 2nd. "Another *analogy*, as it may be considered, to the *habits* of insect life, is the apparent preference of *direction* along the course of waters;" but, here the motion is less "erratic." 3d. "The well attested observation that the cholera sometimes spreads *in face* of the winds"; this being peculiarly adapted to the forward march of the insects. 4th. "In the *hypothesis* of production by certain animal species, *minute beyond the reach of all sense*, we have an explanation of the fresh creation and diffusion of the material cause;" and, especially, as "out of the direct *dominion of the microscope* these animals are removed." But, since this last proof, particularly, may be thought to render the animal doctrine in some degree hypothetical, we would suggest another probable analogy, namely, that the cholera, like the supposed vermin, was choice in its subjects; taking those only in whom it was notorious that "visible" vermin already abounded. Were it not for this fact, we might do injustice to the "hypothetical" cholera vermin, since they are supposed by Dr. Holland to possess a musical taste, and of being "capable of exciting vibrations in the air, of which man's grosser hearing is wholly *unconscious;* but which, received by their *fine organs* as audible sounds, minister to purposes of enjoyment and activity among beings *unperceived by any of the human senses*." It is stated by our author, however, that "Rameaur and other naturalists have *conjectured*" that the clouds are sometimes made up of these insects. (a)

The insect hypothesis originated during the "dark ages." It was scouted by Hodges in his Λοιμολογια seu Pestis, &c., 1762, when an attempt was made to refer the plague of London in 1665 to animalculæ. Bradley came out furiously with it in

(a) Medical Notes and Reflections, pp. 569, 574, 576, 577.—1839.

and which now enters largely into the *rationale* of many diseases. (See APPENDIX on the *Microscope*, vol. 2.)

The foregoing difficulties led Sydenham to predict that we should never understand the causes of epidemic disease.(1) The philosopher Moore remarks, that "the difficulty of discerning the real sources of diseases, and all their sources, appears greater in proportion to the pains which we have bestowed in investigating the subject."(2) Does this not arise especially from our leaving the phenomena of nature for artificial methods? Again, one school contends for the constitutional nature and causes of many fevers, whilst another maintains that they are all symptomatic of local inflammation induced by local causes.

Here, then, is another ample ground for setting aside the humoral pathology, whilst solidism remains unaffected in the midst of the doubt and strife as to the remote causes of disease. To render even probable a hypothesis that the foreign causes are first taken into the circulation, and then corrupt and ferment the blood before disease can be produced, it must be first shown that there are other causes besides fatigue, heat, cold, moisture, insects, the sun, moon, and stars. But since no others have been produced to the satisfaction of many able observers, the whole doctrine of absorption is clearly hypothetical: so that we have the following series of inductions as the foundation of humoralism, in relation, at least, to the vast range of idiopathic fevers. 1st. Injections of putrid animal matter produce some of the phenomena of typhus fever, though many other agents, even wounds of the veins, will do the same. 2nd. Therefore, the blood is "vitiated," "corrupted," "fermented," "altered in its nature," by the putrid matter. 3d. The blood thus corrupted is, therefore, the true cause of the disease of the solids. 4th. From which it follows, that miasmata, whether they have an existence or not, are the special causes of fever. 5th. That it also follows that they are absorbed from without into the circulation, and there produce their effects exactly in the same way in which it had been assumed of the action of the putrid matter injected. 6th. The existence of the miasma being assumed, and, consequently

his "Pest of Marseilles considered, 1721." But, it "fell dead from the press;" and the doctrine slumbered till Dr. Crawford, (a rare example in America,) endeavoured to prove that yellow fever, and every other epidemic, is owing to "animalcular action on the human body." (b)

(1) On Epidemic Diseases, ch. 2. (2) Medical Sketches, p. 234, *Note.*

(b) See a series of papers entitled "Remarks on Quarantines," in Baltimore Observer, 1807.

the primary deterioration of the blood, remedial agents must act primarily upon that fluid. 7th. When tartar emetic is injected into the veins, it produces vomiting. Like the injected putrid matter, it alters the blood, but it alters it back again—makes it healthy. 8th. Therefore, when tartar emetic, or other remedies are given by the stomach, they are absorbed into the circulation and alter the blood exactly in the same way as had been affirmed of the same agents when injected into the circulation.

Now we refer back to our numerous quotations for the proof that we have in no respect exaggerated the foregoing humoral *rationale* of the effect of morbific and remedial agents; whilst the very existence of the great humoral cause of disease, about whose action so many preliminary steps are concerned, has not been conclusively shown, is denied by many sound philosophers, and is admitted by the humoralists to be inappreciable by any of the senses, or by any of the tests of art. A doctrine involving so many suppositious data as humoralism, appears to us irreconcilable with the true principles of philosophy, which, above all in hypothetical questions, admonish us to prefer the most simple and unobjectionable to a more complex and hypothetical process. Where the nature of causes is undetermined there can be no room for mistake, as to principles, in explaining their modus operandi. We must reason from the plainest and most simple of what is best known, and limit the construction within the narrowest bounds of the phenomena; and where, as in the treatment of disease, an opposite series of causes and effects concur in the more simple interpretation, and are even opposed to the complex hypothesis, no ambiguities can embarrass our conclusions.(1)

We may here stop for a moment, for the purpose of saying, that since this, and our other essays were written, several able observers have given indications of a returning era of Hippocratic

(1) We wish not to be misapprehended upon the foregoing subject. As far as our own opinion is of any moment, we are thoroughly satisfied that miasmata, of vegetable origin, are generally the predisposing causes of idiopathic fevers. It should be considered that something more than swamps and marshes, or vegetable matter, are necessary to the generation of the specific virus. A peculiar and continued temperature, and a certain constitution of the air, are as indispensable as the matter upon which they act. But, the disbelief of many renders the question undecided; it is still *sub judice*. It is therefore evident, that the existence of such miasmata cannot be taken as the ground of a hypothesis which affirms many positive facts. Solidism, on the other hand, avoids all speculation, and, above all, the induction of absolute, specific results from premises which are controverted. It does not assume that miasmata are the causes of fever. It admits it may be something else;

and Baconian philosophy. Experiments which have hitherto swayed the physiological world are denounced as incompatible with nature, (see pp. 511, 514, &c.,) the vital powers recognised as something entirely distinct from the physical, and even a *vital principle* admitted, where they had been but recently scouted as a visionary assumption;(1) and, finally, as we have also seen, (p. 529, *Note*,) a philosophical doubt has been thrown over animal chemistry by one who writes in the very glare of its syren fascinations, and with the prospective penalty which awaits a disturber of the public peace. But, to the specific object of this memorandum. A critical writer, in speaking of the experiments of Magendie and Philip, remarks, that "the one comes to the conclusion that the nerves are nothing more than electric conductors, and that the stomach digests food by a galvanic power: the other, that the stomach is of no use in vomiting, since, when it was cut away, and a pig's bladder substituted, there was no difference in the effects! It would be a curious point of inquiry, as to how many animals these two physiologists have destroyed, to acquire opinions which are now only retained by themselves."(2) And now we ask of what better consideration are all the experiments with injections, and all the inductions founded upon them, which we have been considering? It is due, however, to Dr. Philip to say, that he is yet far from "alone in his glory." Three-fourths of the existing physiologists either adopt his conclusions, or contend for a primary agency of galvanism in the processes of organic life. See our Essay and Appendix on the Vital Powers, and on Digestion.

We resume our inquiry. Reasoning from final causes, which we hold to be justifiable in a limited sense, there would have been an improvidence in All-Seeing-Nature to have left an avenue for poisonous exhalations or other morbific agents into the blood-vessels; and the more so, were those agents forever liable to "ferment" and alter the constitution of the blood,—rendering it the efficient cause of disease in the solids, and thereby divesting it of the possibility of being again restored to its normal state by the action of the instruments from which its existence is, in part, de-

that it may be the heat of the sun: that it may be the light of the moon; as supposed by some solidists. But it takes, and justly so, all analogy for its guide, in explaining the operation of the unknown cause; nor has a fact been produced to invalidate that explanation.

(1) See p. 11, *Note*, and first *Note* in vol. ii.

(2) British and Foreign Med. Rev. Oct. 1839, p. 537.

rived. But we shall see that a multitude of direct facts come to the aid of this argument. It is something, in the first place, considering the morbific agents to which we are so constantly exposed, that thousands enjoy an uninterrupted exemption from disease. It is more, that where twenty individuals are exposed to the miasma of a marsh, fifteen may escape disease; for we apprehend that humoralism will not make a reservation in favour of the latter, by imputing to them a lesser liability to the absorption of the poison, or that their blood will not "ferment" as readily as the blood of the former; and it would be manifestly absurd to assume that fermented or otherwise altered blood will not lead uniformly to disease of the solids. On the other hand, however, solidism has its thousand facts, as we have hitherto explained, and cited examples, by which it interprets the development of disease in the first class, and the exemption of the second. This it does, as it respects the principles, by regarding in their true light the character of the vital properties of the solids, as made known by their diverse phenomena when subject to the direct action of foreign causes. It sees those properties variously modified by external agents, and variously susceptible in different individuals; and, more than all, it sees them modified constitutionally, and by the habits of individuals. It sees the five, who, perhaps, are smitten with yellow fever, the subjects of high living, of generous wine, irregular habits, or moral emotions; whilst the fifteen, who escape, have lived with greater frugality;—and this, perhaps, is the only apparent difference amongst them. But, that it is right in imputing the liability of the former to the morbific action of the poison, and the exemption of the latter, mainly to those causes, it feels assured by witnessing the same coincidences in a vast number of instances, at various times, and in different countries. But, again, on the other hand, physiology, reason, give us the most perfect assurance that the entire mass of blood cannot be diseased without exciting disease universally in the solids.

That we have not ascribed too much to the natural constitution of the properties of life, and their temporary modifications by the habits of mankind, is farther manifest from the comparative exception of brute animals from malarious diseases whose habits are natural and constitutions uniform. And since we know them partially liable to these affections, we find in their comparative exemption another proof against the humoral pathology;

for no reason can be assigned why they should not be equally with man exposed to the absorption of deleterious agents, whilst grazing the meadow, or devouring the putrid carcass, and to an equal "fermentation," "corruption," or other morbid alteration of the blood.

If miasmatic, or other morbific exhalations, enter the circulation, we are ready to admit, that they probably find their ingress through the medium of the lungs. We object wholly, however, to the analogical induction which is predicated of the phenomena of respiration. It is no more applicable to the present question, than the conclusions which have been formed, but mainly defeated, as to the absorption of morbific agents by the alimentary canal, because the constituents of the blood enter through this medium. They are exactly parallel cases, and the reasoning which is applicable to one is equally so to the other. The absorption of the gaseous elements of air is as much a natural function as that of chyle, and designed for an important purpose in the animal economy. The vital properties of the absorbing vessels are specifically adapted for the admission of those gases, and the same principle excludes all others. There is ample proof, for instance, that the effluvia from putrid animal matter, which is said to be so morbific when injected into the circulation, may be at all times respired with impunity. If we present to absorbing vessels, of whatever kind, any noxious substance, they generally reject it. It may produce inflammation of the part, and this inflammation may be propagated by sympathy, as we have shown, along the vessels. They admit nothing in their natural state whose physical properties are not in harmony with themselves. The principle is universally present in the living tissues, though variously modified according to the specific office of each part. It is wonderfully present, and comes under the demonstration of the senses, in some capacious orifices. It is seen in that of the larynx, which repulses everything but gaseous substances, and even a great proportion of these. It is remarkably exemplified in the pyloric orifice of the stomach. Evidence of this nature defeats all the inductions of the chemists. (1)

(1) As to miasmata, it is true, so great a solidist as Bichat, allowed that they may be taken into the circulation; but for no other reason than because he thought it was not impossible. (a) But it is one of his most important objects to show that

(a) Récherches Phys. sur la Vie, &c. p. 438 — 455.

But do you deny that deleterious substances are sometimes taken into the circulation through the alimentary canal? Certainly not. We deny nothing that has been reasonably shown. But when such agents, with marvellous rarity, are taken into the circulation, it is clearly in virtue of some modification which they previously establish in the vital properties of the absorbing vessels. We know it from the manifestation of an altered state of the vital phenomena before it be possible for the agent to have entered the vessels; and we see it exemplified in the pyloric orifice of the stomach which gives up its protecting care when long irritated by food, or by cathartics, or under circumstances of disease. The proof, however, is ample and various, and will be more or less adduced as we advance with our inquiry. But we may now say, that if the foreign agents affect the vital properties of the absorbing vessels in the foregoing manner, so, also, do they affect the properties of the other tissues of the part; and this is the beginning of disease, which may now go on accumulating without any farther agency of the exciting cause. But supposing the entire quantity of the offending agent to pass on to the circulation; it may now be either too much diluted by the blood to be injurious, or it may continue, *per se*, to exasperate disease.

As to the inhalation of the vapour of turpentine, and its subsequent odour in the secretions, there is scarcely an analogy between the absorption of that highly volatile odour, and the ponderable gases.(1) This will be manifest, when we consider that a small phial of the "otto of rose," although nearly hermetically sealed, will send its perfume over a large space for many years without any apparent diminution of its contents.

Again, the whole of the foregoing is equally true of the odorous matter of assafœtida, camphor, garlic, and of other substances when absorbed by the alimentary canal. Magendie, whose negative affirmation we may well trust upon this subject, gave half a pound of assafœtida to a horse; but, on killing the animal sixteen hours afterwards, he admits that even the odour could

all gaseous substances, when artificially introduced into the blood-vessels, are speedily fatal; a fact which has been also variously shown by Nysten (*a*) and others, though lately controverted to a certain extent.

(1) "The peculiar odour of urine occasioned by the use of the oil of turpentine," says Mitscherlich, "is no proof of the presence of the latter, but merely points out a change in the urine."

(*a*) Récherches de Physiol. et de Chim. Pathol.

not be observed in the arterial blood, or in the lymph; (meaning the chyle.)(1) Shall we decide that the essential principles of rhubarb enter the circulation because the urine is tinged with its colouring matter? Here, again, we have the best authority upon our side. Magendie gave four ounces of an infusion of rhubarb to a dog, but could discover no trace of it in the thoracic duct. This, indeed, was true of all other active medicinal substances.(2) In this extremity, shall the veins be brought forward as agents of absorption? It is utterly undetermined whether they absorb any thing noxious or innoxious.(3)

Again, appearances are often delusive, and even ludicrous mistakes are made to sustain a preconceived hypothesis. Thus, it is said by Udall, that after giving for ten days only a few grains of iodine, a black deposit took place in the urine, and that the iodine continued to affect the urine in this manner for several months afterwards.(4) There can be no doubt that changes of this nature are due to the vital properties of the solids; whose modification, indeed, may have been owing to a deleterious effect of the remedial agent.

The foregoing case, as is constantly happening with many analogous ones, is cleared up by the progressive researches of chemists. Drantz had a patient with influenza that passed blue urine. It was supposed that the colour was owing to the presence of hydroferrocyanite of iron, although the patient had taken no such preparation.(5) Again, we may take an authority which the humoralists will not question. "Another principle," says Dr. Prout, "apparently connected with lithic acid has been found, in one instance, by Dr. Marcet, in a specimen of *black* urine, and for which, from its properties, the term *melanic* acid has been proposed. (See Med. Chir. Trans. 12, 37.) Instances of *black* urine have been frequently alluded to by authors; and, indeed, a condition of the urine which would be vulgarly considered black is not very uncommon." "Again, such is a part of the series of changes which lithic acid is capable of undergoing, and apparently does undergo, in the human body in *different diseases*, either *by the* action of the kidney, or the natu-

(1) Précis Elément, &c. p. 240. (2) Ibid. pp. 232, 235.

(3) The discovery of the termination of lymphatic vessels in different parts of the venous system, the obvious final cause of the veins, and that of the lymphatics, render it in the highest degree probable that the veins do not absorb.

(4) Diss. de Effect. Iodin. 1833. (5) Journ. de Chim. Méd. Juin, 1837.

ral operation of the various principles existing in the urine upon one another." (1)

Dr. Marcet has recorded another remarkable case of the foregoing nature. Bracqnnet, Willis, and others, mention cases of blue, green, and black urine; of which it is said by Willis "the peculiar colours could not be referred to the existence of any purpurate, or any combination of iron with the cyanic or phosphoric acid." (2) We believe it to be now generally conceded, that these colours depend upon a new proximate principle. But what shows the relation of our subject to vital principles, is the fact that *a single drop* of creosote has not only increased the flow of urine, but changed its colour to "black and other hues." (3) "Black" urine has been also voided in constitutional fevers. (4) In one of Marcet's cases it appears to have been natural to the subject.

Dr. Wright introduced into the stomach of a dog an ounce and a half of the sulphate of iron, along with a pound of bread and milk, after the animal had fasted 56 hours. The presence of the iron could not be detected in the thoracic duct by the tincture of galls, &c. (5) So, also, Dr. Fordyce. (6) Acids have been often administered with a view to correct a supposed redundancy of alkali in the blood; but Malcolmson found the urine highly alkaline after having given in beriberi daily, for some time, strong muriatic acid, in doses of one or two drachms. (7)

M. Grisolle could detect no lead in the blood or various secretions of those who laboured under the colica plombæ. (8) So, also, Mitscherlich. (9) Its soluble preparation is extensively employed, with impunity, as an endermic application; and whilst

(1) On Diseases of the Urinary Organs, &c. p. 15.

(2) On Urinary Diseases and their Treatment, p. 139.

(3) Cormick on Creosote, Ch. 2. Exp. 12.

(4) Jackson on Febrile Diseases, vol. i. p. 174.

(5) Philos. Trans. Lon. vol. xlvi. p. 295. (6) Fordyce on Digestion, p. 122.

(7) On Beriberi, pp. 138, 293, 294, &c.

We have the following example from the humoral pathology, in respect to iron. "After the discovery of iron in the blood," says the British and Foreign Med. Rev. (a) "it was supposed that the pallor peculiar to chlororis, was due to a diminution in the quantity of that metal; and this seems to have been the foundation of the use of ferruginous preparations in its treatment." "But the disease cannot be owing to this deficiency, since the same diminution is met with in other and widely different diseases."

(8) Essai sur la Colique de Plomb. p. 73, &c., 1835.

(9) Essai sur la Colique de Plomb.

(a) Oct. 1836, p. 447.

the colic has been produced by three grains, internally, it has been given in other instances, without injury, to the extent of 240 grains in 37 days, and 1620 grains, or ℥iij and ʒiij, in three months. (1) It is said, also, that lead offers another anomaly in producing its specific effects most readily when applied to the mucous coat of the lungs. Mitscherlich, although believing in the doctrine of absorption, affirms that "acrid and stimulating substances have not yet been detected in the urine by chemical examination;" (2) where, certainly, they should appear when absorbed at all.

This subject has been also tested by other direct and unequivocal experiments. Thus, Seguin rubbed over the abdomen 73 grains of scammony, and after allowing it to remain ten hours, he collected all but a quarter of a grain. Of the same quantity of gamboge he recovered, after ten hours, upwards of 71 grains. "The substances were collected and weighed with great care." (3) The most remarkable circumstance about this statement is the little waste that was sustained.

What, then, shall be said of the experiments of Dr. Madden, who procured a purgative effect upon himself by the application of infusions of rhubarb, gamboge, and of jalap to the skin? And yet these facts are received into the humoral pathology, as substantiating its principles, when it is known that many advocates of that hypothesis, even Magendie, deny entirely an absorbing function of the skin unless divested of its cuticle, and whilst, also, it is highly probable that there are not living a dozen individuals upon whom the foregoing applications would produce a similar effect. The only mode, therefore, of explaining the phenomenon is by a recourse to the principles of solidism. There are remarkable sympathies betwixt the skin and intestinal canal; and in Madden's case there may have been an idiosyncrasy of the former organ, through which the specific impressions were propagated to the intestines; or we may reasonably ascribe the result to the effect of imagination operating upon an irritable constitution, and through an ardent mind. We have already cited examples of cholera morbus which had been invariably induced, from youth to advanced age, by the approach of a thunder storm, and others of a parallel nature. (p. 422.) We know a lady in this city who is always purged by shaking in

(1) Barbiere's Mat. Med. p. 627. (2) Med Chir. Rev. Lon. vol. xxxii. p. 107.
(3) See Magendie Précis Phys.

her ears a box of pills, which is sometimes employed for this special purpose. And so *vice versa*, the operation of cathartics may be remarkably restrained by mental influence. (1) Again, Madden was nauseated by rubbing upon his hands a solution of tartarized antimony, (2) which, we believe, no one has yet pretended to have detected in the blood or secretions; nor have we ever witnessed the symptom after long continued applications of this substance to an eruptive surface. Physicians, indeed, are daily employed in making similar experiments in their ordinary practice.

Certain anomalous appearances, in morbid anatomy, are sometimes put forth as proofs of the admission of poisonous agents into the circulation; whilst the very facts are, *prima facie*, against the conclusion. Even Mr. Hunter was betrayed into this mistake in respect to lead; and the error has been appropriated as fact by Dr. Stevens and others. "On examining the body of a paralytic house-painter, the muscles, particularly those of the arms, had lost their natural colour; but, instead of being ligamentous and semi-transparent, as happens in common paralysis, they were opaque, resembling exactly, in appearance, parts steeped in a solution of Goulard's extract. From this case it appears the lead had been evidently carried along with the blood, even into the muscles themselves." (3)

(1) See Dunglison's Therapeutics, p. 35; and Thomson's Materia Medica, &c. vol. i. p. 39. (2) Physiology of Cutaneous Absorption, &c., 1838.

(3) Hunter on the Blood, &c., p. 99.

The belief is so general, that in the paralysis and colic of painters the effects are owing to an absorption of the lead, we shall here add a few words more to what we have already said upon this subject in our ninth section. In the first place the very imperfect power which the sound skin possesses of absorbing innoxious substances, if it possess any, and the insolubility of the oxyds of lead, appear to us conclusive against the opinion. Again, is it not manifest that before the lead could have produced, *per se*, the foregoing chemical alteration of the muscular substance, it must have been fatal by its profound alteration of the blood? Or, thirdly, why was the remarkable appearance confined to the paralyzed muscles; why was not the lead equally transmitted to the brain, and the disorganization of that part equally the result of it? Or, fourthly, supposing a soluble preparation, which could have only entered the circulation progressively in exceedingly minute quantities, would it not have been immediately decomposed by the blood, and therefore fail of its supposed chemical action upon the muscles? Or, fifthly, was not the continued paralysis of the muscles, and their disorganization, attended by a corresponding degree of morbid action; and this, being the consequence of a specific and unusual cause, is it remarkable that the morbid action thus specifically different from that attendant on ordinary paralysis, should have induced the unusual appearance in the morbid product? Or, sixthly, is it too great a tax upon analogy, in a science so greatly made up of analogies, — seeing that

We are well aware that the defence is now set up, that many substances cannot be detected in the blood unless they exist in large quantities.(1) But is this any proof of their having entered the circulation? Besides, what becomes of those affirmations that many of these very agents, which are now said to be inappreciable unless largely combined with the blood, were once very easily discovered in that fluid after a few grains only, or a drop of prussic acid, had been administered by the stomach? Are we not justified in the belief, that former analyses, which have imparted so much strength to the humoral pathology, have been imperfect and illusory? And seeing, also, that no ill effects can arise from the admission of morbific agents into the circulation, unless they exist in appreciable quantities, and being admitted to be generally inappreciable in the circulating mass of blood, an important branch of the humoral pathology is thus overthrown

it will be admitted that in common paralysis the alteration of the muscles to "a ligamentous and semi-transparent" state is wholly the result of a morbid condition of those parts, and entirely independent of any humoral condition of the blood, and of the physical action of any foreign morbific cause, — is it unphilosophical to suppose, that, in the other case a nearly coincident appearance, with coincident symptoms, was owing to a like pathological cause? And, seventhly, is not this one of the numerous proofs, that all our conclusions from morbid anatomy should be drawn with the strictest reference to those physiological principles which have been deduced from living actions?

(1) It should be also considered that the amount of matter, if it exist at all in any given quantity of blood or secreted fluids, is exceedingly small, and its detection embarrassed by the compound nature of the fluids, — and this the more so, by the chemical re-agents employed. The substance sought may be sometimes found, or something as nearly like it as sweet almonds to milk, or vinegar to sugar, or a product of cancers and of muscular putrefaction to the albumen of eggs, which are identified by chemists; but who shall say that they are not often produced in the test glass of the chemist, or generated by the blast of the furnace? When an approach to certainty is required of the chemist regarding the most tangible poisons, arsenic, mercury, &c., however great the quantity, he does not make up his opinion till he has exhausted all the resources of his art. Nor does he look for them in the blood, or the secreted fluids, should he fail of detecting them within the alimentary canal.

But it is otherwise in physiological science. Here it is considered safe to establish fundamental principles upon vague results of a superficial analysis of animal or vegetable matter, perhaps upon the sense of taste or smell alone; when those principles may lead to a greater destruction of the human race than all the carnage of war. Nor is this all; for it is now proposed by some humoralists that the vital phenomena shall be disregarded, and the treatment of disease shall await a tedious analysis of the blood, and be decided by those obscure changes which may happen at the onset, or at any period, of disease, or which will be more likely to result from the analysis itself; whilst it would be considered monstrous to suffer a supposed detection of a metallic poison in the blood or secretions to affect even the liberty of the greatest criminal.

by its own attempt to demonstrate its validity. And, may we not also add, that it is indeed astonishing that vessels which are designed for absorption should admit deleterious agents so sparingly, at least, as to render it a controverted subject whether they are admitted at all; and that this fact, in itself, probably shows the design of nature as to their exclusion? In our present discussion, we have an especial reference to the functions of the lungs; for here it is mainly that the non-vitalist conveys into the circulation those deleterious exhalations which lay the foundation of wide-spread epidemics, and of other diseases, which make up a vast proportion of human suffering. Nor shall we lose sight of the other imputed inlets to morbific agents; or those practical inductions which impart to our subject its inexpressible importance.

Considering, again, the immediate and indispensable importance to life of that function of the lungs which concerns the absorption of oxygen, and, perhaps, of nitrogen, it is a natural conclusion from this fact alone, that the vessels destined for the foregoing office possess an exquisite modification of their vital properties; so that, in relation to gaseous substances, at least, those peculiarly modified properties, whilst they admit the ingress of oxygen, repel with the most admirable unity of design all gases that may be deleterious to life. In this conclusion we are sustained by a few philosophical observers, who maintain, that the term absorption, in its ordinary physiological sense, cannot be applied to the process of aëration which the blood undergoes in the lungs;(1) that "carbonic acid is not formed at once in the act of respiration by the combination of the oxygen of the air with the carbon of the blood, but is entirely the product of exhalation."(2)

It is for the foregoing reason, that the chemists have been unable to detect the absorption of any other gaseous substances, simple or compound, than oxygen and nitrogen gases. We see exactly the same principle operating in the leaves of plants, and we may say universally throughout every part of the animal and vegetable kingdoms. It is especially true, also, of such parts as are concerned in the office of introducing and animalizing alimentary substances. Here the elective function is most sensibly manifested, and exemplifies in its discrimination betwixt good

(1) American Journ. of Med. and Phys. Sciences, vol. xxxvii. p. 138.
(2) Edwards on the Influence of Physical Agents on Life, p. 239.

and evil, as it were, the perfection of Design, and the Unity of that Power and Wisdom and Beneficence by which it was conceived and executed. We know this sentiment has been ridiculed; but that does not alter the fact, which none has invalidated by any other expedient.

Nor can it be said in opposition to our fundamental principle, that deleterious agents are permitted to operate directly and injuriously upon the *vires vitæ* of the solids. For in the first place, as we have already said, the evil is greatly of our own procuring, or transmitted to us from our ancestors, in virtue of their violations of nature. But we admit it to be in a measure natural and *ex necessitate rei;* since morbific causes are permitted. The vital properties are maintained in action by the blood and foreign agents; and the Almighty in His wisdom, has so constituted them for useful purposes, that they shall be susceptible of changes in their nature, as in the processes of gestation, lactation, &c.; and when great violence is inflicted, or when exposed, as they must sometimes be to agents not administering to the uses of the animal economy, those changes may be very abnormal. This is one of the necessary consequences of the final cause of the adaptation of the properties of life to the influence of salutary agents. Still, from the very nature of the properties of life, their susceptibility to the action of unavoidable morbific causes may be more or less controlled. Why morbific agents are permitted is another question. It is certainly true, also, that in consequence of *morbid alterations*, the elective power of the lacteals, the pulmonary, and other absorbing vessels, may admit the ingress of morbific agents, — their vital properties being now more or less in harmony with the specific properties of any particular poison. So far as this is true, however, it only strengthens our position. But it is, nevertheless, certain, that there exists in these particular organs, under all circumstances of disease, a most astonishing repulsive principle against the admission of deleterious agents.

Nor can we leave this subject without adverting to the grounds upon which it appears to be now generally admitted that mercury is absorbed into the system before it produces its constitutional effects; the principal of which is the very imperfect analogy which is supplied by the absorption of other substances. And we notice this species of induction, the more, in relation to this particular substance, since, having arrived in

that manner at the conclusion as to its absorption, it has been assumed as an important basis for a corresponding analogical process in respect to other substances, and even taken as a confirmation of the doubtful absorption of those agents, and of their mode of action, whose assumed absorption and humoral action had formed the premises for the same conclusions as to mercury.

Many careful analyses of the blood of patients, after taking large quantities of mercurial preparations, have been made; but, perhaps, none more so, than those which are related by the late distinguished Dr. Warren of Boston, in his work on Mercurial Practice, and by the eminent Dr. Physick, and Dr. Seybert.(1) In all the cases there was an entire failure to detect the presence of mercury in the blood and secretions. On the other hand, the facts which have been generally adduced in proof of absorption are vague(2) or extravagant.(3) Other methods have been pursued to ascertain whether mercury be taken into the system, some of which are of a direct nature. Thus, Seguin applied 73 grains of one of its salivating preparations to the skin, and after an interval sufficiently long for its constitutional effects, (ten hours,) he collected all but one grain and two-thirds, which may be reasonably allowed for loss. (See foregoing note.) We

(1) See New-York Med. Repository, vol. v. p. 288, 1801.

(2) Dr. Christison thought he had detected this metal in the crassamentum, when it could not be found in the serum. Colson imagined that he produced an amalgam by plunging pieces of brass into the blood of subjects who had taken mercury. (*a*) Why is it not then susceptible of detection by other tests?

But Colson's experiment is an old affair, and belongs this side of the water; having been made by Physick, and Seybert in 1797. They employed polished gold, and copper; immersed them in the blood and saliva; boiled them in a flask, &c., but neither was in the least tarnished. They also made various other fruitless experiments. The acute mind of Dr. Physick came to the conclusion that the action of mercury is local, and its general effects the result of constitutional sympathy. "*No one*," he says, "*now suspects that opium, tobacco, aq. lauro-cerasi, Peruvian bark, &c., enter the blood vessels;* and yet these substances act extensively and powerfully on the body." (See Med. Repos. ut supra.) American physicians, having still remained true observers of nature, continue to cultivate the foregoing philosophy.

(3) "It is said," says Sigmond, "that no less than a pound of mercury has been found in the brain, and two ounces in the scull cap of a person who had been salivated." It is not stated how many pounds were discovered in other parts, or how many more had been administered to the patient. The curious may find other proofs of this sort in Wepfer's Exercit. de loco affect. in Apoplexia, p. 278. Also, Percival's works, vol. ii. p. 311, *note*. Canter, Autenrieth, Zeller, Schubarth, Rudolphi, record similar instances.

(*a*) Revue Méd. Jan. 1838.

know an apothecary who is invariably salivated by rolling with his fingers some half a dozen blue pills; another insoluble preparation.

Without, therefore, regarding the affirmation of some of the strongest defenders of the humoral doctrine of absorption, that the skin will not imbibe even water, as Magendie for instance, it appears to us that the foregoing examples of the constitutional action of insoluble preparations of mercury, whether applied to the stomach or the skin, must settle the principles of its local operation and propagation by sympathy. The importance of the question is comparatively little in its relation to this agent; but the mode in which mercury operates illustrates very forcibly the character of the vital properties, and contributes largely in laying a basis for the philosophy of solidism. It may appear enigmatical, that the rolling of a few blue pills should result in salivation from the action of the mercury upon so small a surface of the skin, and from so slight an application; but certainly less so, if we avail ourselves of analogies, and of the light of physiology, than that the same result should follow the possible absorption of a most inappreciable quantity of the insoluble preparation; and this, too, after its dilution by the entire blood of the body.

Again, the humoral pathology affirms, that when mercury produces its constitutional effects, it is in virtue of a primary alteration of the blood; and since that alteration is produced by the inappreciable quantity which we have stated, and since, also, the whole circulating mass of blood, the *pabulum vitæ*, becomes so profoundly altered as to inflict a serious and somewhat lasting disturbance of the solids, how much greater should be that disturbance where an ounce is received into the stomach, but where none at all may take place?

Since, also, the blood is constantly renewed, and foreign substances are eliminated from it within a few minutes, certainly within a few hours, (p. 461,) it should follow, of course, that a given quantity of any morbific agent should affect the blood more profoundly when introduced at once into the circulation, than when administered in divided doses at distant intervals; and consequently disease should be established in the solids in the former case, not only with greater certainty, but to a greater extent, and more permanently. It is just otherwise, however, with mercurial preparations.

Solidism can alone interpret the phenomena. In the instance

where five or ten grains of mercury are exhibited, an impression is made upon the vital properties of the stomach, and more profoundly than is produced by the eighth of a grain. But the effect in constitutions which are now the subject of observation, is evanescent; whilst by the occasional repetition of the agent in small quantities, the impression is maintained. The vital properties, too, of the stomach becoming more and more susceptible in consequence of their gradual alteration, each successive dose is felt more and more sensibly; till, at last, the alteration, as in a thousand other cases of the operation of remedial or morbific agents, is so completely established that it maintains itself for an indefinite time. In the meanwhile, also, the sympathetic impression is propagated at large over the system. If, as in one instance, the agent be not repeated, the general impression, like the local, is evanescent, and too slight to be followed by a change of vital actions; or, in a more proximate philosophy, if we may say so, that alteration of the vital properties is not induced which constitutes the essence of disease. In the other instance, the sympathetic effect begins, also, from the commencement; but here it is maintained by the increasing alteration at the centre, in the cases under consideration, and it increases, *pari passu*, as those changes progress, or become established.

This is the key, in our opinion, to the whole philosophy of the operation of morbific or remedial agents. It may be illustrated in a similar manner, by a thousand examples, from either source. It is the fundamental principle upon which the whole treatment of disease proceeds, — upon which that disease is established in whose subversion this principle is alone concerned. Hence the magnitude of our inquiry, when placed in opposition to the doctrine of humoralism.

We might, also, bring a variety of examples of different aspect to illustrate the foregoing principle: which, indeed, we have done already. Thus, intermittent fever, whose paroxysms are often repeated at its early invasion, is far more likely to recur at some distant period, than when the disease is cut short at its commencement. What has any imaginable alteration of the blood to do with this phenomenon? Again, instead of this established liability of the vital properties to a morbid condition, and to the development of that condition by causes which had no agency in its production, and of which, *per se*, they are incapable, we have opposite examples in which their susceptibility

to the most highly morbific agents is extinguished. This is exemplified by all the strictly contagious diseases, which rarely or never occur but once. The principle is most remarkably illustrated by the protecting power of the vaccine disease; and what we have said of repeated impressions by mercury is also exemplified, under a different aspect, by the failure of vaccination to protect the system against the full force of the smallpox when its impression is only once made upon the vital properties, and by its complete extinction of their susceptibility to smallpox, when vaccination is frequently repeated. And it is also shown, at the same time, by the recurrence of the vaccine pustule whilst that susceptibility remains, (1) and by the failure to reproduce it when the susceptibility to smallpox is extinguished. And who will imagine that this solitary pustule depended upon a general contamination of the blood, whilst in the end, no amount of the virus will so "ferment," or otherwise alter the blood as to reproduce the affection of the solids?

And if the pustule in this case be owing to the action of the virus upon the vital properties of the solids, and the constitutional impression be determined through the principle of sympathy, why should not the same philosophy be applicable to smallpox, — especially, too, when we regard the relation which one disease bears to the other?

Again, the same philosophy is illustrated by other diversified influences of agents, which may be remedial or morbific according to the manner in which they are applied, and the existing condition of the vital properties. We will take one which cannot be suspected of any taint from humoralism, viz. bloodletting. We have already gone over the philosophy of its operation; but since it is prolific of instruction, in the way of analogy, we shall briefly review one point in its connection with solidism.

We have shown that, in many cases of fever, and of acute inflammations, two pounds of blood abstracted at once will subvert the morbid action; but, that when the same quantity is taken in divided portions, it may exasperate disease. We think, also, that we have fully demonstrated that the loss of blood can only produce its effect by a direct impression upon the vital properties. In these

(1) We have a child in whom we produced a perfect vaccine vesicle, and characteristic scar, at eight successive times. In less than a month after the last vaccination he had a severe attack of varioloid. He was then five years old, and had been infirm from his infancy. From that time his health improved.

instances, therefore, we see those properties altered favourably by a strong impression upon them, and unfavourably by the constant repetition of the same impression in a slighter degree. Other auxiliary results may be concerned; such as an abatement of the force of the circulation in the former instance, and its increase in the latter; but, nevertheless, the whole series of phenomena, so far as bloodletting is concerned, is owing primarily to the direct effect of the loss of blood upon the vital properties of the solids. Again, the same application of this agent, which may be remedial by either of the foregoing methods, will, when applied in either way under circumstances of health, so alter the vital properties as to constitute disease.

But we go back to our example of that great humoral agent, mercury. It is maintained that mercury produces its effects by "fermenting," or otherwise altering the circulating blood. Now let us look at the whole philosophy. We will not repeat, however, what we have just said, but simply state the fact that the constitutional action of mercury is constantly promoted by bloodletting, and the more we bleed, the more it is increased; whilst in some cases it can only be established by the loss of large quantities of blood. And as to the depurating cathartics, it was fully ascertained by Dr. Fordyce, and others, that mercury salivates with greater certainty when cathartics are given, aye, *saline* cathartics, than when they are not. Cathartics, also, shut up the absorbing vessels of the alimentary canal; and whether, therefore, these, or bloodletting, contribute to the constitutional action of mercury, the humoral interpretation involves the absurdity of supposing that the more we lessen the amount of the morbific, or remedial agent, — the more we dilute it, or, if you prefer, the more we cleanse the blood by neutral salts and lessen its volume, the more we increase the vigour of its morbific action. The solidist, however, finds an easy explanation of the apparent mystery upon his own principles. He sees that bloodletting and cathartics so modify the vital properties as to render them not only more susceptible of the action of this agent, but of all others. Again, in susceptible subjects bad forms of fever are often immediately extinguished as soon as the constitutional effect of mercury is manifested in the gums, and before there is any apparent augmentation of the secretions. But, at other times, the same disease will be greatly aggravated by this agent, notwithstanding a profuse ptyalism, and a redundancy of other secretions.

Before leaving this subject we ask the humoralist how, upon his doctrine, he will explain the prolonged constitutional effects of less than a grain of calomel, when the saliva continues to flow in torrents long after the administration of the poison, and long after an abundance of "saline" cathartics, and perhaps no little diminution of the general mass of blood by venesection? Or, why again, this same individual shall continue, perhaps for years, to be the subject of remaining influences of that grain of calomel?(1) And addressing ourselves to the purely non-vitalist,—to him who holds in derision the doctrine of those giants in mind and philosophy, Hunter and Bichat,—we ask him how he will interpret, by anything that is known of the forces which reign in the inorganic world, the phenomena which we have now considered?

But, finally, admitting the absorption of the insoluble preparations of mercury by the stomach and the skin, although in opposition to all facts, it would in no respect affect the great principle of solidism; since, it is clear enough that they do not alter the blood, and therefore produce their effects upon the solids, *per se*.

Again, as to other active medicinal substances, which are admitted by humoralists to operate precisely upon the same principle with such as are more strictly the causes of disease. The corroding effects of iodine upon the intestinal mucous membrane; its paralyzing the skin when applied in the form of ointment, and the extension of this paralysis to parts which are supplied by the same nerves of sensation,(2) its well known effects upon glandular tumours when applied over the seat of the affection, and the doubtful results of experiments to ascertain its presence in the excretions,(3) appear to show the certainty of its local action. This is also farther confirmed by the fact that when iodine is administered by the stomach, it disperses the tumour as when applied to the skin beneath which the tumour is seated, whilst it will have no such effect if applied at a distance from the tumour. We would refer the reader particularly to a late work by

(1) "When the deleterious effects of mercury," says Dr. Todd, "have been once established in the system, it is surprising how long it will continue to operate. I have frequently detected it in patients after several years, the mercurial action being called forth, from time to time, by cold, privations, or intemperance." (*a*) This is exactly the same condition, in principle, of which we have spoken in regard to the repetitions, at long intervals, of intermittent fever, &c.

(2) See Cartwright, in American Med. Recorder, vol. xv. p. 272—274. 1828.

(3) See Dr. Cogswell's Essay on Iodine, p. 62. 1837.

(*a*) Dublin Hospital Reports vol. ii. p. 186.

Mr. Davies(1) for a great variety of proof of the local action of iodine in its controlling power over many inflammations, &c.

"Professor Müller has lately called attention to the fact, that a solution of the aqueous extract of opium is possessed of a purely local action upon nerves with which it is brought into contact." (2) Have we not examples of this nature in belladonna, hyosciamus, and other narcotics when applied to the eye; in aconitin when rubbed along the nerve in sciatica, &c.; and is not the dilatation of the pupil in the former case the same when the narcotics are taken into the stomach? If it be through sympathy with the skin of the eyelids in one instance, why not with the stomach in the other? (3)

Professor Müller, being one of the least transcendental of the German physiologists, intellectual, erudite, and of unwearied industry, is justly in high repute among sound philosophers. He is also of acknowledged authority in the humoral school, and we shall therefore continue to avail ourselves of his assistance.

When an alcoholic solution of the extract of nux vomica is applied in the quantity of a drop to the tongue of a rabbit, the constitutional symptoms, as we have shown, begin on the instant. And yet with this, and a thousand other analogous facts, Müller says, that "strychnine, applied in powder to the moist spinal cord of the frog, excites no twitchings of the muscles; it must first enter the circulation, and there act on the spinal cord, which transmits the influence to the nerves." Again, "it is now proved that the symptoms produced by the poison, (narcotic and animal poisons, especially,) are due to its having entered the blood by imbibition." "It is likewise susceptible of proof that the general symptoms of poisoning are principally owing to the *action of the blood*, impregnated with the deleterious substances, on the central organs of the nervous system." (4)

These characteristic conclusions rest in part upon the experiments of Fontana and Sir. B. Brodie, which we have already appropriated to the use of the solidist, (pp. 484, 508;) but most of all do they grow out of the failure of poisons to produce their

(1) Practical Remarks on the use of Iodine, &c. 1839.

(2) British and Foreign Med. Rev. vol. v. p. 223.

(3) "When veratria is applied for tic doloreux, sometimes the paroxysm is brought to a termination during the first friction, and does not return again." Turnbull on the properties of the Ranunculus, p. 27.

(4) Elements of Physiology, vol. i. pp. 627, 629; also, 242, 246, 247.

effects when applied to the *trunks* of nerves. We have sufficiently shown the fallacy of these experiments of applying morbific agents to the trunks of nerves, and even to the surface of the brain. But we always prefer having an author refute himself when he is in error; and for this purpose we need not go beyond the page from which the foregoing affirmation is quoted. "Wedeymer," says our author, "found that prussic acid, of which the action was so strong that it proved fatal *within a second* when introduced into the eye and other parts of the body, produced no *sudden* effect when applied immediately to the nerves." Will any one believe, that this poison was absorbed by a sound surface, carried along the absorbing vessels to the heart, and thence distributed to the "nervous centre," so as to have the animal dead, — all "within a second"?

We cannot help again adverting to the constant want of distinction between the spinal cord or the trunk of a nerve and the minute ramifications, in which not only its peculiar properties especially reside, but there, also, are the great organic forces which appertain to the various series of capillary vessels of other tissues. It is here, and upon these nervous and organic properties thus situated, that all agents produce their strong impressions, — not upon the trunks of nerves in their relation to other parts. Will the eye see, or the ear hear, if you apply light or the causes of sound to their nervous trunks? Will the skin feel, in like manner; or, will you propagate any sensation to the brain by gently irritating the trunks of the cutaneous nerves? Or, in the same way will the stomach vomit, or the heart be convulsed by agents that will produce these effects when applied to the organs? Our author says, in proof of his position, "that the narcotic action of morphia, &c. is not propagated from the trunk of a nerve to its branches," because "the muscles of the leg are not paralysed." But this proves nothing but the simple fact. It does not show, in the least, that a very different and even fatal effect may not be propagated to the brain, by its direct action upon the part, when the narcotic is applied to the extremities of nerves; and this, indeed, as we have just seen, our author has proved against his own hypothesis.

But it is singular, as we have said before, that it is not seen that the experiments of applying narcotics, &c. to the brain, the spinal marrow, and the trunks of nerves, to show by their failure to produce the same manifestations as when applied to the

surface of the organic viscera, or other complex tissues in which the minute fibres of nerves are distributed, that the poison must be absorbed, is fatal to the hypothesis, since the brain and spinal marrow, and their membranes, may be supposed to be as good absorbing surfaces as many other parts. Or, will it be maintained that the poisons are decomposed by the brain and nerves, as we have seen it to have been affirmed of the wourali, the virus of the rattlesnake, &c. in respect to the stomach, to get rid of the difficulty attending their inertness upon the latter organ? (See p. 484.)

Again, our author states, that "the action of prussic acid, of angustura poison, and of the upas, in Emmert's experiments, took place, when most rapid, in from *two* to *five seconds*."(1) This is enough. Our author, however, endeavours to explain away, in another place, the rapidity of the operation, by resorting to the microscopical measurements of the capillaries, and by *chemical* experiments upon the *dead* membranes of frogs. There was some evidence of permeability, through the agency of *chemical* attraction, in the delicate tissue of a frog's lung in about a second;(2)

(1) Op. Cit. p. 628. (2) Ibid. p. 245.

On a subject of this moment, it is proper, perhaps, that experiments upon which the humoral hypothesis rests should be given in the language of the writers; that the nature of their analogy to the processes of life, and how far they are considered necessary to sustain the hypothesis of absorption, may be fully and accurately appreciated.

In regard to the foregoing experiment, Müller says, "having put into a glass vessel with a very narrow neck some solution of prussiate of potash, *I tied over it*, in one experiment, the urinary bladder of a frog, in another the lung of the same animal; then with a hair-pencil I applied to the surface of the soft membrane some solution of the muriate of iron, and at the same moment inverted the glass, so that the solution of the prussiate of potash came in contact with the inner surface of the membrane. A second of time had not elapsed when a pale blue spot formed, and soon became more distinct."

From this *chemical* experiment upon dead animal membranes, and where the organic pores have lost the control of the vital properties, an induction is carried up to the actions of living organized beings, that poisons are absorbed into the circulation; and the same experiment, and the time which the blood is supposed to occupy in circulating throughout the entire body, are made the ground of a conclusion, that when poisons destroy life in a *second* of time, it is by passing through the long tract of the absorbing vessels, and then taking the route of the general circulation till they reach the brain.

This is, in part, one of the fruitful results of denying to the living organized system the powers of life, and of regarding it as a mere chemical laboratory; and, in part, of applying the specific phenomena of chemical *endosmosis* as observed in dead matter to the actions of living organized beings.

The foregoing objections are alike applicable to Magendie's method of illustrating

but, in this time, Wedemeyer's living animal was dead;—and this, where chemistry had no connection with the process. (See APPENDIX 2.)

If, however, many poisons are absorbed into the circulation and distributed by the heart throughout the system, and produce their fatal action by their direct application to the brain, or to other organs, in from one to five seconds, what shall be said of the virus of hydrophobia, of the smallpox, &c., when applied even to abraded capillaries? Where is chemical *endosmosis* in these cases? What is the next hypothesis?

But, after all, our author admits, that by the greatest power of numbers, it has not been shown that the blood can perform its round through the system in less than from "half a minute to two minutes," which, it will be seen, does not take into consideration the time that is necessary for the absorption of extraneous matter; and the shortest time in which it has been pretended by any other, that foreign matter has been detected in the vessels of the intestines, is one minute after its application to the mucous membrane. And although our author affirms that "the foregoing experiments, as well as many others, instituted by well-known physiologists, *prove* that *before* narcotic poisons can exert their *general effect* on the nervous system, they must enter the circulation;" yet, in the next paragraph, as a chronicler of facts, our author adverts to the instantaneous death which is produced by the external application of prussic acid, and also states that "the spirituous solution of extract of nux vomica introduced in a small quantity (one drop) into the mouth of a young rabbit, produces immediate death;"—that is to say, in a second of time. Nor should we neglect saying that our author endeavours to reconcile the effects of prussic acid with the hypothesis as to absorption by another of a more purely conjectural nature, viz. "the rapid effects of prussic acid can only be explained by its possessing great volatility and power of expansion;"(1) but he neglects to account

the supposed law of endosmosis by the experiment upon the membrane of an egg. *See his Leçons sur les Phénomènes Physiques de la Vie, &c.* The subject of endosmosis has been placed in its proper light by M. Raspail, who has attacked the doctrine, and shown that the results are owing to ordinary imbibition. If there be a dense fluid, it *soaks* into the membrane, when a gaseous or other fluid on the other side will unite if they possess chemical affinities. (a)

(1) Op. Cit. pp. 246, 247.

The British and Foreign Med. Rev. (Oct. 1839, p. 314,) tolerates this opinion. We

(a) Chimie Organique, p. 80.

for the equally rapid effects of the nux vomica, of the angustura poison, and of the poison of the upas, which he announces in connection with this subject. (See Exper. by Brachet and Dupuy, p. 480.)

Many facts of the foregoing nature might be stated. Thus, Capt. Hall witnessed the death of a dog from the bite of a rattlesnake in eight seconds;(1) and Magendie states that if a phial of hydrocyanic acid be opened under the nose of a small animal, it falls instantly dead. And so Pereira, of rabbits.(2) Now, as we have said, it is not pretended even by Müller, that any substance can be absorbed and distributed through the system in a shorter time than from half a minute to two minutes, and this conclusion, as we have just seen, is founded mainly upon a crude experiment upon a dead membrane, and designed for the special benefit of the humoral pathology. (Note, p. 565.)(3)

see not, however, how the volatility of prussic acid should promote its general combination with the blood, even admitting that the whole process of organic actions is purely physical.

In a case by Orfila, where the extract of nux vomica was introduced into the thigh in a solid state, the symptoms took place in one minute and death in three and a half. Here the reviewer, reasoning upon the insolubility of the extract, shows conclusively that the results could not have arisen from an absorption of the poison.

As to M. Magendie, our able and very impartial author remarks that "the confidence with which he asserts the operation of poisons through the exclusive medium of the circulation, perfectly astounds us," &c.

(1) Philos. Trans. vol. xxxv. p. 309. A surgeon, and "five or six" others, were present at the experiments. "We could not see where the dog was bitten, nor any blood."

Another dog, "as big as a common bull-dog, was bit on the inside of the thigh, and died in half a minute exactly." "No blood was drawn." A third large dog "died in four minutes" after the bite. Dissections were made by the surgeon. In the two first of the foregoing cases, the symptoms must have begun at the instant of the bite. (See our pp. 508, 522.)

Finally, Capt. Hall made the rattlesnake bite himself: "he was quite dead in eight minutes or thereabouts."

On another occasion, Capt. H. and Dr. Cooper repeated the experiments with another snake. One dog was dead in thirty seconds. The anatomy of the rattlesnake accompanies the experiments.

The foregoing statement, which substantiates fully Dr. Johnson's experiment with the Cobro de Capella, was designed for page 486.

(2) Elements of Materia Med. Part I. p. 242.

(3) True, an able reviewer, in his analysis of Mitscherlich's Materia Medica, adapted to the physical doctrines of life, affirms that "we know that in from fifteen to thirty seconds, the blood passes from one side of the body to the other." (a) This may be very true; but it does not show what is implied, that substances may be absorbed, and distributed within that period. Sympathy is put down as a "subterfuge"

(a) British and Foreign Med. Rev. Oct. 1839, p. 355.

We do not see how there can be any hesitation as to the philosophy of the various results of the remarkable nature which we have now stated; and when the entire variety is considered in connection, we think it cannot be objected that they form the basis of a great general law, establish the most momentous principles in medical science, and which, more than all things else, concern the well-being of mankind.

Again, Müller, and other humoralists, admit that "the local influence of narcotic poisons on the nerves, is certain." Having gained thus much, one great step is taken in tracing the impression onward to the brain and other organs. And how shall we dispose of the varied experiments by Philip, and other eminent physiologists, in which they influenced the action of the heart, and capillaries, by narcotics and stimulants applied to the brain?

After having written thus far, we came in contact, in our systematic perusal of our author's valuable work, with the following statement, where it stands in a very different relation from the foregoing experiments upon the nerves of the cerebral system, and from which the induction was made that the life of the *organic* viscera can only be destroyed by narcotic poisons through the medium of the circulation.

"The influence of narcotics locally applied to the sympathetic nerve," says our author, "does not extend to the distant organs which the nerve supplies; but *these organs may be paralyzed by the direct narcotism of the minute fibrils which are distributed in them.*" And what is more remarkable, he adds, that "in this respect the sympathetic resembles the cerebro-spinal nerves, which are affected by a narcotic substance, and deprived of their excitability, only so far as the substance has actually touched them." (1) This is all we ask; and it is exactly the ob-

growing out of an "ignorance of sound physiology." We would not complain of this, had the writer disposed of the phenomena of sympathy in conformity with gravitation, chemical agencies, &c. or had he shown the "ignorance" or "subterfuge" of vitalists. We do not object to any latitude of remark where it is deserved; but its applicability should be fully shown.

(1) Op. Cit. p. 734. Whytt established by experiments the local action of opium, and that all its results upon the system are sympathetic. (a) In referring to them in another work, he says, "I shall at present only observe, that since a solution of *opium*, by affecting the extremities of the nerves to which it is applied, *and without being received into the blood, or carried to the brain or muscles*, destroys not only the power of voluntary motion in animals, but also renders their muscles incapable of being excited

(a) See his Essays, in Edin. Physical Essays, vol. ii. p. 309; and Physiological Essays.

ject of the argument we had employed without the advantage of our author's direct experiment. And, although in the experiment relating to this subject, the extract of nux vomica did not arrest the movements of the heart when applied to its inner surface under "a few seconds," the idea of its absorption is combated;(1) whilst in the case where death took place from nux vomica, and prussic acid, in a *second* of time, when applied to the tongue, the fact of its absorption is maintained, and that death could not have taken place till it had gone the whole round of the circulation; for which entire process nature accommodates the hypothesis with just *one second*, whilst we may well suppose that the fatal impression began at the *beginning* of the *second*. The mind itself is utterly incapable of a more rapid operation.

The laws of reflex action, setting aside the organic properties, supply the means of tracing about the local and instant impression of the poison which we have now seen fully admitted. But we prefer having its farther illustration done by our author, who is entirely *au fait* upon this subject. "The laws of reflection (in the cerebral system) stated in the third chapter of this section," says our author, "prevail likewise, in the actions of the sympathetic nerve. Strong impressions *on parts supplied by the sympathetic nerve may be propagated to the spinal cord, and give rise to motions of parts which derive their nerves from the cerebro-spinal system.*" Thus, "Volkman has observed *convulsions* of the body produced by irritating the intestines of *decapitated* frogs." Why not, then, with the advantage of the head on, if concentrated prussic acid, applied to an animal less tenacious of life than a frog, kill in a second, — why are we not entitled to the foregoing explanation which our author lays down as a *general law?*

Sensation is never necessary; for according to our author, "the *sensitive* impressions received by the sympathetic nerve, although conveyed to the spinal cord, may not be perceived by

into contraction by the strongest stimuli; it evidently follows, that involuntary as well as voluntary motion depends upon some power or influence of the nerves." (*b*) The facts are indisputable, however false the induction.

(1) "The rapid effect of the poison," he says, "is not explicable on the supposition that the poison penetrates from within rapidly through the walls of the heart." (p. 735.) What, then, is to be done with the *endosmotic* experiment on the dead lung of the frog? (p. 565.)

(*b*) On Nervous Disorders, p. 5.

the sensorium, the organ of consciousness," and those which "give rise to reflex motions through the spinal cord are, in most instances, not productive of sensations."

But we have still more from our author's discoveries to help along our principles; for it appears that "*impressions on parts of which the nerves are derived from the sympathetic are communicated to the spinal cord and brain, and excite the motor influence of the sympathetic nerve by reflection;*" and "reflected action of the sympathetic," continues our author, "from an impression communicated to the spinal cord by the cerebro-spinal nerves, is a more frequent occurrence." (1)

Our author abounds, here, with *examples* which show how all impressions upon different parts, from poisons, medicines, &c., may be transmitted over the system, and establish alterations of the organic forces and actions, without the least necessity of supposing their absorption.

The effects which are thus propagated from a part to all other parts will depend upon the nature of the exciting cause. If mechanical, there may be only spasmodic twitchings in some remote muscular tissue, and an augmentation of natural action in a part or the whole of the vascular system, as in Philip's experiments. There is no alteration of the vital forces of a part thus momentarily irritated; and the transmitted impression being simple in its nature, there may be no change of kind in the forces secondarily affected; although if the local impression be long continued, a change may be established in the irritated part, and in those to which the impression is transmitted; though one is not necessary to the other. Under these circumstances, the precise effects will depend upon the peculiarities of the vital forces of the irritated part, and of those which receive the impression indirectly. Again, if the agent be of a miasmatic nature, the vital properties, when the impression is made, being otherwise and perhaps more profoundly altered than in the former case, this modification may be propagated throughout the system. We do not imply, however, that the secondary or propagated affection is necessarily of the same nature as that which is constituted by the primary impression. It may be various. That inscrutable affection called idiopathic fever, and inflammation, may spring up simultaneously, and the latter appear in a part remote from that upon which the morbific impression had

(1) Ibid. pp. 735, 736, 737, 742.

been primarily made. And yet it often happens in these cases, that the local inflammation has manifest peculiarities. If the agent be prussic acid, then a destructive impression is propagated, and the organic forces of the brain, the heart, &c., may be immediately extinguished. The law is the same in all the cases. The results in distinct parts vary, because the primary impression is different in all the cases. We do not believe, as we have explained in our seventh section, that the nervous system has the entire agency; whilst it is highly probable that the sympathies may be sometimes more direct than through the medium of the brain and spinal cord, — that there may be a direct interchange amongst the organic viscera through the ganglionic system; and, as we are more and more inclined to believe, the more we study nature, through the vital properties of other tissues. The doctrine of reflex action having again become a novelty, it is scarcely probable that some will not impute to it all the phenomena of life. (1)

But this is certain, — that the organic properties of all parts, whose forces are independent of the nervous system, may be variously affected by impressions transmitted by the nerves. The mind does it in one way, the passions killing, or exciting disease in all parts; mechanical irritants in another; chemical in another; odours in another; whilst certain poisons either profoundly alter their nature or destroy them at once. But whilst they may variously act upon the organic properties through the medium of the nerves, it is also through a common principle. Still, we have as little doubt, that our various facts demonstrate a greater

(1) Müller, as it appears to us, has taken, in a general sense, an enlightened view of the whole of this question. Here we have very little else than *vitality;* and to do our author justice, this is the only part of his work to which we have seen any special objections made and those have been by the non-vitalists. But, for our own part, we only complain, in this instance, that he draws too broad a distinction betwixt the "reflex motions" and "sympathies." Many of his examples are exactly the same in both cases. At other times he speaks of them in a common acceptation, and even admits that there may be an analogous sympathy among other tissues, that may be entirely independent of the nerves.

However the sympathies occur in a circle in one case, or whether there be no intervention of a central organ, or whether the property of sympathy appertain to other tissues than the nerves, the phenomena appear to be very analogous. Bichat and Whytt, (*a*) and even Müller, have supplied many strong examples of sympathetic results betwixt parts which have no other nervous communication than the ganglionic. Dr. Park (*b*) has also an interesting essay on this obscure subject.

(*a*) Observations on Nervous and Hypochon. Affections.
(*b*) London Quarterly Journal of Science and Arts, No. 7, Art. II.

susceptibility of the organic properties to morbific impressions, and that these, in their turn, communicate the influences to the nervous system, by which they are extended to other parts, as well as through the independent relations of organs.

The nervous influence must always be regarded as an exciting cause operating upon the organic powers which mainly determine all the phenomena. The former, however, modifies more or less the results according to the nature of the remote causes. When inflammation is excited by dividing a nerve, or by paralysis, the organic forces are directly altered by suddenly withdrawing the nervous influence, as well as by thus deranging the mutual relations between the forces which animate the different tissues of a part; those relations being equally influenced by the nervous power.

No part of our subject is more important than that which we have just considered; and having dwelt upon it with some minuteness, with a view to economy we have endeavoured to remove some important obstacles at the same time; whilst we have also allowed our opponents to expound the laws of sympathy with which solidism is especially concerned. We now return to a consideration of the *modus operandi* of particular agents, which are specially regarded as illustrative of the doctrine of humoralism.

The experiments of Martin-Solon with morphine are, in every respect, a study for the humoralist. "If the point of a lancet, dipped in an aqueous solution of morphine, be inserted horizontally, about one line in depth, beneath the epidermis, and allowed to remain a few seconds, the following effects are observed. About a minute and a half after the operation, a small pimple, with a diffuse, rosy areola, is observed. In twenty minutes, the pimple becomes four lines in diameter, and one line in thickness, hard; areola an inch and half in diameter, and very red. After an hour, these appearances begin to diminish. Thirteen of these punctures produced heaviness of the head, and an invincible desire to sleep." The quantity of muriate of morphine employed "did not exceed a quarter of a grain," ( )

Here the local action of opium was obvious enough. How was it with the constitutional effects? This inquiry, we think, may be satisfactorily answered by its effects as stated in a communication from Dr. Burne. In a case of hydrophobia, in which the

(1) British and Foreign Med. Rev. vol. iv. p. 506.

symptoms were of the most violent and distressing nature, and where "no medicine could be given by the mouth, nor even put upon the tongue, nor by injection," about ten grains of the acetate of morphia were sprinkled on a blistered surface along the spine. "Scarcely had *one minute* elapsed when we observed the stare of the eyes and the dreadful alarm and anxiety of the countenance to diminish, then the violence of the spasm to abate, and the retching to subside; and, to our astonishment, this general amelioration progressed, till, in *four* minutes, the countenance had become placid, and the respiration free; the retching had ceased and the spasms vanished." "So wonderful an effect from medicine I never saw." (1)

Here, then, it will be admitted, if facts are to be at all respected, that the morphia, to produce the foregoing effect in "scarcely one minute," must have begun its constitutional action as soon as applied; and since, therefore, this was wholly a matter of local action and general sympathy, we suppose that the induction will be admitted as sound, that the effects which had resulted at the end of the fourth minute were owing to the same causes. The whole philosophy is explained by the principles which we have just seen laid down by Müller. But, referring again to the case by Martin-Solon, we may say that if the *entire* quantity of a quarter of a grain of muriate of morphia be injected into the veins of a large dog, it would be so diluted by the blood as to produce no effect upon him.

The action of croton oil affords an elegant exemplification of the law of sympathy under its different aspects; especially as sensation sometimes concurs with its other effects. In many subjects, on applying less than a drop of this substance to the tip of the tongue, a strong impression is immediately felt; and where the bowels are easily excited, the sensation is rapidly, and progressively propagated over the tongue into the throat and down the œsophagus. When croton oil is rubbed upon the abdomen, it frequently induces catharsis; and that it now operates through contiguous sympathy is highly probable from its failure to produce this effect when rubbed upon the extremities. (2) It is here, with this substance, as with many other applications that are made to the abdominal region to produce different impressions upon the intestines, and which fail of the result when made to

(1) London Med. Gaz. April 14, 1838.

(2) See a case of Ileus, in American Med. Intelligencer, vol. ii. p. 13.

other parts; as with blisters in inflammations, "antidynous applications," opium, fomentations, and other sedatives in intestinal suffering.

True, it is said of croton oil, that, "if it be absorbed it will purge; but if it be not, it will produce counter-irritation." "I have only seen one case," continues Dr. Stokes, "where there were both the eruption and the catharsis. This was a gentleman who had lately suffered from dysentery in warm climates."(1) Was not the application, in the latter instance, made to the abdomen, and in the others, at least generally, to other parts? But, however that may be, these are valuable facts. We only complain of the induction. We have seen it stated that there is no catharsis from croton oil when it produces an eruption, and it is, therefore, to be inferred that there is no absorption when it purges without disturbing the cuticle. Did this result depend upon absorption, it should, surely, be most common when the cuticle is abraded, as it is by the eruptions.

Waiving our suggestion as to the place of application, we think that an easy explanation of the facts as stated by Dr. Stokes, and in strict conformity with numerous analogies, may be found in the different modifications of the vital properties, and which is connected, of course, with the foregoing principle of contiguous and remote sympathy. Thus; one individual shall possess peculiarities which render the intestines susceptible of the cathartic impression, whilst others (p. 422) do not; just as we see of the effect of cantharides upon the bladder, when that substance is applied to the skin. Indeed, our able author has supplied an illustration of this, in the only example of imputed absorption which he has mentioned, where "the gentleman had lately suffered dysentery." Here there was probably remaining a preternatural irritability of the bowels, and a strong predisposition to extraordinary peristaltic movements. Again, in another case, other peculiarities shall exist, and there shall be, in consequence, no catharsis. And here it is important to say, that in most of the cases where frictions of croton oil are applied to the abdomen, there is torpor of the bowels.

Here is another case from the author with whom we have had the honour of holding the foregoing argument. It is the endermic application of quinine, in which the time of operation, and the manifest connection of symptoms, denote its local action, and

(1) Stokes' Theory and Practice of Medicine, p. 47.

the propagation of its action by sympathy. "Mr. Lembert states," says Dr. Stokes, "that in ten minutes after the application of quinine to a blistered surface on one of the extremities, a sensation of gentle warmth diffused itself *along the limb towards* the trunk, and *so on* over the whole body."(1)

While we are on this subject of sympathies artificially produced, and which supply so much proof for the solidist, we may advert to those more natural ones which unite all parts of the system in one harmonious action; or, those of a morbid nature, where it is manifest that derangements of one organ are constantly disturbing the actions of others through the same constitutional principle. It is exactly the same property, rendered most remarkably sensitive for its great final cause, which fulfils in one case the natural condition of intimate relations, and in the other presents those relations under another aspect. It is the same by which the brain, the lungs, the stomach, &c., in their healthy condition, subserve some of the important uses of each other, and through which those uses are impaired, and injuries propagated from one to the other, when either organ is deranged in its functions. The morbific influences which are propagated through the principle of sympathy, and the susceptibility of the vital properties of the solids to morbific impressions from external agents, are the unavoidable attendants of that natural constitution by which all the properties of life are rendered highly susceptible to agents of inferior power, to answer the intentions of organized matter; a final cause, in itself, which leads directly to the true philosophy of the operation of morbific and remedial agents.

Finally, as it regards the absorption of foreign substances, which are not directly alimentary, but which form component parts of the blood, as those of an alkaline nature, and some others not deleterious, they may be more or less freely admitted into the circulation; whilst provident nature has so endowed the lacteals, absorbents, and veins, that they exclude offensive agents, or only now and then admit them sparingly after those agents shall have established an alteration of their vital properties. "Take a purgative," says Bichat; "it contracts, shuts up the mouths of the absorbents of the intestinal canal. All the drinks pass off by the anus."(2) Is the purgative absorbed, when all else is excluded,

(1) Op. Citat. p. 356.

(2) General Anatomy, &c. vol. iii. p. 108. See, also, Fordyce on Digestion, to the same effect, p. 123.

and the intestinal membrane is pouring out its secretions? And yet the humoralists will have it that it must first enter the circulation before the purgative effect can take place, or the blood be altered back from its morbid state. If bile accumulate in the intestines, it is always rejected by the absorbing vessels. Even the father of humoralism says, "natura, suis viribus usa, quæ noxia sunt expellere noverit, quæ utilia usui servare."(1) All this is strikingly illustrated by the impunity with which many poisons may be swallowed, which act with great intensity when inserted into wounds, or injected into the circulation.

This leads us to speak of the common belief that alcohol may be absorbed in such profusion that the secreted fluids become inflammable, or the entire body liable to spontaneous combustion, — not excepting the aqueous parts which constitute so vast a proportion.(2) It has been even gravely stated, that "some experiments have been recently made in the United States, when the blood flowing from the arm of a man addicted to spirituous liquors actually took fire, being placed in contact with a lighted taper."(3) But, if the excellent author, who relates this anecdote, will look into Cook on Nervous Diseases, Stevens on the Blood, or the Cyclopædia of Practical Medicine, he will find that Americans are not alone in their credulity as to this "test of inflammability" of the blood. Dr. Apjohn remarks "that the bodies of drunkards may become, as it were, soaked with alcohol, seems fully established by observation." Still our author concedes that "the theory of alcoholic impregnation is, *per se*, quite insufficient. When a healthy individual dies of intoxication, his body is found but little, if at all, more inflammable than if he had perished by any other cause."(4) How is it with an *unhealthy* individual "who dies of intoxication"? Not in the least more inflammable, and neither are more so than water itself.

(1) Galen, L. de Nat. Facult.

(2) Haller says there are probably fifty pounds which circulate. (*First Lines*, &c., Sec. 149.) M. Chaussier dried in an oven a human body, the original weight of which was 120 pounds. It was reduced to 12 pounds. Here, then, are 108 pounds of water to be burnt up spontaneously. Lecanu states that about 800 of 1000 parts are water.(a)

(3) Millingen's Curiosities of Medical Experience, p. 60. We remember the case. It occurred amongst a sparse population, in the "backwoods," some years ago; but we have heard nothing of it since, nor has the phenomenon been repeated.

(4) Cyclopædia of Prac. Med. Art. Spontaneous Combustion, p. 452. And yet, say the combustionists, the blood will burn by the light of a candle.

(a) Journ. de Pharmacie, Sept. and Oct. 1831.

It has been even stated that alcohol has been found accumulated in the ventricles of the brain, and subjected to the test of the torch. (1) There can be no doubt the alcohol was placed there by the hand of mischief. Dilute this poison, even below its inflammability, and its contact with the internal organization of the brain will instantly, and fatally derange its functions. This has been abundantly shown by Fontana, (2) and other experimenters. Courton, (3) Wibmer, (4) Orfila, (5) Brodie, (6) Viborg, Bichat, and others, also found that three or four drachms of alcohol injected into the veins produced serious derangements; and if into the jugular vein, it occasioned immediate death. Pommer states that when he injected it in small quantities into any of the veins, it killed the animal, though it produced no sensible alteration of the blood; (7) and it has been lately stated that about an equal quantity of alcohol does not apparently affect the composition of the blood.

Nevertheless, we are not disposed to deny the partial absorption of alcohol, when the stomach is powerfully stimulated, and its organic forces thus artificially adapted. (8) (See p. 556.) But this is not our present inquiry.

Compared with the number of drunkards who have perished since human combustion became the compeer of animal magnetism, the ratio of reputed cases does not equal one in a billion. (9)

(1) See Boston Med. and Surg. Journ. vol. xii. pp. 117, 150, 236; and vol. xiii. p. 355. (2) Treatise on the Venom of the Viper, vol. ii. p. 371, &c.

(3) Philosoph. Trans. vol. xxvii. p. 485. (4) Die Wirkung, &c.

(5) Toxicol. Gén. (6) Philos. Trans. 1811.

(7) Schweizerische Zeitschrift für Natur und Heild-kunde, 1834.

(8) Since the foregoing was written, the experiments of Dr. Percy have been published. (a) It appears to be rendered in the highest degree probable, that alcohol is absorbed in small quantities by animals when injected into the stomach, so as to produce a powerful, and even fatal impression. The alcohol, however, is unchanged, and therefore adds not the least plausibility to humoralism. The alcohol is still as much the morbific agent as when it operates upon the stomach. And here, by the way, these experiments prove conclusively that alcohol often destroys by its local action upon the stomach, and the consequent sympathies of other parts. Thus, Dr. Percy: "In some of the experiments, total loss of sensibility and voluntary fever so instantaneously followed the introduction of the poison into the stomach, that we cannot conceive that absorption to a sufficient extent could possibly have been instantaneously effected. Hence there can be little doubt that alcohol can produce its fatal effect without being absorbed." We may add that Dr. P. allows that only small quantities were absorbed during the violent process, and that he failed of detecting it in the fluid of the cerebral ventricles.

(9) It may be well supposed that Dr. Apjohn has produced the strongest cases

(a) Exp. Inquiry concerning the Presence of Alcohol in the Ventricles of the Brain, &c., 1839.

The few, which we have been able to find amongst all sources, have been gathered from that loose authority, the common people, or the mere rumour of superstition. Old hags have been the usual subjects; and every instance which has come within our knowledge is destitute of the elements of a good story. And yet we are told that it is worse than sacrilege to doubt this climax of humoralism. We know it is for our interest to adopt the policy inculcated by Addison, who says: "there are some particular opinions in which a man should stand *neuter*, without engaging his assent to one side or the other. It is with this temper of mind that I consider the subject of *witchcraft*. When I consider whether there are such persons in the world as those we call witches, my mind is divided between the two opposite opinions; or, rather, to speak my thoughts freely, I believe in general that there is, and has been, such a thing as witchcraft; but at the same time, can give no credit to any *particular* instance of it."

But there are other circumstances touching this matter of spontaneous combustion, which ought not to be overlooked in a grave disquisition upon its merits; especially considering the prominent rank it holds in the humoral pathology. In the first place, it is credibly received that water will not extinguish the fire; and Swediaur and others have suggested urine for this purpose.(1) Although it be rational enough, since water is the substance

on record; and these being only six, we shall state them very briefly to confirm our text. We refer to the Cyclopædia for particulars.

1st. An Italian Countess, aged 62; appears not to have been a drinking lady, but addicted to the "constant practice of bathing her body with camphorated spirit of wine." A case, therefore, of intemperance by cutaneous absorption. Her ashes were found by the chambermaid, four feet from her bed, 1745. Nothing around her, that was really combustible, was burnt.

2d. A sober friar of Mount Volere, was found burning by his neighbours. "This case appeared in the Allgemine Literatur Zeitung, for 1786; and has been transcribed by most writers on juridical medicine."

3d. Mrs. Nelis, the drunken wife of a porter merchant of Dublin. Her ashes were found by her maid-servant. The fire in the room had burnt out. "There was no inquest, such was the anxiety of the family to hush up every thing connected with the occurrence."

4th. Mrs. Peacock, aged 60, of Limerick, a confirmed tippler. "Case extracted from the Methodist Magazine of 1809, as related by a Wesleyan minister." Burnt a hole through the floor, but nothing else.

5th. Mrs. Stout, "an inveterate dram-drinker, aged 60, county of Cavan.

6th. Mrs. A. B. aged 60, county of Down; a dram-drinker. Ashes discovered by the family; case communicated by the Rev. Mr. Ferguson, as obtained by him from the family.

(1) Thus proving the truth of what Scott says, that "a Lilliputian may kindle a fire which will take the diuretic powers of Gulliver to extinguish." *Diary' Nov.* 25, 1825.

burnt, that we should increase the inflammation by farther additions, this induction does not lessen the impossibility of the imputed phenomenon. Is it not, also, a little remarkable that this species of spontaneous combustion should confine itself exclusively to the living body; never occurring in the dead; especially seeing it is thought necessary to bring chemistry to an explanation of the phenomena? If the accounts had been limited to the dead subject, it might have been more difficult to have shown their fallacy, since they would have had a pretext in a remote analogy. And, then, as to the broken down old women, why should they be more combustible than the young, or why should not this penalty for tippling be equally inflicted on the other sex, who certainly drink in greater numbers, and more sottishly. (1)

The doctrine is, that a "saturation of the body with alcohol" is necessary to spontaneous combustion. But, some of the most respectable cases, where the body was reduced to "a heap of ashes," have occurred amongst the perfectly temperate. This paradox is always left out of consideration. It is evident, however, that some distinguished chemists are not satisfied with the theory. Thus, "Dr. Willis asks, whether, in those jolly people, who now and then expire of spontaneous combustion, the phosphorus of the body is properly eliminated by the kidney!" (2) And Dr. Stevens represents Bichat as having "long ago ascertained that hydrogen is absorbed in the lungs, and *communicates its inflammable properties to the living blood*, to such a degree, that the blood taken from a distant vessel produced a flame when a taper was applied to it." (3)

We might go on to multiply similar difficulties; but we will come at once to a case which we believe will serve as an interpretation of any real instance of combustion that may have been witnessed.

This case occurred under the observation of our friend Professor Lee, who stated it to us without the least design upon the popular belief in human combustion.

Being summoned to visit the corpse of an intemperate old man, he found him lying on the floor with his head and neck thor-

(1) "Women," says Dr. Apjohn, "seem particularly prone to it. Thus of seventeen cases collected by Kopp, sixteen occurred to females, while the subjects of the eight cases mentioned by Lair, are all of the *same* sex."

(2) Med. Chir. Rev. April 1839, p. 359. (3) On the blood, p. 156.

oughly consumed. As there was no fire in the apartment, he naturally enough supposed that fortune had thrown in his way a case of spontaneous combustion; nor was there any room for doubt when he discovered by the side of the remnant a jug of whiskey. None of the family were present to enlighten the doctor; and had the investigation stopped here, such is the reputation of Dr. Lee, that this case would have settled the question, — albeit in defiance of universal experience and philosophy. But the Dr. does nothing superficially; and accordingly he made it an object to call again, when he came to an understanding which was inauspicious to the hypothesis, for he was assured that the old man had tumbled into the chimney, and was there burnt up by a fire which had burnt out at the time of his visit. (1)

(1) The examples of human combustion may find their parallel in almost all the annals of man. We shall quote one instance on account of its physiological coincidence. We derive it from Dr. Elliotson's Human Physiology, (a) who obtained the facts from "a useful book entitled The Uncertainty of the Signs of Death."

"Mr. D'Egly, member of the Royal Society of Inscriptions, declares that he was engaged to a dinner for which the fish was to be provided by a Swiss diver, who got his living by plunging into the water, and pulling the fish out of their holes. The dinner hour arrived, but no fish. Drags were employed, and the diver's body found The curate wished to bury it immediately, as it had been nine hours under water; but Mr. D'Egly determined on attempting recuscitation, and succeeded in three quarters of an hour. The Revd. Mr. Derham, in his *Physico-Theology* is more credulous than the Curé. He quotes Pechlin for the case of a man pensioned by the Queen for having joined his fellow creatures again, after remaining upright under water, his feet sticking in the muddy bottom, for sixteen hours, at Troningholm. Yet, this is nothing; for Mr. Tilesius, the keeper of the royal library, has written an account of a woman whom he saw alive and well, after being three days under water. And, this is nothing; for Mr. Burmann declares he heard a funeral sermon at Boness, in Lithovia, upon an old man of seventy, who, the preacher protested, had fallen into the water when seventeen years old, and remained under it for seven weeks. Mr. Bydone was told that one diver, called Calas, but nicknamed Pesce, could live several days in the sea; and Kircher asserts, that this aquatic person could walk under water from Sicily to Italy." But all this is nothing; for Dr. Elliotson says that he can select his woman, and, by the power of animal magnetism, enable her to travel from London to Pekin, survey the Seraglio, &c., and back again, — all in five minutes.

Blumenbach (b) remarks that "the author of the *Recherches sur les Egyptiens* seems unwilling to admit that real mummies had ever been introduced at table; but his skepticism appears to me no better founded than the contrary assertion of one of the most eminent physicians of the last century, Casp. Hoffman, who, in his once classical work de Medicamentis Officinalibus, in the section of Egyptian Mummies, gravely relates, that in Lower Saxony no feast was ever given without the introduction of a mummy. And strange as this *qui pro quo* between an Egyptian corpse and a particular kind of Brunswick strong beer must appear, it is, however, a fact, that several more modern writers upon mummies have actually copied it out into their works with implicit confidence."

(a) Part 1, p. 225. (b) Philos. Trans. Lon. 1794. p. 195.

Finally, since all the well authenticated cases of spontaneous combustion have occurred in dead matter, it strikes us as not a little remarkable that we have never been provided with instances of human combustion in the dead subject, — especially such as die "dead-drunk." Or, if it be really true that the phenomenon is peculiar to living organized beings, we might show that the process is not a chemical one, and institute an argument in favour of our vital forces.

SECTION XII.

We shall consider in the present section the validity of the humoral inductions which have been extensively founded upon the morbid alterations of the secretions. They form, indeed, no small part of the quicksand upon which the whole superstructure reposes.

The various emunctories, in their natural state, elaborate from the blood specific fluids peculiar to each. Since, then, the liver will not secrete urine, nor the kidneys bile, nor the salivary glands the seminal fluid, and the specific character of each fluid depends upon the vital action of a particular organization, when that action is varied, there should be some change in the product. And that this change may be wrought by causes which can have no direct effect upon the blood is shown by the rapidity with which the bile and milk are altered by mental emotions.

Since, however, the blood is liable to a variety of alterations, perhaps from the glandular affections we are now considering, it is a matter of course that the fluids secreted from it should participate, more or less, in the change. If the blood have lost the constituents or the elements that are necessary to the formation of perfect bile or urine, the change may be apparent in those secretions. But this does not prove that "vitiated secretions" may not depend entirely upon the morbid action of the solids, nor that the blood may not have been deteriorated by the very influence of this diseased action. The kidneys, for instance, becoming diseased, may eliminate an unusual proportion of salts from the blood. A consequence of this simple change in the

blood will be, that the urine, from having at first a redundancy of salts, will now manifest a deficiency, although the kidneys remain diseased in precisely the same way. But the diseased state of the kidneys may exert a greater change upon the blood through the principle that that fluid depends for its formation and integrity upon the healthy action of the solids. It may, therefore, affect the very constitution of the blood. It may involve other organs by sympathy in a morbid state, and these superadded causes will still farther deteriorate the blood. The manifest consequence of such a change in the blood will be some change in the products that are derived from it. But it is important to remark, that during the whole of this complex operation, the blood is not an *agent*, but an object *acted upon*.

Again, it is true, the secretions may be variously affected by the nature of the food, since the blood is derived from it, and the secretions from the blood. This is one of the starting points of the humoralists. "M. Magendie," says Andral, "by changing the food of different animals, and consequently modifying their blood, has made the urine and bile of some of the carnivorous species resemble those fluids in the herbivorous."(1) This is carried up, by way of analogy, to show that morbid secretions are the result of an altered state of the blood. But, allowing what is very problematical, that the analyses were correct, what connection have these experiments with the ordinary causes of disease? What relation have they to the science of pathology, or even of physiology? There are no morbific principles in food; and however blood may be varied by the varying nature of the aliment, the variations are entirely owing to the action of the solids, and the nature of the supply. The blood is the *pabulum vitæ*. If we withhold the materials from which it is formed, it cannot be formed. If we exhibit food which has not the elements that are necessary to the secretions, or any part of them, those secretions cannot take place. "Out of nothing, nothing comes." However the secretions may be diversified, as in the foregoing experiments, by variations of food, it is in no respect connected with a morbid condition of the blood or of the solids. How, therefore, shall we designate that logic which would make these experiments a ground of conclusion that miasmata enter the circulation and ferment the blood before disease ensues in the solids?

(1) Patholog. Anat. vol. i. p. 417.

Carry the experiment still farther; subject a full feeder to a low diet, and although no noxious matter have entered the circulation, disease may follow, and with it a very different condition of the secretions from that announced by Magendie in the experiment just quoted; but the results, as we shall show, are foreign from the humoral pathology. But, in the present case, there is simply a variation in the natural elements that are supplied to the circulation, whilst the organs go on elaborating according to their supply. Or, again, where the food abounds most with a particular element, it may be less apparent in the excretions, than when other food is consumed, and where no disease is present. Thus, it is stated by Fyfe, that when carnivorous or omnivorous animals are confined to vegetable food, a diminished proportion of carbonic acid is evolved from the lungs.(1)

And yet, this natural phenomenon, manifesting itself more or less at every hour of our existence, is made the basis of analogical reasoning, to show that morbid products depend, also, upon changes in the constituents of the blood; whilst it is, at the same time, conceded that "many of them result from a purely local alteration in the part where they are developed."(2) Why, then, is not the solidist more entitled to the benefit of the analogy afforded by this admission, since a coincidence exists here in the morbid nature of the product, whilst, in the other case the analogy is drawn from the phenomena of health.

In the vegetable kingdom we find, however the soil may be varied, or the irrigating water impregnated with foreign matter not deleterious, that the sap and excretions, as various as the species of plants, are always nearly the same in the same individuals. If we graft a pear upon an apple-tree, we still have pears upon the parasite. Here, every thing is more simple, and yet nothing but vital action can explain the phenomenon. It is here that our philosophy should begin, nor be diverted from the fundamental principles by the apparent anomalies that may appertain to more complex organization; for how can it be otherwise, where, in the natural state of the animal, the analogous phenomena are determined by analogous laws, which have no type in the inorganic world? Man is fitted for every variety of food, and however he may compound or simplify it, we find his blood and secretions, in a healthy state of the solids, often unvaried, as in the vegetable kingdom, but again liable to mutations which we may

(1) Annals of Philosophy, vol. xiv. (2) Andral, Op. Citat. p. 417.

clearly trace to a slight modification of action which is determined by some emotion of mind. But, it may be otherwise with the brute creation, whose economy is adapted to the simple principle of instinct, which fits them, and directs them, to an uniformity of food. This, therefore, is another reason, for rejecting the specific inductions which have been extorted from the foregoing and analogous experiments. And being thus constituted, shall we believe that so great a violence as feeding dogs with butter and sugar, and horses with meat, will not inflict an injury upon the chylopoietic viscera, and derange the whole vital actions of the system, whereby not only the blood shall be deteriorated, but the entire glandular apparatus deranged in its actions? And admitting that we have been unjust to animal chemistry, and that the nicest exactness, and the *closest harmony*, have prevailed amongst the analyses and opinions of chemists, and that the variations in the constituents of the secretions have been truly noted as set forth in the foregoing experiments, shall we overlook, not only the fact that by the art of man the necessary elements had been withheld, but, what is far more probable, those intermediate processes of disease upon which all the anomalies may have depended? Still, under any circumstances, the blood is the only *pabulum*, and, according as its constituents may be varied by the natural sources of supply, so may they be variously decompounded, and new combinations formed, or certain original constituents may be elaborated without change, however improbable may be this last proposition.

Again, although the alterations produced by Magendie in the urine and the bile, by varying the food, depended remotely upon its qualities, we shall find in the ordinary changes to which it is subjected a farther proof that the formation of the secretions was a vital process; and not, as it was intended to appear, that their modifications "were independent of the secreting organs."

The blood, (for we must repeat it,) primarily derives its component parts from the food. But the secretions contain many substances not found in the food which may be exclusively eaten; and, therefore, no inductions of the nature we are considering, nor even as to the composition of alimentary matter, can be founded upon analyses of the blood or secretions. It is confidently said that there is no urea or lithic acid,(1) nor many of the

(1) Marcet says there is no lithic acid in the urine of graminivorous animals; (a)

(a) On Calculous Affections, pp. 40, 139.

principles of the bile in the food either of man or animals, especially of the latter, whilst eight of them are not known to exist in the blood, unless, by rare accident, they be absorbed after their formation. Nor do the late analyses show, with much probability, the natural existence of urea or lithic acid in the blood. (See APPENDIX I.) If, however, as is stated in the preceding note, there be no lithic acid in the urine of graminivorous animals, they produce urea, which contains more abundantly the peculiar constituent of the acid. The former is formed by reptiles after fasting for months; and Lassaigne found the urine natural in a madman who had fasted eighteen days. (1)

If we feed the viper exclusively with any one substance, will not its peculiar poison be generated; and so of the characteristic secretions of the civet, the cuttle-fish, the skunk, the beaver, &c.? Here there are tests which are liable to no deception; and they awaken a doubt as to chemical analyses. Do we find in the food thus administered any thing analogous to the products of which we speak? Is not the chyme of man and vertebrated animals one common homogeneous substance, however various their food? Or is there any ground for believing that the poison of the viper exists in its harmless blood, or the peculiar urinous principle of the skunk? Is not the virus of hydrophobia generated exclusively by the salivary glands, and this probably, only in particular species of quadrupeds? Has it not been often decided by inoculation; and will you explain the phenomena upon any mechanical or physical principles? Is not the virus of smallpox, of measles, &c., as much the peculiar product of the

yet it exists in the urine of man when he lives exclusively on vegetables. But this is only an *item*, to which may be added Dr. Prout's statement, that the urine of carnivorous animals abounds with this acid. (*a*) True, it is said that this is owing to the nitrogen contained in animal matter. But the chemists also assure us that the nitrogen of the air is absorbed into the blood, and united with it, in all animals, so far as known. But waiving this embarrassing fact, as well as the former in relation to man, whence came this principle, since animals are ultimately dependent for food upon the vegetable kingdom? There is, however, no free nitrogen gas in any animal substance. It is the product of the laboratory, which can never restore it to its original combinations.

But, what is especially important, if animals fed on vegetables do not generate lithic acid, they certainly do urea; and according to the best analyses, this substance is found to contain a greater proportion of nitrogen than the former.*

Again, "I think we may safely assert," says Dr. Prout, "that at least two-thirds of the whole number of urinary calculi originate from lithic acid." (*b*)

(1) Journ. de Chim. Méd. p. 272.

(*a*) Thomson's Annals, June, 1815 (*b*) On Affections of the Urinary Organs, &c. p. 99.

skin, as bile is of the liver? It belongs alone to the physiologist to answer the interrogatories.

These, and many other considerations which we have stated in other places, show the dependence of the blood and secretions, for their formation, upon vital actions, and of which chemistry affords no intelligible account. (1) From the same actions the blood and gastric juice derive, also, their vitality, which is freely admitted, but which appears to us strongly opposed to the participation of chemical actions.

However, then, we might for a moment admit, that the various constituents of the natural secretions may have been detected in the blood, shall we conclude, with a prevailing philosophy in humoralism, that such complex organs as the liver, and kidneys, act only as strainers, or in some mysterious way subserve the laws of chemistry; whilst the advocates of those doctrines admit that much of the action of the gastric juice upon food, and the conversion of chyme into blood, are the result of vital actions to which chemistry has no alliance? "We might as well say that astronomy is the physiology of the stars, as that physiology is made up of the physics of animals." (2)

The greatest advocate for the principal agency of physical forces in the formation of the secretions allows the vitality of the blood. Nor does he pretend that there is any thing in nature, save the living solids, that can impart vitality to the fountain from whence the solids, and other organic fluids, proceed; since he has no remote analogies for this extension of his hypothesis. And yet, he cannot find in those forces which impress vitality on dead matter an adequate power for the decomposition

(1) This was Mr. Hunter's doctrine. Mr. Palmer says that Hunter's "observation is no longer true." We ask for the proof, — having shown in our essays on the Vital Powers, and Digestion, that the chemists admit, up to this day, that nothing can be explained, in the living economy, without the agency of the vital forces. But, continues Mr. Palmer, "some important additions have of late years been obtained from chemical investigation of the blood with the present improved means of analysis," &c. (*a*) Again we ask for the proof, that chemistry has advanced our knowledge of physiology one step since Mr. Hunter's day. *We defy one clear and specific fact.* On the contrary, as we have endeavoured to show, in our first essay, it has been the greatest obstacle physiology has had to encounter. It has been a perfect incubus upon the science; and we believe the time is not distant when it will have proved its want of relation to our subject, if it have not done so already. Upon this question, even Dr. Prout concedes that nature will not permit the chemist to officiate as her journeyman, even in the most trifling degree. (*b*)

(2) Bichat's Researches on Life and Death, c. vii. p. 81.

(*a*) Hunter's Lectures on the Principles of Surgery, p. 26; *note*. (*b*) Gulstonian Lecture, 1831.

of the living fluid which results in the various secretions. He can see no analogy betwixt the vital formation of the blood by organized matter and eliminations from it by the same living vessels, — none betwixt those and the formation of the living solids. But, whilst he is compelled to admit that the living solids, and the blood itself in the larger vessels, resist all the ordinary operations of chemical agents and chemical laws, he passes with violent abruptness from admitted principles, and without offering any substantial reason for so great a transition, he tells us that when the living blood has entered the vessels where the vital forces predominate, and from which it mainly receives its impress of vitality, it there becomes subject to the forces of chemistry, which, in this organ and in that, are forever engaged in various, yet precise and uniform decompositions and recombinations, according to the part to which the blood may be distributed, but no one of which has the chemist been able to imitate.

Can it be, that whilst one series of parts are thus destined to perform the wonderful transmutation of food into blood, — bestowing vitality upon the elements of dead matter, — other parts, perhaps even more intricately constructed, and not less endowed with the principles of life, should perform, according to different sects of humoralists, the office of mere filterers of the blood; or, that their results should be sometimes determined by "vital properties" or "vital actions," and at other times by chemical or other physical forces? (See pp. 39, 75.)

Again, what possible dependence can the remarkable properties of the seminal fluid, and its animalculæ, if there be any, have upon the forces of physics or of chemistry? Can it be entertained that there is any thing analogous to it in the blood? (1) Why that labyrinth of tubes in the human testis, which, when unfolded, is computed by the microscopists to be but little short of a mile? True, we know that the ancient belief is even

(1) We are aware that it has been stated that the blood of a frog, after castration, has fecundated the spawn. But will any one believe it but the experimenter, any more than that the female ovum exists in the blood?

As an example of this philosophy, we may quote an opinion of the distinguished Professor Arnold of the Zurich University in relation to the subject now before us. "We are cautioned" in his Pathological Physiology, says the British and Foreign Rev. (Oct. 1839) "against the immoderate use of snails and oysters, as, by furnishing too large a supply of material for the *manufacture of semen*, they may exert a deleterious influence on the genital organs." The professor ascribes a great variety of local and constitutional diseases to the moon, and has great faith in the physiological effects of magnetism.

maintained at this day, by Sir A. Cooper, and others, that this apparatus is of no use, but that the semen is the product of those simple reservoirs, the vesiculæ seminales.(1) But what does this show? What imparts the solvent property to the gastric juice? This fluid is secreted, and has even more acid, when the parvagum is divided. From whatever parts of the body granulations spring up, they have all, originally, the same appearance. The same in bone as in muscles. But they are ultimately changed into the nature of the tissue from which they are produced. We know there must be specific forces to effect these results, and that in each different tissue they are modified; and we know that the results defy all explanation by any chemical or mechanical laws. We would also refer to what we have said in our remarks on inflammation of the nature and formation of pus; how its formation is indiscriminately imputed by the same philosophers to a spontaneous change in the large vessels, to chemical actions in the small, and to the decomposition of dead animal matter; how its analysis has led different chemists to opposite conclusions both as to its nature and formation; and how it is affirmed by the chemist to be unchangeably the same, whether the product of an abscess, of a chancre, or of the variolous pustule. The confusion in these respects is very remarkable, showing the perfect inadequacy of the principles by which the explanation is attempted; whilst they, who believe that animated nature operates by other forces, see nothing but admirable simplicity, and find a new source of delight and of reverential awe. In the same place, also, we have endeavoured to illustrate this subject by a reference to the varieties in mucus, lymph, &c., in health and disease, — showing how entirely impossible it is to expound their formation upon any other than vital laws. The farther, indeed, the chemist pushes his investigations, the more he multiplies proofs that the whole subject belongs to another department of philosophy. Whenever he

(1) Nothing can exemplify more strongly the fallacies of organic chemistry than its identifying the mucus of the vesiculæ seminales and the seminal fluid. Even Mr. Hunter was led to believe that the semen does not accumulate in the vesicles, because when one testicle is removed all the resources of chemistry could detect no difference betwixt the contents of one reservoir and the other. Yet who will doubt that one was a mucous fluid, and the other a product, chiefly, of the testis? Who will believe that there is any more real affinity betwixt them than betwixt the products of certain cancers, milk, pus, sweet almonds, *mucus*, &c. or vinegar and sugar, which, we are also told by the chemist, are identical. See APPENDIX on *Analogy*, *vol. ii.*

attempts an analysis of the compound fluids, he is at fault. If he begin with the chyle, which is the substratum of the whole, there is often a great discrepancy in the results of different analyses. Thus, it is said by Dr. Babington, that "it is very doubtful, from the discordant opinions of different chemists, whether the chyle contains more oily matter than the blood itself. Berzelius, indeed, makes its solid part to consist of more than 20 per cent of *fat*, and Raspail considers it as differing but little from *milk*. Prout, however, whose analysis is adopted by Turner, only admits an *appreciable trace* of oily matter in the chyle, and makes its composition differ but little from blood, except as respects the absence of red particles." (1) But the chemist has no difficulty with results that are clearly the product of chemical forces; but since we cannot even determine the composition of chyle, how shall it be said, as stated by Voisin, Tiedemann, Gmelin, and others, that "bile has no influence on its formation"? And so of albumen and fibrin, and every other constituent of organized matter, even to the saline principles of the blood and secretions. See p. 529; *note*.

Do we not find remarkable relations betwixt the structure of secreting organs and the matter secreted? Where organization is most complex, the secretions are most compounded, and, as the structure becomes more and more simple, so also do the corresponding secretions. And yet, in the most simple membranes, apparently of the same organization, the products, according to Cuvier and others, are almost as various as the different species of animals, — consisting even of fluids in some, and of air in others; yet always the same in each species. And so it is in the vegetable kingdom. "The fibres of a muscle of animal life," says Bichat, "would as well serve for the structure of any other muscle of the same system. The tendinous fibres, the cartilaginous, the osseous textures, &c. are everywhere the same. On the contrary, the texture of the liver would not serve to compose the kidneys, nor that of this last the salivary glands. The glandular system, then, has a resemblance in its different parts only by certain general attributes which have many exceptions." (2) Who can rise from Kiernan's description of the liver without a sense of mental weariness? And yet all this complex organization is precise, harmonious, and adapted to a specific

(1) On the Morbid Conditions of the Blood, p. 16.
(2) General Anatomy, &c. vol. iii. p. 193.

end. But its very complexity is fatal to all that is known of the simple processes of chemistry. Still the secretion of bile, which, according to the chemists, is compounded of forty different substances, and these made up of five elements, (p. 57,) is compared to what is supposed to be a chemical evolution of carbon from the blood; and the bile is also said to be merely "an excrementitious fluid that precipitates the chyme, and stimulates the intestines, without having any other functions,"(1) and the liver, by others, is called the "abdominal lung."(2) We are told, after assuming that urea is already formed in the blood, that "physiologists have been induced to suppose that the structure of the kidney is such that it allows the urea to percolate through the fine vessels emptying into its pelvis, like the mechanical operation of sifting or filtering, but denies a passage to the other constituents of arterial blood."(3) (See APPENDIX I.) But how "deny" them; why do they never escape; why do not the constituents of the bile come this way, and *vice versa*? Is it more difficult for one substance to "sift and filter" its "passage" through one set of vessels than the other, — especially seeing that all these vessels sometimes allow the passage of the red globules? (See APPENDIX II.) The same is affirmed of the liver and other organs, without attempting to show how such complex structures are adapted to such a process, why the products are so constantly the same, how other constituents of the blood are excluded, and perfectly regardless of the fact that many of the products have not been detected, or certainly but very equivocally so, in the blood. (See APPENDIX I.) The iatromechanical, it is true, are comparatively few with the iatrochemical philosophers. The latter, also, have greater zeal. They are more recently in a field full of seductive novelties, and other allurements. Their resources are vast, various, and mysterious.

Again, all the secreted fluids have not only an apparatus peculiar to each, whose complexity corresponds remarkably with the compound nature of their products, but each fluid appears destined for an important specific end in the economy of living bodies; a final purpose of which chemistry and physics are

(1) Ut Cit.

(2) Kiernan, &c. See, also, Brodie, in Quarterly Journ. vol. xiv. p. 341.

(3) Professor Draper in American Journal Medical Science, No. 41, p. 124. Professor D. however thinks that "perhaps it admits of very much doubt, whether any one of the operations of secretion is entirely due to this cause." — *Ibid.*

wholly incapable. One would be perfectly unsuited to the office of another, and would be even destructive of life, in most of the cases, should one product interchange with another. The saliva, gastric, and pancreatic juices are designed for digestion; the blood being thus an almost direct cause of its own reproduction. The bile subserves three specific purposes, which, when regarded in their connection, supply one of the most remarkable instances of design in nature; and the preservation of which in their uniform combination would seem almost to require the direct control of Omniscient Power. (P. 57.) This fluid participates directly in the animalization of food, is the important cause of peristaltic action, and performs, lastly, that inferior office which is often regarded as its only one, of contributing with the lungs, kidneys, and skin, as an emulgent of the blood. True, if we tie the duct of the liver, death will follow.(1) But does this show that the last mentioned function, is its great office? Suppose, however, that all the constituents of the bile exist in the blood, and that death follows from their accumulation; neither will this justify the conclusion, since those constituents are, at least, necessary to the specific purpose of precipitating the chyme, and maintaining peristaltic action. The precise

(1) From all that we have stated of animal chemistry, and of the speculations in regard to the office of the gastric juice, we can have no confidence in the experimental results of Leuret and Lassaigne, Tiedemann, and Gmelin, and the German physiologists, who profess to have found that chylification went on unimpaired after tying the ductus choledocus. The very injury inflicted upon the whole system, and especially the stomach, by this experiment, would immediately interrupt the whole process of digestion. Indeed, Sir B. Brodie stopped the process of chylification, entirely, by tying the duct.

Dr. Beaumont, and others, have endeavoured to determine the agency of bile by mixing it with chyme out of the stomach. But these experiments must be wholly fallacious. There is no mode, in the first place, of ascertaining the identity of the product with that of the natural process. The living solids, also, exert a great influence upon the vital fluids whilst they are in contact; and, indeed, the blood, the gastric juice, and, undoubtedly, the chyme, lose their vital properties almost as soon as deposited in the test glass of the chemist. The action of bile, therefore, upon chyme will be different out of the body from what happens in the body.

Again, if our conclusion as to the final causes of the bile be correct, this secretion should be found to exist, at least, in all the higher order of animals. This is not only true, but Müller (*a*) and Meckel (*b*) maintain that it is generated by appropriate organs in the orthopterous, coleopterous, and arachnidous insects. In proof of our conclusion, too, we may add, that "in the vertebrata, and in mollusca, the bile is poured into that part of the canal where the formation of the chyme is completed;" and that "its most abundant component, picromel, is not found in the fœces." (*c*)

(*a*) De Gland. Penit. Struct. tab. 8. (*b*) Archiv. 1826.
(*c*) Muller's Elements of Physiology, p. 153.

relation of the liver to the intestinal canal evinces its vital participation in the work of chymification, and other intestinal agencies. Indeed the final cause of the real "redundancies" of the blood, as they are called, which respect the liver, is, in part, to supply this organ with materials for the formation of its "forty" products.

The secretions of the serous, mucous, and articular membranes, are designed, for the most part, to facilitate organic and voluntary movements; the "humours" of the eye, and of the internal ear, are media of communication betwixt external objects and the nerves of sensation: and they are wonderfully adapted to the laws which they are intended to subserve. Fat is another instance of remarkable final causes which the secretions are intended to answer: and not less remarkable for specific objects are the secretions of semen, milk, &c. To these might be added many others, less important, but not less to our purpose,—as the poison of snakes and insects, the galvanism of aquatic animals, the ink of the sepia, the fluid from which the spider builds his house, and with which we cure intermittent fever,(1) &c. Each has its appropriate apparatus, and every product is derived from a common fluid, has its specific objects, and those objects are as the products. Can chemistry, or any department of physics, enlighten us upon these facts? As well might we attempt the solution, as we saw, indeed, had been devised by Müller in our first essay, by an algebraic process. p. 68, *Note.*

But, what is material to our object is the fact, that Babington,(2) Dumas,(3) Le Canu,(4) and others, have probably shown that blood is, in reality, a homogeneous fluid.(5) That, according to the former, at least, there is no such animal fluid as coagulable lymph, serum, &c., existing as such in the circulating blood, and that they are only formed and separated by appropriate vessels, in which even Müller sometimes agrees. (See p. 56, *Note.*) Still, it is manifest, that they must have a certain individuality in their blended state, since they separate, more or less, after the

(1) Jackson says it is better than Peruvian bark, or opium. "It acts like a charm" in curing intermittents, and in relieving pain and spasms." On Febrile Diseases, vol. i. p. 305; and Med. and Phys. Journ. vols. xxi. and xxii.

(2) Med. Chir. Trans., Lon., vol. xvi. p. 293.

(3) Princip. de Physiol. t. ii. p. 33.

(4) Etudes Chimiques sur le Sang. Humain. 1837.

(5) This is, of course, a fatal blow at all the microscopical observations of the blood.

control of the vital powers is lost. But their intimate union in the former state, and their uniform character in the latter, show, by their greatly diversified and always modified conditions, that when the analysis takes place in the living organization, the forces which govern dead matter have no connection with the process. How much more improbable, then, that the compound bile, &c. should be the products of inorganic powers. And when we regard, in connection, the homogeneous blood, the bile, the gastric juice, semen, milk, &c., and consider the uniformity of their respective composition in health, their changes according to the alterations of the vital properties in disease, and these changes corresponding with certain modifications of the vital phenomena, are we not moved with astonishment at the total difference in their nature? Is there any relief for our astonishment but in a firm reliance upon powers that are equally unique in their operation? Would not amazement otherwise increase, till it should prove that the human mind does not rightly interpret the laws of nature, and is unjust to its own endowments?

The poppy, digitalis, croton, spurge,—everything, growing side by side in the same earth, the same air, and watered with the same fluid, have, each one, its unique and unvarying sap and secreted products; an infinite variety of precise combinations derived from some four or five simple elements.( ) Apply all this to the formation of blood alone, which is composed of about the same elements, and we see how vain the attempts to explain by chemical laws even the formation of chyle; its conversion from a white to a deep red colour, and yet that colour changing to white again under the influence of slight disease, (p. 449,) and, finally, the remarkable composition of the blood and the vitality with which it is endowed. And, notwithstanding the complexity of the human body, its endless variety of food, and its artificial combinations and changes, has not the chemist given us a standard of the composition of the chyle, the blood, the gastric juice, the bile, milk, saliva, &c., by which their morbid changes are to be tested in all countries, at all seasons, at all ages of man and of

(1) "Here," said Leon, "is this delicate plant, springing and blooming, throwing out the same minute odour, as it has been doing since the creation day of its Parent Light, for six thousand years, through all the deluge, and that second deluge — the dark ages."(a) Nor does this rest alone on analogy, or any dictate of philosophy. It is shown by the germinating seeds which are at this day taken from the mummies of Egypt.

(a) Miss Reid's Passion Flower, No. xxv. p. 74. N. Y.

the world? Has he not told us, that all this is so uniform in the natural state of the animal, so unlike the results of chemical agencies, that when changes arise they are indicative of changes in health? And does he not offer to show, that this alteration of the blood and secretions is so uniform under the same circumstances of disordered health, that you may tell by it the nature of disease and its appropriate remedy? Is not this the basis of practical humoralism? We grant the fact as to the relation of specific changes in the secretions, perhaps the blood also, and specific modifications of action. But, is not all this in absolute opposition to whatever is known of the capricious operation of chemical forces? And what shadow of proof is there, that those vital forces, which the chemist now and then invokes to his aid, are not entirely adequate to the physiological results that are ascribed to the forces of chemistry?

But we would not be here misunderstood. We are considering either organic products, or such as exert a vital agency in the living economy. That chemical changes arise in fluids, like the urine, that are merely excrementitious, there can be little doubt. To this conclusion, indeed, we should arrive upon our own grounds. But these changes, we apprehend upon physiological principles, arise either immediately after the formation of those fluids by vital actions, or at any subsequent time.

Again, if we regard the analogies which prevail amongst various plants and their different parts, as it respects their growth, organization, functions, and even composition, and trace them to and up the animal kingdom, and consider, as we pass along, their want of analogies, in fundamental characteristics, with inorganic matter, we must look to other forces than those of chemistry for a solution of those analogies, and their abrupt termination at the confines of the mineral kingdom. Thus, nitrogen is found to abound in fungin, which is closely allied to the animal kingdom. Carbonate of lime is the most common mineral ingredient in vegetables, and enters largely into the lower animals. Silex occurs in the algæ, and especially in the more complex plants, whilst it is the consolidating principle in the skeletons of many poriferi, which stand on the border of the animal kingdom. Here are slight similitudes to the lower kingdom of nature: but if we pursue the investigation, we shall find that they are rather analogies in the higher departments. The infinite diversities that occur in the configuration of parts, yet uniform in every species,

as in the bones, muscles, &c., leaves, petals, calyx, stamens, pistils, fruit, bracts, and scales of plants, all of which are only modifications of one structure or organ, whilst identity of composition produces identity of form or of crystalization in inorganic bodies; the modes of nutrition through roots among vegetables, and very generally an alimentary canal, or an internal cavity in animals, (Müller says in all,)(1) a similitude which has an analogy in the respiratory organs of aquatic and atmospheric animals; the manifest resemblance of certain organs in plants to the stomach of animals, as in the dischidia rafflesiana; the disappearance of the alimentary canal in animals that are closely allied to the vegetable kingdom, and their external absorption of nourishment; the analogies appertaining to the phenomena of parasitic plants and animals; the production of organized beings by their own species, perpetuating their individuality ad infinitum; their interstitial growth, whilst inorganic substances grow by aggregation; the power of the former of generating actions within themselves; the constant mutation to which organized matter is subject, yet constantly retaining its qualities, whilst inorganic bodies exist only while their elements are at rest; the conversion by vegetables of inorganic into organic matter, and investing it with their own endowments; the exclusive subsistence of animals upon vegetable or animal substances; the necessity of aliment to organic bodies, whilst there is none at all to inorganic; the heterogeneous components of animals and plants, whilst minerals are generally composed of homogeneous parts; the former compounded of solids and liquids, whilst the latter of solids alone; the conversion of fluid parts of the organized body into its solids, and the return of these into fluids; the elementary forms of organized bodies consisting of corpuscules, and their entire absence in mineral substances; the complex union of tissues that result in the manifestations of life, and yet with an astonishing unity of organization,—the absence of all analogy in this respect in the lower kingdom; the investments, by skin and bark, of all organized bodies, and their disappearance in minerals; the reciprocal dependence of all parts of organized beings, upon which their existence and the perpetuation of their species depend—"consensus unus, conspiratio una, consententia una," "omnia similiter principium et omnia finis,"—whilst no such relation of cause and effect, of means and end, exists among

(1) Physiology, p. 487.

the aggregate parts of minerals; the consequent indivisibility of one and the divisibility of the other; or, if there be rare exceptions in relation to the former, each part possesses the qualities of originating actions, of interstitial growth, and of reproduction; the renewal of matter being indispensable to the continued existence of organic, whilst it is destructive to inorganic bodies; the exclusive property possessed by animals and vegetables of generating heat and maintaining an uniform temperature, and their resistance of the law of communication as it operates in dead matter; the resistance which is opposed by the animated kingdoms to the forces which operate in the inorganic; the modification of their physical properties, as seen in the variableness of specific gravity, their cohesion and adhesion, fluids moving in opposition to gravitation, &c.; the analogies which prevail, and which alone exist, in the higher kingdoms in all organic processes; a specific organization in each for every distinct product, and the uniformity, in each, of these products; the coincidences which attend their circulation; the mutual and exclusive relation to each of atmospheric air; the subjection of both to vital stimuli; the analogous morbid changes which are produced in both by many common agents; the disappearance in both of all the phenomena which distinguish them from all other species of matter when nutrition is stopped, or when various agents operate upon them; their immediate subjection, in consequence, to the laws which prevail in the inorganic world; besides many other facts and coincidences which contradistinguish the organic from the mineral kingdom, show us, in this combined aspect, that the whole subject, in relation to causes, has probably no connection with chemistry.

We are asked, what is the nature of the vital powers by which secretion is performed? This inquiry comes with an ill grace from those who can tell us nothing of the forces of chemistry, and of physics generally, but what is manifested by their effects. It is in part, however, to satisfy this inquiry that we have just recited some of the prominent characteristics of organic life, that they may be placed in ready contrast with the narrow means of knowledge which relate to those forces that preside over inorganic matter.

The attempts which have been made by some physiologists who believe in their specific difference, of whom Hunter is one, to illustrate the nature of the vital powers and their modes of

action by comparisons with the laws and phenomena of dead matter, appear to us wholly inconclusive; since, being totally different from each other, as the theory supposes, there can be no sufficient resemblances, and no reasoning from one to the other. And again, as we know nothing of the forces of physics or of chemistry (1) beyond the results of their action, they cannot help our knowledge of the vital properties, whose phenomena are more numerous, and quite as manifest to our senses. It is only by certain phenomena that we learn the existence of electricity, to which so much has been ascribed in the animal economy; and what is not a little remarkable, as we have shown in our essay on the Vital Powers, some physiologists, who suppose that this is the great principle of life, tell us that it is probably only a state of nothing; (p. 49.) We enter our protest against the vitalists as to the foregoing illogical method of induction, and the tacit admission that the science of life requires extraneous help. (See p. 43.)

The ground which we have taken in this discussion renders it important that no aspect of our subject should be neglected. In connection with the foregoing remarks, we may ask if it be not in the highest degree probable, considering the variety of morbific agents, that, on uniting chemically with the blood, they would establish a great variety of specific changes in that fluid; that each one, from the intimate and important relation of the blood to the solids, would produce a specifically distinct disease of the solids? Even mental emotions and mechanical irritants, which develope, alike, the same exact conditions of inflammation, should, if the blood were chemically altered, affect its constitution in different modes; and this morbific cause incessantly pervading the capillary system, and uniting with the solids, should establish very dissimilar changes in the latter. And so of all other morbific causes, according even to their shades of differences. Every article of the materia medica, in sufficient doses to excite disease, should produce specific changes in the blood, and therefore in the solids, especially since the former is converted into the latter. Not so, however, with the vital forces of the solids. Here no specific products are formed as in the chemical union of foreign agents with the blood, or when that fluid may be otherwise changed in its constitution by such

(1) We use these terms conjointly, for the sake of precision, although the former embraces the latter.

agents. But we witness, what reason would suggest, in respect to the vital properties of the solids, a great variety of modifications, constantly running into each other, and thus corresponding with the established nature of the forces of life, with the fixed condition of the solid parts compared with the blood, and with the varieties which exist in the properties of morbific agents.

But, however the vital properties of the solids may be altered by certain causes, there are large classes whose morbific principles are allied to each other, and therefore induce, as in the genus of inflammations, only corresponding varieties of morbid action, whilst the same agents uniting with the blood, or otherwise altering its constitution, would produce results in the solids as radically different from each other as the alterations of the pabulum vitæ.

Again, the diseases which prevailed at the earliest records of medicine exist now, and are distinguished by the same vital phenomena, and require the same means of cure; from which we may also infer, that the conditions of the blood and morbid secretions are the same. But, from the ever changing results of chemical actions, and the varieties which must necessarily arise in the nature of morbific agents, there should have been some striking mutations in disease through this long lapse of time, were the humoral pathology founded in nature. If climate, &c., have produced some variations, they are only varieties, and not those essential changes which are the constant results of only slight variations in the proportions of two simple elements which unite chemically with each other. These modifications, too, tend very strongly to corroborate the doctrine of solidism, since the vital forces are liable to corresponding differences in change from the differences which appertain to many non-humoral agents. And since these different influences require some variation in the details of treatment, yet always determined by the same general principles, where analogies prevail, it illustrates still farther the dependence of the whole upon forces which are peculiar to living matter. These coincidences, which may still be foreseen through all coming time, bear no affinity with any thing that is known in chemical science.

Suppose, again, that we endeavour to imitate the human machine, and with the aid of "pepsine," &c. to have contrived a fluid precisely like the blood. If it be contended that our art reaches not to such a "presiding genius" as the brain, no chem-

ical philosopher will object to a Galvanic battery as a substitute. Besides, it has been sufficiently shown that the brain is not indispensable to the formation of the secretions in a chemical sense, or to organic actions. Thus provided with the most perfect model, and the ability to regulate with precision the galvanic fluid, would it not be absurd to suppose that we can obtain a single result which arises from vital organization? The blood would be as liable to be decompounded in one part as another, and it is plain that we should have all the confusion that ever distinguished the laboratory of the alchymist.

The chylopoietic and sanguiferous organs separate and combine the elements of food, and endow the compound with vitality. So, also, do the glandular organs, the membranes, &c. select certain constituents from the blood by virtue of their vital properties, and impress upon them various peculiarities, according to the mechanism of each tissue, and as these properties happen to be modified. Consider, also, the peculiar and uniform changes in the blood itself, when this fluid, so modified, forms one of the natural secretions. In the menstrual flux, we need not say that it requires a uterus to impart the specific character to the matter secreted. Here, too, are important final causes, even as it respects the incoagulability of the blood; since, were it otherwise, it would block up the womb, become a source of irritation, and might become organized. Here is the modified blood itself, palpably a secretion; but will any one pretend that, altered as it is, its excretion is the result of mechanical exudation, or its change is owing to chemical actions? Why then is not the same philosophy applicable to all other secretions? "Taking the blood as a whole," says Dr. C. J. B. Williams, most truly, "we find that various matters are added to it and separated from it in different parts of the system; and no one of these matters resembles, in composition, the blood itself."(1) Müller, even, carries this doctrine to all the important constituents of the blood. (See p. 56, *Note.*)

Considering, then, the vast variety of unique products which depend upon the vital properties of the solids, how very probable that whenever those properties may undergo any change, so also, will their results be varied? Nor is it consistent with reason, that the component parts of a homogeneous substance like the blood, so highly endowed with vitality as to lead Andral,(2)

(1) London Med. Gaz. vol. xvi. p. 842. (2) Patholog. Anat. vol. i. p. 394.

and other able philosophers, to infer of its coagula that they may create organs within themselves, and carry on an independent life, should be the subject of chemical agencies, or should be "strained off like mechanical mixtures, as through a sieve." The various parts of the blood are held together by the properties of vitality, and they require for their separation, in the living system, whose most remarkable characteristic is its resistance of chemical agencies, the specific action of analogous forces. Otherwise, also, no reason can be assigned why the kidneys should not strain off bile; the liver, urine; the serous and cellular membranes, either of those products, &c. or, why great confusion, if chemistry operate, should not prevail in the component parts of those fluids. But the astonishing facts which actually exist are an absolute proof to our mind that the eminent reviewer whom we quoted in another place, (p. 529,) is right in his induction, that scarcely any of those substances which the chemist detects in the secretions, or in the blood, have an existence as such in the latter fluid.

These principles may be extended to every part of the body, and they should apply equally to the most simple structure as to the most compound,—equally to the results of the varying changes of the solids, or to the blood in any of its deviations from the natural standard. If the foregoing philosophy be true of the various parts of the system, and its various results, in their normal state, it is but a common dictate of the understanding, that *it is* equally so of all their modified conditions. We cannot have a vital process for the natural elaboration of serous, or other secretions, and a physical one for their morbid alteration, and their redundant effusion. Indeed, these very alterations of the secretions, especially from certain causes, not only establish the vital nature of the action upon which they depend, but exclude the humoral hypothesis. A blow upon the head, moral emotions, &c. may speedily produce a morbid state of the bile, and of other secretions. (1) Here is no room for imagination,—no pos-

(1) In addition to what we have said in our fourth Section, p. 427, as to the influence of moral causes upon the secretions, we may state a curious fact, as ascertained experimentally by Mr. Hunter. "In the ass, the secretion of milk is not continued, (as in the cow and goat,) after the mother has lost the impression of the existence of the foal. The keepers, therefore, take off the skin of the foal, and preserve it, so that it may be occasionally thrown over the back of another foal, and smelled by the mother, more particularly *at the time they are milking her*. The ass, under the deception of having her own foal, gives down her milk, and the secretion is carried

sible chance for the chemist or mechanical philosopher. What, therefore, is so manifestly the result, in a vast number of cases, of the vital action of the solids, cannot be explained in other instances by totally different causes, without a sacrifice of all philosophy. We believe that this doctrine is founded in nature, and must yet prevail; and we have, therefore, no hesitation in connecting our fortune with it.

To the general principle we have stated, however, there is one seeming, though not a real, exception, as we shall have shown by different facts. It applies to foreign substances which may exist in a state of mechanical intermixture with the blood. Such substances may possibly be elaborated by many parts of the body. Nor is this alone applicable to such matter as may be introduced artificially, or by absorption, into the circulation. It is so with what may be regarded as natural constituents of this fluid when they do not form a part of the homogeneous substance, and which may or may not be analogous to certain component parts of this substance. The supposition of their existence, however, as derived from alimentary matter, must be, from what we have shown, more or less hypothetical, and contrary to what is supposed to be known of the chyle, as well as to certain physiological facts of which we have spoken. Still, we admit it for the sake of examining our subject in its various aspects, and to anticipate, as far as we may, any doubtful points that may be raised, and from our earnest desire to respect nothing but the truth. So is it, also, with the bile, and urine, should they be absorbed. And although we shall have endeavoured to show that they are not taken into the circulation, or, at most, but very sparingly, we shall suppress nothing that is urged against us. Thus, in a very strange case by Dr. Lyncker, it is said that urine, in consequence of its obstinate retention, made its escape from the nipples, next from the navel, and, finally, in great profusion from the legs; its elimination being confined to these parts.(1) At other times, it is said to

on as usual, and she is kept in milk; but if this artifice is neglected, she soon goes dry." (a)

(1) Caspar's Wochenschrift, No. 16; and other writers.

We think our general facts in physiology, and the arguments which we have founded upon them, should be sufficient to set aside the foregoing statements. But where we are true to nature, we have always direct proof enough at hand to sustain our position; and perhaps, therefore, it should not be withheld. We take our latest

(a) Lon. Quarterly Journal of Science and the Arts, No. 2, Art. II. See, also, Essay by Dr. Park, "On the Influence of Mental Impressions in producing Change of Function in the Living Body," in Ibid. No. 8, Art. V.

have escaped, equally without change, from the liver and other glands. So, also, we have already seen it affirmed by high authority, that milk has made its escape in the same way, and that a dose of castor oil has exuded from all parts of the body.(1) It has been also stated by Harrison, Apjohn, George, Murray, and others, that crystalline formations, which they have taken to be of an urinous nature, have been found, or, at least, suspected, in the heart, brain, nerves, bowels, &c. Sir J. Murray, of course, proposes, that chemical agents should be administered in these cases.(2) But the discoveries are altogether a post-mortem affair, and are not only liable to great suspicion from what we have said of animal chemistry, but from the well known fact, however denied by some, that urine is in the highest degree morbific when

journals for the appropriate facts. Thus, here is a case of suppression of urine of twelve days' duration. It is Dr. Doring's, (*a*) published for the purpose of showing that Adelon was wrong in stating that the secretion of urine cannot be arrested for three days without resulting in death. But, in this case there was neither renal nor vicarious secretion during the twelve days. And here is another, by Prof. Dunglison, where the suppression existed for ten days, and apparently without any vicarious discharge. (*b*) But "a singular case of ischuria," which should forever settle the question as to the absorption of urine, is related in that work of facts, the New-York Med. Repository. (*c*) The ischuria continued for eight weeks; but there were no symptoms that denoted an absorption. Sometimes "half a gallon" would come away; and, on dissection, nearly a gallon of urine was found in the bladder.

To the same effect, also, we might refer to a vast variety of renal diseases, in which the urine ceases, more or less, to be elaborated, without being attended by those symptoms which follow the introduction of a small quantity of this fluid into the circulation. In a late case by Dr. Hayes, of the foregoing nature, "the left kidney was a mere sac, containing about 12 oz. of purulent fluid, not a vestige of any thing like kidney remaining. The right kidney distended with purulent fluid to about the same amount, with about one-third that was comparatively healthy. The pelvis of this kidney enlarged to the capacity of 3 or 4 fluid ounces. Both ureters from ½ to 1 in. diam. their whole length. At the lower extremity of the left ureter, was a calculus of 1¼ in. in length, weighing 1 oz.," and another in the bladder weighing 4 oz. (*d*) In this case, it is manifest that the secretion of urine had been long extinguished in the *left* kidney, and but very partially performed by the right. Yet "the patient was able to walk about the room until the last few days of his life."

We have many examples analogous to the foregoing in the books upon our table. But we think the question should be left to what we have now stated. See Coulson on Diseases of the Bladder. 1838.

(1) The case of Dr. Hale, who tasted the oil, is in no respect in point; since this intrepid physician had injected it into his circulation. And yet the humoralists have even argued from this case that castor oil is absorbed by the stomach.

(2) Report of the British Association, 1837.

The crystals "did not amount to tangible particles," and were only to be seen by the microscope.

(*a*) Dunglison's Amer. Med. Intelligencer, vol. i. p. 194. (*b*) Ibid. p. 231.
(*c*) Vol. vi. p. 117—122. (*d*) Boston Med. and Surg. Journ. Aug. 14, 1839, p. 9.

injected in small quantities into the circulation, and from the fact, also, that foreign matter is rapidly eliminated from the circulation; but, especially, from the greater fact that the aforesaid crystals are of a soluble nature, and the blood constantly receiving a fresh supply of aqueous, solvent materials. But, perhaps, the most remarkable of all physiological perversions on this subject, is the doctrine that urine may be formed, *de novo*, by other organs than the kidneys.(1) It is a principle with which all parts appear to be more or less endowed, that when an important excretory organ fails in its office, they shall cöoperate for the general good in removing foreign causes that may be more or less offensive to all parts. This harmonious relation amongst the various organs of the body is every where manifested in their healthy functions, and is one of the striking illustrations of the vital nature of the properties by which the functions and their products are determined. Whilst, however, many organs may join in the removal of foreign matter from the circulation, others will remain passive in regard to some particular substance. But again, in respect to another substance, (all being perfectly soluble,) the organs that were passive in the former case will take an active part in the work of expulsion,—the brain, however, and some other parts, very rarely, if ever. Now, what does all this signify? That those substances were strained off from the blood, when one organ, in both the cases, is as good a strainer as another? We will answer. Every organ has its peculiar allotment of the forces of life, and when substances, whose properties do not harmonize with those peculiar modifications, or establish a relation to them by changing their nature so as to adapt them to the elaboration of the offending agents, they are rejected;

(1) Here is a case from the Transactions of the King's and Queen's College of Physicians, in Ireland, (vol. i.) which shows the source of the error, whilst the scanty production of urine goes, with our other facts, to our general statement. The subject of the case died of dropsy. On dissection, it is said that the ureters were entirely obliterated. Here, then, is room for mistake; and what renders it especially probable, besides the reasons already assigned, that the ureters were not wholly impervious, is the fact that urine was regularly discharged from the bladder till the death of the patient, and there were no characteristic symptoms of its absorption into the circulation. But such was not the construction. It was supposed that the secretion of urine was performed by the mucous membrane of the bladder; no other part participating in this strange anomaly. And yet this is only parallel with the hypothesis which ascribes the production of semen to the vesiculæ seminales, and, perhaps, does not much more surpass the limit of philosophy, than the inquiry into channels of communication between the stomach and bladder. See APPENDIX on *Analogy*, vol. ii.

whilst they are carried off by other parts where those relations exist, or are produced. This, as it appears to us, is the whole philosophy of the subject, which is utterly insusceptible of explanation by any known principles in physics. (1) Nor does the interpretation require the equally absurd hypothesis which respects an intelligent agent.

But the foregoing is a natural, not a morbid process; and the products, however they may differ from the natural secretions, are composed of the redundancies of the blood, and are wholly unlike those vitiated secretions that are consequent on morbid action. In the former cases, the principles which may be absorbed into the circulation, or the accumulation in the blood of unknown substances in consequence of the suppression of some important excretion, are eliminated mainly in the state in which they exist, as is shown by their coincident appearance wherever they are cast off. In the other case, these coincidences have no existence; the products are every where unique and peculiar to each part, just as they are in their natural state; and therefore, they have been, not only as in the former case, the products of vital actions, but, as in the formation of natural secretions, the same actions have decompounded and recombined the elements of the blood in a way that makes up the morbid products, and in a way, too, which is as peculiar to each organ as the morbid products of each are of a specific kind. This is also demonstrated by the effect of remedial agents. In one case, nothing will suspend the general excretion, so long as the foreign matter continues to circulate; whilst in the latter, a single bloodletting, or a dose of calomel, may speedily convert a morbid into a natural secretion, — a very difficult phenomenon, by the way, for the humoral pathology. In the case of redundancies, however, there may be, and probably often is, whenever they happen, a morbid state of the solids, and then a complex condition arises as to the state of the secreted products, which involves both of the foregoing analyses. In either case, too, the blood may, or

(1) "I have said twenty times, and I repeat it again," says Bichat, "that the only cause which prevents the red globules from passing into vessels with white fluids, is the want of relation between the nature of the fluid and the sensibility of the organ. The opposite opinion is of the school of Boerhaave." (a) This is beautifully shown by Buniva's experiments, in which he had great difficulty in effecting an injection of an artery of a living dog; but the capillaries ceased their contraction when the upper portion of the spinal marrow was divided, and the contents of the syringe passed freely on.

(a) Gen. Anat. vol. ii. p. 74.

may not, be diseased, and thus contribute, as we have explained, to modify the secretions. But all the changes, from first to last, are determined by the action of the solids, and through their action the blood is restored.

Again, the secretions of different organs are often modified in a very uniform manner in certain diseases, but vary at different stages of those affections in exact correspondence with the changes in the vital signs. This we shall show upon the highest humoral authority. It may be seen in the discharge from the nose in the inflammatory affections of the schniderian membrane; in the specific changes that take place in the matter expectorated at the different stages of phthisis, in the common variations of purulent matter, or in the colour of the urine in the acute stage of that disease, — it being "of a deep red, as if it held blood in solution," "and we shall almost always find that the solution of the disease, even when effected by repeated venesections, is attended by a critical white deposition in the urine;" (1) in the formation of sugar in diabetes, and another variety in lactation, &c. And yet it is, in part, upon the foregoing facts that humoralism is again advancing, and the doctrine, "mutata figura pori, plures alii diversi generis particulæ per eam secernerentur," is again pressed into service. The whole ground, however, is the same that was anciently trodden, and which, from having become a wilderness, may appear to be newly discovered land.

In respect to the absorption of secreted matters into the circulation, perhaps we have already said enough. But there are certain specific affirmations which seem to require a special notice. Thus, M. Andral speaks of the "bile *passing into the blood*" as something of such frequent occurrence as to have suggested the foregoing popular phrase, "which happens," he says, "to be *consonant* with the *science*." (2) So, too, that able philosopher, Dr. Stokes, — "the urine is loaded with bile, and chemical analysis shows that a large proportion of the biliary secretions is blended with it." (3)

Such affirmations, from such sources, would certainly demand

(1) Lænnec on Diseases of the Chest, pp. 224, 225.

(2) Patholog. Anat. vol. i. p. 414.

(3) Theory and Practice of Medicine, p. 101.

Dr. Stokes says that whilst all other secretions are coloured with bile, "the milk during lactation, appears to escape the general impregnation. This, he thinks, would appear to be a *beautiful provision of nature* to prevent the child from being injured." But, since it is admitted that the milk so impregnated would be deleterious

an implicit confidence, were they not wholly contradicted by the results of that art upon which humoralism rests its hope. According to the experiments of Lecanu, even in jaundice, with the exception of the colouring matter in the proportion of less than one part in one thousand, there is to be found in the blood no other specific constituent of the bile. Another chemist not less profound, (Mr. Kane,) (1) could find no picromel, or resin, or cholesterin. The yellow colouring matter, alone, was all that could be detected. The proportions of salts were natural, and the red colouring matter had its "healthy average." And yet there are still some five other constituents, at least, of the bile which are said by chemists, who have no interest in humoralism, to be peculiar to that fluid. Mr. Jennings, after giving a table

to the stomach of a child, may we not carry the reasoning a little farther, and conclude that were the circulating blood impregnated with bile, its pernicious effects would be universally and far more injuriously felt by the mother?

It is also very appropriate to the foregoing subject, and illustrates the influence of hypotheses upon specific inquiries, that Dr. Stokes, in speaking of Dr. Grave's theory of the absence of yellow vision in jaundice, says, "it may be objected to this, that when all the fluids, the blood, saliva, serum, perspiration, &c., are impregnated with bile, how is it *possible* that the fluids of the eye should escape? Does it not seem very extraordinary? It does, certainly." (*a*) But not half so extraordinary, Doctor, as your affirmation on the preceding page, that the milk escapes impregnation, whilst here we have it, that "*all the fluids* are impregnated," and the impossibility is implied that even the humours of the eye should escape. Why is not Graves's final cause as good as yours, and why not ours better than either? For it is worthy of remark, that Dr. Graves reasons, in relation to the eye, from the same final cause, and *in the same language*, that we have seen employed in regard to the milk; that is to say, the exemption of the humours "*may be a beautiful provision of nature* for the preservation of the sight." (*b*) In making all these exceptions, and "beautiful provisions" for comparatively unimportant purposes, is it not highly probable that nature has also considered the well-being of the brain, the stomach, — aye, and even of the organic constitution of the liver itself, and that to carry out this great purpose, she has so contrived the vital forces of the absorbing system as to reject, at least, those principles of the bile which would irritate the external part of any organ, or affect the vision like a pair of green spectacles? Indeed, we had employed this argument in a general sense, (p. 546,) before we were aware of its partial use by Drs. Stokes and Graves.

The humoral doctrine requires the broad shoulders of Dr. Stokes, and we are content, so only he puts the matter right at last. We have ascribed something, in the present instance, to the influence of hypothesis; and this is partially true as relates to the merits of humoralism. But there is another object in changing the ground when Dr. Graves comes forward with the same hypothesis that was used in regard to the milk; and this is, in upsetting Dr. Graves, to offer an explanation which Dr. Stokes "thinks has not been taken notice of before."

(1) Dublin Journal of Medical and Chemical Science, vol. ii. p. 346.

(*a*) Ut Supra, p. 102. (*b*) Ibid. p. 102.

of four analyses of jaundiced blood; two by Lecanu, one by Kane, and one by himself, remarks that "it does not appear that the general constituents of the blood are, in this disease, materially altered from the healthy standard." In all the analyses, there was an "absence of cholesterine;" though, it is added, that this may readily be obtained from healthy blood. (1) The latter may appear very probable to some, from the absence of cholesterine in jaundice.

Thus it appears, again and again, that the more these strong examples of humoral disease are subjected to analysis, the more perfectly are they exploded, and the more are we justified in our suspicion of mistake, or of assumption. What especially shows the importance of attending also to alleged facts, as well as to general affirmations, where a favourite hypothesis may be at risk, is the statement of M. Andral, that the constituents of the bile have been found deposited in different parts of the body, as well as a like affirmation which, as we have seen, has been repeatedly made of the urinary formations. Nor would we, in saying this, in the least degree impeach the veracity of distinguished observers, which, in a general sense, would be very unjust, and adverse to science. Dr. Fordyce long ago supplied the true interpretation upon this very subject of the imputed absorption of bile. "Many physicians," he says, "have been led to suppose that bile

(1) On the Chemistry of the Blood, in Trans. of the Provin. Med. and Surg. Association, 1835, vol. iii. p. 71.

Since the foregoing was written, we find the following summary statement in the British and Foreign Med. Rev. (Oct. 1838, p. 446): "A third class of pathologists have admitted, *a mezzo termine*, that the blood (in jaundice,) receives only the colouring principles of the bile. This last opinion appears to be correct."

We may reasonably employ this well ascertained fact, in the way of analogy, as nearly conclusive in itself that we are right in our induction from other facts, that there is no absorption of the characteristic constituents of the urine, and that the supposed detection of urea and sugar in the blood is probably a fallacy.

It would appear, therefore, that what Assalini, Saunders, Mascagni, Tiedemann and Gmelin, and Sœmering, considered bile in the lymphatics proceeding from the liver, (*a*) when the ducts had been obstructed, was nothing but the lymph tinged with the colouring matter of the bile. Indeed, Tiedmann and Gmelin, in the ten experiments which they made of tying the gall ducts, profess only to have found the colouring matter.

But, admitting, that Boudet, (*b*) and perhaps, others, have found a trace of cholesterin in the serum; this would only show that it is now and then very sparingly absorbed from the liver, and thus existing in a free state, it would contribute nothing to the advancement of humoralism.

(*a*) See Weiber, in Hildebrant's Anat. t. iii. p. 193.
(*b*) Essai. Crit. et Expériment. sur le Sang. 1833.

is constantly contained in the blood-vessels, *misled* by that *very superficial cause of delusion*, viz. the serum being of a yellowish colour." He then shows that the yellowest serum does not contain the smallest quantity of bile.(1) And still later, as chemistry went on confirming the past, and philosophy still drew its inductions from more ample and better sources, Bichat expressed his confident belief that it was not the "bile, urine, or the semen, themselves that are absorbed into the circulation, but only their most delicate parts, some of their principles which we do not exactly know,—probably the serous and lymphatic part."(2)

Again, if bile be injected into the cellular membrane, it produces inflammation and suppuration, and we have already shown its rapid and pernicious effect when made to circulate with the blood. We have also shown the same of urine, so that, in the language of Bichat, "we know that urine effused is not absorbed, and that it destroys every part it touches;"—and yet, by the adaptation of the mucous membrane of the bladder to its properties, nature has supplied a remarkable exemplification of the peculiar modifications of the vital powers in different and even apparently similar tissues. How, then, can it be entertained that those destructive agents can be absorbed with impunity; especially to those absorbing vessels where they must exist in a state of concentration? It is admitted even by Dr. Stokes, that the most formidable symptoms attendant upon jaundice,—the coma, especially,—is not owing "to the action of the bile on the blood," and that "we must seek for some better reason."

(1) On Digestion, p. 65.

(2) General Anatomy, &c., vol. iii. p. 94, and vol. ii p. 68.

This is what Adelon calls Nature's revision of the materials of decomposition before they are expelled from the body.

The editor of Mr. Hunter's lectures, apparently adopting the opinion of the able Blumenbach, states that "it is not improbable that the ferocity of animals during the rut, as well as man's superior courage, greatly depend upon the reabsorption of secreted semen into the circulation." (a)

There appears to us to be, in the foregoing paragraph, three errors in physiology. The first, which supposes the absorption of the specific constituents of semen; the second, which implies, by the word "reabsorption," that this unique compound exists already formed in the blood, though in this construction we may be mistaken; but, thirdly, the greatest error consists in imputing to absorbed semen what is due to the vital influences of the genital organs upon the whole system. That "courage" and that "ferocity," of which our author speaks, are due to the same laws that determine the passion of love in *both* sexes, or the secretion of milk in the female sex. Why has not the Eunuch "superior courage," since, especially, he has no drain upon his blood?

(a) Hunter's Lectures on the Principles of Surgery, Lec. 11. *Note.*

SECTION XIII.

We have hitherto endeavoured to show that there has been, throughout, a begging of the question on the part of humoralism; and we have in reserve some important exemplifications of the same nature. We must, also, in the farther prosecution of this subject, continue to refer to illustrious individuals by whom the doctrine of humoralism is mainly sustained; but, in doing this, we once more insist that it is error, and not our meritorious fellow-labourers, at which we aim; nor do we hold ourselves responsible for any fair means by which we may ascertain the truth, and most efficiently serve its cause.

The present section is but little more than a continuation of the last; and, as in that, so shall we begin this by a farther consideration of the special influences of food, and its privation, upon the state of the blood, and in the production of disease. Doubtless, our own friends are already weary; but, we protest against the complaint of prolixity from the humoralists. At the onset of this investigation we submissively acquiesced in the charge from one author, that "it has been of late the custom to *ridicule* the humoral pathology;" and, from another, that "the modern solidist talks with *contempt* of the contracted views of the humoral pathologist," and that we had therefore undertaken an enterprise of greater seriousness. If we have many facts to analyze, it is because they are individually assumed as a substantial proof of humoralism. If we controvert a palpable error, or indicate a mistake, or dissipate a shadow, it is because they are all mustered under the banner of humoralism. They must be all met, one by one, since each one is in conflict with the laws of nature. But, more than all, we have in view an illustration of some of the most important principles in physiology. We begin, once more, with an example which abounds in the humoral pathology.

"Erysipelas," says Sir Anthony Carlisle, "appears to be a humoral disease arising from vitiated blood. Its common source is improper diet, through which the blood is supplied with crude or noxious materials." He then goes on immediately to say, that "those violent eruptions, called surfeits, are the immediate consequences of one pernicious meal, and they frequently display

erysipelas in its most malignant character." This we believe to be the universal opinion of humoralists. We will not argue the question as to the remote causes of those cases of erysipelas now under consideration, but take the statement as it stands, since there can be no doubt that when the system is predisposed by other agents, undigested food may be an immediate exciting cause of the explosion. But, it is palpable, in the first place, that in these cases the alleged vitiation of the blood by the food can have no existence, since every one knows, that on giving an emetic, the food often comes up in the state in which it went down. If it have undergone some chemical fermentation, so much the better for our argument; for, under these circumstances it is plain that the undigested food has only acted as an irritant to the stomach, through which a pernicious influence has been propagated over the system, which is especially manifested in the skin, in consequence of the very strong vital relations that subsist betwixt this organ and the stomach. That this *rationale* is correct is shown by Sir Anthony himself, who refers all the primary symptoms to the digestive organs. Thus, "the eruption," he says, "is *always preceded* by some derangement in the alimentary canal, and, for the most part, by the loss of appetite, nausea, and constipation, followed by the other signs of incipient fever."(1) This, we suppose, will prove satisfactory, especially if we add that there are no facts to warrant the conclusion that common food, with rare exceptions, contains any pernicious principles; or that any may be generated from it in the stomach, which may alter the condition of the blood.(2)

(1) London Med. Gaz. 1828.

(2) Erysipelas being a favourite example in the humoral pathology, perhaps a few more words should be devoted to its consideration.

If we look at other causes that produce this affection, solidism will establish its title still more strongly to what we regard as constitutional varieties, as well as to such as are of a more local nature. Thus, "we have known a lady," says Dr. Johnson, "who has erysipelas of the face whenever strong north-easterly winds set in. Shell-fish and salmon frequently occasion it, and in certain individuals they always do it. Acids produce it, and wounds are its most fruitful causes." It is remarkable, too, that it must be a certain kind of wound; thus showing that it depends upon the manner in which the instruments of action are injured by the direct cause. "A mere prick or scratch is usually followed by cutaneous erysipelas; not so with a deeper wound; a clean cut is less likely to be succeeded by erysipelas than a punctured wound is; a punctured wound is less likely to induce it than a lacerated one." (a) Just so it is with different varieties of food, as they may happen to be less and less susceptible of digestion, or where some idiosyncracy exists, as in relation to fish, the

(a) Lond. Med. Chir. Rev. vol. xxxi. p. 368.

Let us, again, regard the humoral philosophy upon this subject in other analogous cases. M. Andral states, in confirmation of the foregoing humoral doctrine, that "in times of scarcity, the whole population of a country has been known to be reduced to have recourse to the herbs of the field for support, and to live more or less exclusively on that poor sustenance."(1) By referring to page 62 of the same volume, it will be seen that M. Andral had a particular instance in view in the foregoing statement, where "the inhabitants lived for a considerable time upon *grass* alone, the consequence of which was a general anæmia." Now, there is nothing in grass that can contaminate the blood, any more than in the natural food in the foregoing case. But, a substance so perfectly indigestible could not fail of inducing disease by its direct irritation of the digestive organs, and death might be more rapid than when all food is withheld. But M. Andral, as we shall show more particularly, is, like many others, a humoralist on one page and a solidist on the other. We mean, of course, in relation to the same specific inquiry.

We shall see that M. Magendie "vitiated the blood" by the most unnatural food to which he subjected the suffering animals. (APPENDIX III.) Let the aliment be in any respect natural, (and it is only then that it is entitled to this appellation,) and the blood and secretions will remain without change. "*At si nutrire non potest, nomen non est alimentum.*"(2) Make it unnatural, and the stomach and digestive organs, and other parts of the system may suffer immediately in their powers and functions, and, as a necessary consequence, the blood will be altered

properties of the food may alter the vital forces of the stomach in that peculiar way which establishes the sympathetic development in the skin.

"Drinking cold water," says Mr. Hunter, "when a person is hot, will produce effects analogous to a surfeit. In a similar case, I have seen inflammation come on in the heart and lungs; but being less able to bear the action than the skin, it proved fatal." (a) The principle is the same in all the constitutional cases. The primary injury is of the vital properties of the stomach. This is propagated by the power of sympathy either to the skin, or to the lungs, &c., according to the susceptibilities of the different parts. If to the lungs, there is simple inflammation; if to the skin, inflammation also. But here, on account of the organization, natural modification of the vital properties, and the peculiar functions of the part, the character of the inflammation may be different from that of the lungs; and, for the same reasons, it may speedily result in urticaria, pustular eruptions, erysipelas attended by effusion, &c.

(1) Patholog. Anat. vol. i. p. 410.

(2) Hippocrates, De Aliment. ver. 43.

(a) Lectures on the Principles of Surgery, Lect. xi.

in its character. But the blood will sustain a greater change from its want of supply; and so far as this circumstance goes, which, indeed, is made the basis of the humoral argument, it has no analogy with the ordinary causes of disease, however it may have been a fruitful source of error in physiology. The secretions will be also varied, primarily, both by the morbid state of the digestive organs and their sympathetic influences upon the whole animal fabric, and by the artificial privation of the natural constituents of the blood; to which must be added that secondary cause consisting in morbid changes induced in the blood by the diseased state of the solids.

The same reasoning is applicable to cases of absolute privation of food, where, from the absence of positive agents, the philosophy of the former case is fully illustrated. In these instances, however, the stomach suffering no mechanical irritation, the patient may perish without any special marks of morbid action. (See APPENDIX on *Abstinence*.) In either case, too, it is perfectly compatible with solidism, to suppose that blood when merely artificially altered by withholding its natural constituents will lead to disease of the solids. It is only extraordinary that it does not, since, in these cases, a great alteration is induced in its composition. The whole proof, therefore, so far as it amounts to anything, is directly in conflict with the humoral doctrine. If the animal perish in consequence of the privation, the cause is analogous to the removal of the blood itself. And we may say, as far as the stomach is concerned in supplying the system with the *pabulum vitæ*, "it is only to be considered as a good cook, who may dress everything to the best advantage, but cannot make the flesh of a starved old cow so nutritious as that of a young well-fed heifer." "At stomachus, vitæ neque principium, neque sedes est; aliquis tamen ab ejus imbecilitate lædi potest. Nam et cibi cordi nocentes non stomacho officiunt, sed cordi per eum noxam infligunt." (1)

It must be considered, also, that imperfect nourishment, or indigestible food, disposes the system, in the foregoing manner, and by increasing the susceptibility of the vital properties, (2) to the pernicious influence of atmospheric and other morbific agents, by which the poorer classes may become the subjects of an epidemic, whilst their better fed neighbours may wholly es-

(1) Aretæus, de Acut. &c. l. ii. c. 3.

(2) A state of the system which is often vaguely called "debility."

cape. We shall have some examples of this nature, though they are not common. It may be, also, illustrated by the following fact: "I have been repeatedly wounded during dissection," says Dr. Marsh, "when bodies were in a state of putrefaction, and yet have not sustained any injury; whereas, at a time of unusual exhaustion and fatigue, an injury so slight as merely to raise the cuticle, *without even an appearance of blood*, was sufficient to give rise to the most alarming constitutional and local symptoms." (1)

But we shall show, that in numerous instances of great and general scarcity, health has been unimpaired, (APPENDIX III.) and whilst epidemics have been but seldom connected with this calamity, they are constantly prevailing in times of plenty. The representations upon this subject are similar to what we have seen affirmed by M. Andral of the accuracy of the popular error, that absorbed bile is a fruitful cause of disease. The true physiology was expounded by Hippocrates. "Est febris genus propter malam diætam privatim his contingit, qui ea utuntur; autem aer autor, et causa existit." (2) It is the same principle which led to the early remark by Galen, (3) Avicenna, (4) Fuchius, (5) Crato, (6) Jacchinus, (7) Mercurialis, (8) &c. that if violent exercise be taken when the stomach is full, the lesion of the vital forces is greater and more immediate.

We have seen, what was certainly obvious enough to the solidist, how the blood may become "impoverished," in the language of M. Andral, by defective nourishment. But, this after all, is, in itself, in no respect analogous to diseased blood, nor is the consequent wasting of the system from this cause alone, a state of disease. In these respects, therefore, humoralism has violated philosophy in the parallel which it has instituted. And, in other cases, where disease may be consequent upon a defective supply of material, and a modified condition of the blood ultimately results, we shall endeavour to show that this altered state of the blood probably does not increase the derangement of the solids, however it may contribute to the vitiated state of the secretions. An important law is concerned in this case, which we reserve for our ultimate remarks upon the great causes of epidemic fevers.

(1) Dublin Hospital Reports, vol. iv. p. 504. (2) Lib. de Flat. ver. 73.
(3) Path. l. i. c. 16. (4) Tract. de Disposit. &c. l. iv. fen. 7. tr. 4.
(5) Institut. Med. l. ii. s. 2. c. 4. (6) Concil. 21. l. 2.
(7) Paraphrasin in Rhazis, 9. (8) De Art. Gymnast. *passim*.

If there have been a simply "impoverished" state of the blood from defective nourishment, and a consequent state of emaciation without disease, light, nutritious food may speedily restore the proper constituents of the blood, and rebuild the fabric. But, if disease have been a consecutive result, there can be no restoration of the blood or solids, till that disease shall have been more or less subdued; nor is it probable, could the blood be improved without this antecedent change, that it would contribute either to the restoration of the natural actions or the reproduction of the solids. This conclusion grows out of the law to which we alluded in the foregoing paragraph. The removal of any morbid condition of the solids, however, is often best effected, in these cases, by a simple removal of the exciting cause; that is to say, by the substitution of nutritious for inadequate or irritating food, or for starvation. Nature is not only left unencumbered by the removal of irritating food, but in all the cases the stomach again enjoys its natural stimulus; and, as its functions are restored so does it extend its genial influence over the system, and progressively places the vital forces in harmony with the new constituents that may be added to the blood. Long before the food is converted into blood, all the phenomena of life,—the reanimated and glowing countenance; the rising force of the general circulation; the delightful sensations of the "impoverished" subject,—developed as soon as the food is swallowed, assure us that the great work of renovation has already begun in every part of the system; nor does it require but the superficial glance of philosophy to follow this process, step by step, and to be conducted along the increasing labyrinth, without once turning to the right or to the left to follow the light of a deceitful ignis fatuus. Now the genial restorative enters the portals of the circulation, and whilst yet in the form of chyle, the whole principle of its ultimate operation becomes progressively developed in the increasing signs of restoration. Just in the ratio of these improving changes in the solids will be the improvement, in all respects, in the blood, and the capacity of the system to be nourished. But every advantageous change must begin in the solids. If disease have actually supervened, the vital properties must be first prepared, before any advantage can be derived from an attempted improvement of the blood. In those cases, a morbid change has been induced in the blood by the morbific action of the solids, and they are now in harmony with each other. Alter that relation

by an antecedent improvement of the blood, either as to its "impoverished" or its morbid state, were it practicable, it would at once aggravate the morbid condition of the solids, and, of course, induce an immediate reaction upon the blood.

We may suppose parallel examples. Where there has been a simply enfeebled state from excessive loss of blood, if transfusion of human blood be immediately performed, doubtless it would be greatly advantageous. Wait, however, till a high degree of irritability, or a state of disease, has sprung up, the transfusion would at once aggravate the existing derangement of the solids. So, also, during the incipient change, its progress may not only be arrested, but the system restored, by the gradual introduction of nutriment, and even by the aid of tonics. The whole of this, however, is pure solidism,—deriving its support from humoral facts.

After convalescence from acute, or from long continued diseases, especially those of a congestive nature, and when the various organs are capable of their healthy functions under a proper regimen, the vital properties may remain in so susceptible a state, that animal food may impart such an amount of stimulating principles to the blood, that this fluid, now transcending its harmonious relation to the *vires vitæ* of the solids, may occasion another explosion of disease. The solids, though not actually diseased, were in a state approximating disease, —at least strongly predisposed. And yet there is much reason to believe, that there is often in these cases some latent disorder still remaining. The blood, therefore, does not generate the predisposition to disease. This is an event, as we believe, of frequent occurrence, though relapses are probably oftener consequent on too much, or an irritating quality of food. Still, the phenomenon belongs wholly to solidism, though it serves no more, than in the cases of privation, to illustrate the philosophy of morbific agents. The blood has been altered through the natural means of sustenance. But solidism has its usual interest in the process through which the change has been established, since the whole conversion of the food into blood, and the entire endowments of the blood, have been the result of the vital actions of the solids. This case is parallel, also, with that in which transfusion of blood would develope disease where the solids are left in an irritable state after excessive hemorrhage. The blood, too, in the foregoing case, is even more perfectly natural, and it is for this reason that it has

lost its adaptation to the solids. If it afterwards become diseased, "vitiated," or "impoverished," it will be in virtue of the morbid action of the solids. It is, throughout, a phenomenon of life. As morbid action advances, the "rich blood" loses its richness, becomes diseased; and thus approximating the morbid condition of the solids, acquiring a better adaptation to the state of their vital forces, it also becomes less injurious. And yet, strangely enough, these very cases are produced by Andral and others as a proof of the philosophy of humoralism.

We maintain, however, that the subject derives no illustration from sources which respect the material from which the blood is formed. In regard to the last mentioned case, the animal food bears the same relation to the susceptible vital forces, though in a diminished degree, that it does in idiopathic fever or pneumonia. It is also true, beyond doubt, that in the case of the valetudinarian, as well as in the supposed diseases, that animal food likewise exerts its injurious effects directly upon the stomach, and upon the lacteals during its progress through those vessels in a state of concentration. Here is an instance which may be compared to the action of morbific agents, and is closely parallel with the effect of diffusible stimulants under the same circumstances. It is, also, only in cases of the foregoing nature, that the *properties* of natural food can be morbific. Whilst the vital forces enjoy their integrity, no species of suitable food can develope disease by its own qualities. It may irritate, and produce disease mechanically in excessive quantities; it may undergo fermentation, by its variety; but a new substance, not alimentary, is then the result. All the phenomena, however, which may spring up in consequence, depend on irritation of the solids.

What would be the effect of confinement to any simple substance, as arrow-root for example, upon a healthy system accustomed to the varieties of healthy food? Actual disease may set in, and in different ways, of which solidism can alone afford an interpretation. In the first place, it may begin in the chylopoietic organs. Deprived of their natural and wonted stimuli, they might suffer in their powers and functions; and then would follow all the other lesions which we have already described. Or, secondly, there shall be no primary disease of these organs; but the blood failing to receive its natural elements, and the sanguiferous and other organs thus deprived of their natural stimulus, disease may either spring up in this manner, or the

body become simply emaciated from want of sustenance. Or, thirdly, what is more common than direct disease from this degree of privation, the vital properties become susceptible to the morbific action of other causes, and what is innoxious in a state of health may develope disease in a system thus predisposed.

Emaciation would be also hastened by the development of a law of the system which is intended to operate when the means of nutrition fail, and by which the solids themselves are taken into the circulation to compensate, in a measure, for the want of aliment. And here we have another proof, that in anæmia the changes in the blood, so far as they do not consist in a simple failure of the supply of material, are due to the solids. In consequence of the instrumentality of the vascular system of which we have just spoken, we have it on the authority of great experience, that the quantity of lymph in the blood is actually increased during the first eight or ten days of starvation. (1)

The whole of the foregoing changes, so far as they relate to the solids, depend upon a principle which may be shown by the initiatory step in the process of starvation. Thus, "the general weakness," says Bichat, "which takes place almost instantaneously in hunger, is sympathetic; the alteration of nutrition has not had time to produce it." (2)

Whilst speaking on the subject of anæmia, so important in the humoral pathology, we will present a fact as stated by an able defender of the doctrine: "Anæmia," he says, "in most cases is preceded by a long train of phenomena, indicating a gradual and progressive impairment of health; and whatever its causes or the manner of its production, except when it arises directly from the abstraction of aliment, the nervous system seems to be directly concerned in its evolution. (3)

Here, then, is all that can be desired; and were such admissions allowed their usual force as on other questions, we had spared ourselves and others the trouble of this essay. We shall find it so in relation to every disease of humoral reputation that we may investigate. Sooner or later the solids are brought forward to account for this "evolution;" and the first step in the description of symptoms begins with the solids; and we hear

(1) Magendie's Journ. de Physiologie, t. 8. p. 152.
(2) General Anatomy, &c. vol. 3. p. 124.
(3) Dr. Geddings, in Baltimore Med. and Surg. Journ. and Rev. No. 4, p. 355.

nothing of the state of the blood till disease is far advanced, and, generally, not till the patient is near the verge of the grave.

Whenever the blood becomes diseased by the action of the solids, it is, of course rendered more or less unfit for the purposes of nutrition. But nutrition certainly fails as much from the morbid state of the solids. Not a little has been uselessly written to prove the former obvious fact. It has, however, served the purpose of diverting attention from the primary causes, and of insinuating the conclusion that the deteriorated blood has been the foundation of the mischief.

We naturally pass to those cases in which the system suffers, in a more direct manner, from the excessive loss of blood. "When the system loses a large quantity of blood in a short space of time," M. Andral supposes that the "singular derangements" which supervene are owing to the "blood being *too poor* in quality and *too scanty* in quantity."(1) Take from a man in health two pounds of blood, and let him afterwards subsist on the most nutritious food, he may yet fail, and suffer all the "singular derangements" to which our author refers. It is manifest, then, that the cause has been mistaken. We have already sufficiently shown, in our essay on bloodletting, that every change, in the foregoing cases, is owing directly to an alteration of the vital properties and actions; that every change in the blood is owing primarily to that alteration, and to that cause must be primarily ascribed all the "singular derangements." These cases are very properly regarded by the humoralists as analogous to those in which "grass, and other crude indigestible food" derange the functions of the body. But, if the "watery state of the blood"(2) be the cause of the phenomena in the cases supposed, how, again, shall we explain the rapid convalescence and restoration of flesh in other instances, where profuse spontaneous hemorrhages, or where bloodletting is practised to an almost incredible extent, and in subjects already greatly enfeebled by local disease and protracted abstinence? (pp. 292, 342.)(3) Is there not a direct violence inflicted, in the first of the foregoing instances, upon the *vires vitæ*, where they existed in their normal state; and a salutary impression where they were diseased? Nutrition may be afterwards imperfectly performed in consequence, especially in the former instance; and being so, it was an act of suicide in

(1) Patholog. Anat. vol. i. p. 61. (2) M. Andral, ibid. p. 396.
(3) See Essay on Bloodletting, Sections XI. and XIII.

humoralism to have produced the example. Sensibility, irritability, sympathy, &c., may be suddenly and greatly increased, or otherwise altered, by the direct effect of loss of blood, and, of course, there follows simultaneously a corresponding change in the associated functions. Shall we then argue, with M. Andral and others, under these circumstances, that "if we produce an irritation of the skin or any other part, the result of the irritation is determined by the state of the blood; since there is but little appearance of redness, but a rapid accumulation of serum in the adjacent cellular tissue?"(1)

In the first place, in the cases to which the foregoing quotation refers, the vital properties are peculiarly altered by the nature of the primary cause; and, again, if we "produce an irritation of the skin," we certainly do not irritate the blood; but if the solids, the irritation must consist in some affection of their properties, and, of course, their functions. Here, then, we have positive agents, brought into action by the irritating cause; and, as the blood is the ultimate object acted upon in this instance, do not the "serous effusions," and all the other phenomena, depend upon the action induced by the irritating cause? The same reasoning may be extended to every other part where irritations may arise.(2) How various, also, may be the effects of a simple irritation of the skin in these cases of great excitability of the solids. A puncture by a needle shall not present the isolated example of serous effusion, but only a slight mark of inflammation that shall develope extensive commotions of the system; and, perhaps, the most certain and speedy relief to the constitution will be a small production of purulent matter in the affected part. Will humoralism now assume that this matter had caused the general phenomena, and that in virtue of this puncture it had made its escape from the orifice, thereby relieving the system of the morbific cause? Nor is this inquiry impertinent, for the affirmation has been actually made.(3)

In all the foregoing cases, whether the blood be "impoverished" by defect of nourishment and the concurring causes already mentioned, or by excessive hemorrhage, natural or artificial, its "impoverished" state may become *indirectly* an aggravating

(1) M. Andral, ibid. p. 411.

(2) These considerations appear to be worthy of the attention of those who refer dropsical effusions to an "impoverished state of the blood," or to physical laws.

(3) See, also, Sections II. and III.

cause of disease, or an obstacle to the return of the healthy functions. In these conditions, whilst proper food to replenish the blood-vessels, and other means for restoring the natural state of the solids, are withheld, a thousand causes are liable to derange the functions more and more extensively. It is by thus favouring the operation of other, and the truly morbific causes, that "famine is notoriously the precursor of typhus fever,"(1)—an affirmation, however, which, as we shall show, is much too strongly made. But the humoralists overlook the *invisible* agent, and charge its effects to the "impoverished state of the blood." Precisely what is said of famine may be affirmed of fear, sorrow, remorse, fatigue, and many other depressing causes that predispose the system to every mode of disease, especially to "adynamic" fevers and scurvy. It is beyond imagination to assign any other modus operandi in these cases than what is supplied by solidism.

A single fact will show us the connection between famine and epidemics, whilst it goes to prove that famine as a predisposing cause of disease has been overrated. "If Scotland," says the philosopher Moore, "is less subject to *pestilence*, it is more exposed to *famine* than England."(2) This is undeniably true. The *rationale* is not less obvious. Scotland is colder and more mountainous than England. It is, therefore, better ventilated, and the real causes of pestilence are more scantily generated, sooner wafted away, or sooner extinguished by frost, in the former than in the latter country. In times of famine, therefore, the health of Scotland may remain intact. So it is stated by Dr. Rush, that during a desolating fever at Leghorn, "of the *beggars*, who had scarcely any thing to eat, and who slept half naked every night upon hard pavements, *not one* died."(3) It is a full, rather than an empty, stomach, that aids in breeding pestilence. But here it should not be forgotten, that when scarcity prevails, the poor are apt to resort to "grass," and other indigestible, irritating substances. (See APPENDIX III.)

(1) Armstrong's Lectures on Acute and Chronic Diseases, vol. ii. p. 110.

Southwood Smith supposes that the epidemic fever of 1816, which spread over Great Britain and Ireland, was connected with scarcity and privation. (a) This was probably true, to a certain extent; but is it not equally probable had the rich curtailed their food, they would have suffered less?

(2) Moore's Medical Sketches, p. 235, *note*.

(3) Medical Inquiries, &c. vol. iv. p. 181.

(a) Treatise on Fever, p. 312.

Again, of loss of blood. In susceptible constitutions, and in others predisposed to disease where bloodletting is not appropriate, the injury inflicted upon the vital properties by the excessive loss of blood may rapidly increase by its own unaided tendency, or a full development of the most absolute disease may be at once produced. But why is the blood so "impoverished" in cases of excessive hemorrhage, or where one single bloodletting may have been injudiciously applied? We have already answered the question in part; but the subject may be placed in a yet stronger light. Suppose a single bloodletting, as sometimes happens, to produce the results which we are now contemplating. There may remain at the close of the operation nearly the same relative proportions of the constituent parts, however it may be different at other times. It may be mainly the volume only which is diminished. But a manifest disturbance of the system soon supervenes; and if we now abstract blood, as has been done in these cases, it is found to have assumed a more watery aspect. There was a violence done to all the functions of the body, and hence the "poor and impoverished" state of the blood. (See BLOODLETTING, Sec. XI.) And here again we may remark the wise ordination of nature in the deteriorated state of the blood, by which its constitution is so changed as to be rightly adapted to the altered condition of the vital forces; a construction, to be sure, in great hostility with humoralism.

In all the foregoing cases we naturally attempt, as an ultimate result, an improvement of the *pabulum vitæ*. This is affected by food, and all such means as are calculated to overcome the exact conditions of morbid action, and invigorate the tone of the system. But this improvement of the blood is brought about wholly through the improving action of the solids. Not a change occurs without their previous agency.

On the other hand, the "rich blood" of individuals, apparently in a state of health, is assumed by the moderns as a cause of disease.(1) And how is this shown by the humoralist? Not by a single fact. The following, however, is one example of the proof from a work of justly high authority. "Ratier and Belhomme ascertained by numerous experiments, that the blood of individuals in a state of sanguineous plethora was often buffed and

(1) This, also, is an old doctrine. Thus Celsus: "Corpora, quæ repleta sunt, celerrime et senescunt et ægrotant." (a)

(a) L. i. c. 1.

cupped. This circumstance affords an additional proof, if any were wanting, that the general state of the blood may become a primary cause either of inflammatory fever, or of local inflammation."(1) But has it been shown that such a condition of the blood does become the cause of those diseases? May there not be some begging of the question? Our author goes on to say, that "the same inflammatory appearances are frequently observed also in pregnancy." We will go farther, and affirm that the buffiness is almost always observed in pregnancy after a certain period. But our author does not say, that *here*, also, this same state of the blood may be equally the cause of "inflammatory, or of local inflammation." Our author knows better; and he also knows, that in the latter case those uniform changes are wholly owing to an antecedent and a uniform change in the condition of the solids. May not, therefore, analogy help us a little in regard to the same conditions of the blood in the former cases; and may we not infer, if inflammation, or febrile action, do supervene, in the imputed instances of plethora, it is only a progressive result of a primary, but more obscure, and perhaps neglected, modification of the vital forces,—either of the whole system or of a part? Indeed, the able author just quoted appears, at last, to fall into this conclusion. "The presence of the buffy coat," he says, "may generally be considered a correct indication either of the actual existence of inflammation, or of a strong predisposition to it."(2)

Whilst all parts continue to perform the proper functions, the natural condition of the blood, and the natural balance are maintained. When the latter alone is interrupted, if such interruption can be shown, all experience in relation to the results of food, proves that the plethora must be due to some modified ac-

(1) Cyclopædia of Prac. Med. Lon., Art. Inflam. p. 721.

(2) Ibid. p. 722. "It is believed," says Dr. Craigie, "that the *plethoric habit* is indicated by the tendency to inflammation in various tissues and organs upon very slight causes; by the constant and habitual appearance of the buffy coat in the blood, and, I may add, by certain morbid states as to the quantity and quality in the secretions, but especially the urine." (*a*)

Morgan, in speaking of this "plethoric state of the system," remarks, that "when blood is drawn, it exhibits the buffy coat, &c. Such a state is far from that of health; there is a want of that tone which characterizes the sound condition of the body; the muscular fibre, instead of being firm and rigid, is soft and loose, and the *secretions* are *generally deranged*. In the expressive language of Mr. Hunter, "the individual is living above par." (*b*)

(*a*) Practice of Physic, p. 403. (*b*) Principles of Surgery, p. 93.

tion of the solids. This may even consist in some derangement of one of the emunctories. By a process of sympathetic influences, more or less slow, the vital properties throughout the system may acquire a highly susceptible condition; and, in this state, if there really exist an excess of blood, there can be no doubt that this fluid, from *mechanical* distension, or having now lost its constitutional relation to the altered properties of the solids through a stimulating diet, may accelerate the progress of disease. Hence, it was said by Hoffman, "jam vero nihil æque sit valetudini infensum, quam superabundans sanguis."(1) Hippocrates, and other ancients, have the same precept; but, more correctly do they ascribe the majority of relapses to errors in respect to the quality of food. They had no spasm, or other hypothesis, in view. But, if nutritive food be withheld, a change may be soon wrought in the blood by the action of the solids, and by the diminished supply, which will adapt it to the existing condition of the vital forces. But this, as we have shown, (and allowing the facts in their very exaggerated extent,) is the ground of the solidist, and has no relation to the humoral pathology. Since, however, experiments are a favourite expedient, here is one by Bichat, which embraces the whole philosophy of the subject. "You may double," he says, "by transfusion, the mass of blood in an animal, and local inflammation will not arise, because there must be a *preliminary irritation before the blood flows towards, and enters a particular part of the capillary system.* (2)

After the system becomes predisposed to disease by malarious, or other morbific causes, the blood in virtue of the stimulating properties acquired from animal food, &c., may act as an exciting cause of disease, along with the direct effect of food upon the stomach, and, in conjunction, perhaps, with many other causes, may develope an attack. But it will be perceived, in such instances, that the true morbific agent is very foreign from the blood. This fluid, indeed, is now supposed to be in a healthy state, till the action of the solids is altered; and it is in virtue of the want of adaptation betwixt the healthy blood and the modified state of the vital forces of the solids that this fluid operates upon the exalted irritability of "plethoric" subjects.(3) Thus,

(1) De Mag. Venæ Sec. &c. t. iii. 256. (2) Gen. Anat. &c. vol. ii. p. 40.

(3) Here are more of the facts: "Subjects most likely to be attacked by the *Endemial Causes*," says Moseley, "are the *florid*, the *gross*, the *plethoric*." "Were such,

it will be seen that the blood, even in these cases, (and they are among the strongest examples of humoralism,) is utterly guiltless of the imputed offence, if it have the agency which we are disposed to admit and to believe. But, as we have said, when the latent alteration of the vital forces, or, rather, the morbific impression which they have sustained from foreign agents, is developed into active disease, the tendency of the morbid functions is to change the blood in such a way as to bring it into harmony with the vital forces, and thus to demonstrate, by the very ground-work of humoralism, the error of the doctrine. It is, indeed, one of the objects of art to establish such a change in the blood through the medium of food. In severe fevers and inflammations we restrict the patient, partly for the foregoing purpose, to farinaceous fluids, and allow no animal broth. "Nihil enim ars medica opus erat ægrotis hominibus, si eadem dieta uterentur, et assumerent, quæ sani edunt ac bibunt. Nunc autem ipsa necessitas homines exegit medicinam inquirere ac invenire. Quoniam ægrotis eadem in cibo assumientibus, quæ sani solent, non fuit utile, velut neque nunc utile existit."(1) And we may extend the same principle to those epidemics in which bloodletting and abstinence may be necessary, and affirm upon the broad ground of experience, that he will enjoy the best chances of escape, who renounces a stimulant diet whilst his system may be only in a state of morbid predisposition. It was upon this ground, that the beggars in Italy, whom we have mentioned, (p. 620,) escaped a fatal epidemic to a man; why Audubon, and his party, enjoyed the fulness of health in the jungles of Flori-

just arrived in the West Indies, to expose themselves to the causes mentioned, the probable consequences would be, that *to-morrow* he would perceive heaviness, a lassitude, an oppression, and a loss of appetite. This is the time to extinguish the disease; but Europeans and North Americans neglect it." "The following day the violence of the disease will commence thus," (*a*) &c. And so Jackson, Annesley, and others. We ask again, if the blood of such subjects be more liable to contamination than that of others?

And here is another commentary to the same effect, by Dr. Stevens. Climate fever "is *confined* to the whites, and almost entirely to those who have lately arrived from northern countries." (*b*) Why is the blood of the negroes and the white residents exempted?

M. Blond abounds with facts as to the different susceptibilities of different races to disease, and how these susceptibilities are modified by constitutional causes in various tropical climates. (*c*)

(1) Hippocrates, Aph. l. 2, 5, and 6.

(*a*) Trop. Diseases, pp. 434, 435. (*b*) Ut Cit. p. 198.
(*c*) Sur les Maladies Tropiques, &c. 1805.

da,(1) and this is one reason why moderate bloodletting protected the soldiers at St. Domingo, and, in other instances, which we shall mention, against the invasion of fever. (See Sect. XIV.)

Hippocrates says, what all should know as a general principle, that bloodletting is of little, often of no use, unless a spare diet be simultaneously employed. And Jacchinus truly affirms, that the blood will otherwise "grow *blacker* than it was at first."(2) Still, it is important, in estimating the influences of bloodletting, to distinguish accurately between the absence and the actual presence of disease, and especially the precise effects of this agent upon the *vires vitæ* in all the cases. Where disease is present, it may so increase their susceptibility, as to render stimulating food more pernicious than before its application. And so it may if injuriously practised in a state of health. (See pp. 240, 270.)

The whole of this subject may be farther illustrated by the different influences of morbific agents upon what are called peculiarities of constitution. This we shall do by a quotation from Mr. Travers, where the supposed exciting cause can have no primary action upon the blood. "We say, such a person would be a bad subject for a compound fracture; and whoever has had opportunities of watching several subjects of compound fractures under treatment at one and the same time, well knows the import of this phrase, and that the greatest degree of mischief is often accompanied by the least constitutional disturbance; and for this reason is soonest and most perfectly restored. The first few hours will enable an experienced observer to determine whether the subject of a serious injury or operation will do well or otherwise. How vastly different in different individuals is the inconvenience attending such minor derangements as a bile, an enlarged gland, a whitlow, or a simple inflammation of the eye? In some, the constitution seems ignorant of the affair, (3)

(1) See his Ornithology. See, also, our APPENDIX III.

White, an able observer, was invaluable to the inhabitants of Georgia, by enforcing "a spare diet" upon the healthy during their epidemic fevers. (See New-York Med. Repos. vol. IX. p. 150.) Moseley and Jackson insist upon its importance. Our distinguished Miller states the following "aphorism as a maxim of universal application. *When symptoms, denoting the approach of acute diseases, are discovered, abstain, for a proper length of time, from all aliment.*"—N.Y. Med. Repository, vol. i. p. 197.

(2) Paraphrasin in Rhazis, 9.

(3) This is an unfortunate, though in a figurative sense, a correct expression. Mr. Hunter uses it in the same connection. The materialists, from want of better facts, have taken it up, and pervert its intended import.

and the individual pursues his ordinary avocations. In others, the whole system sympathizes, the spirits are ruffled, the nights are restless, the appetite fails, the pulse acquires an undue bound, and the white tongue, the creeping chilliness, and the slight erratic pains of a sympathetic fever are present." (1)

Finally, we are willing to leave the whole of the foregoing questions as they relate to the dependence of the secretions upon vital actions, the primary independence, at least, of their morbid conditions of any deterioration of the blood; and, lastly, the great question as it respects the humoral origin of disease,—we are willing to submit the whole of this matter to the deliberate judgment of the humoralists themselves. We will first hear the testimony of the modern founder of the humoral pathology upon principles from which there can be no departure.(2)

We will next submit the subject to two eminent reformers, whose apparent admissions in behalf of humoralism have been

(1) On Constitutional Irritation.

(2) We allude to great vital laws, not to the accidents of disease. Mr. Clendening asks, "How should there be a rule in physiology, morbid or healthy, without exceptions?" (*a*) In his acceptation of a *rule*, as illustrated by his subject, there is none; and we think it might be shown, in such instances, that the term is objectionable. Such general rules are subject to great instability from accidental causes, and they have no immediate connection with any of the laws of nature. But, in respect to the great principles in physiology, we know of no exceptions in the natural state of the body; and this is, *prima facie*, a strong ground for analogical inference, as to the principles upon which morbid conditions depend. (See APPENDIX on *Analogy*, vol. ii.) Sooner or later, the apparent exceptions are brought within the general law. Thus, it has been supposed that the circulation of the brain has a law peculiar to itself, and so, also, another for the penis. But one of the conclusions is purely hypothetical, and the other is based upon the microscope, and has been shown to be an error.

Again, on the other hand, it is supposed by Dr. Stokes that inflammation is the usual cause of obliteration or dilatation of the air cells, but that these conditions may grow out of other causes, and they will then "form an exception to a general rule." (*b*) But here no principle is involved, though we see not how the exceptions, as is often said, "increase the force, or importance, of the rule." Precisely parallel, in principle, is M. Louis's statement, founded upon "numerical" experience, which is here useful, that corsets are not an exciting cause of phthisis. (*c*) This is the "general rule;" whilst the observations of all others are the "exceptions;" although M. Louis, not recognising the legitimate nature of such imperfect rules, in this particular case is disposed to consider the exceptions mere "assertions." So our author, with Laennec, (*d*) and Weatherhead, (*e*) and some others, who reject the inflammatory nature of tubercle, affirm that *colds* are rarely an exciting cause of pulmonary consump-

(*a*) Crounean Lectures, *Morbis Cordis*, 1838. (*b*) On Diseases of the Chest, p. 89.
(*c*) Researches on Phthisis, p. 445. (*d*) On Diseases of the Chest, p. 300.
(*e*) On Diseases of the Lungs, p. 41.

appropriated as a sufficient proof of the philosophy of the doctrine.

We shall begin our inquiry with the causes of morbid secretions, and take for an illustration a product which, more than all others, figures in the humoral pathology, and is ascribed, at this day, as we have stated in our last note, to the most contradictory causes. We need not say that this is pus; which enjoys the reputation of being considered the secret parent of many diseases, that only yield when the blood is depurated by casting off this "humour."

We proceed, then, to call up M. Andral. What, sir, have you to say, as a philosopher, touching this matter of the secretions? what say you as to the nature of pus, about which there is so much controversy, and the principles upon which its formation depends?

Ans. "It is interesting to observe the different circumstances which contribute to modify the physical properties of pus. In many cases, an alteration, so slight as to be scarcely perceptible in the process of irritation of a suppurating sore, is sufficient to convert its thick white pus into a reddish sanies, or to reconvert the ichorous discharge into what is termed laudable pus. In order to obtain these results, it is in some cases neces-

tion. This is the "general rule," whilst the united observation of the rest of mankind to the contrary forms the "exceptions." Such general rules, and such exceptions, are perfectly compatible, since they all depend upon accidental causes, or are designed for some special hypothesis.

But, again, on the other hand, when we are told that purulent matter is equally the result of a vital process and of the putrefaction of blood or of the solids, or is at one time generated by the action of the extreme vessels, and at another may form itself spontaneously in the blood; and that ulceration, according to M. Louis, and some others, may depend on inflammation, but again is owing to an "opposite cause;" (a) and that the chemist may form the gastric juice in his laboratory, wherewith he imitates digestion, we consider such "exceptions" to the laws of nature as an indefensible violation of "general rules" which admit of no exception.

The vital powers are certainly distinguished by an instability as it respects their liability to partial modifications; and it is upon this that much of the theory of medicine hinges. But they are always the same powers, affected in the same way, *cæteris paribus*, by the same causes, and leading to the same results according to their modifications respectively. It is the business of observation to ascertain what these changes are, and to record them as perpetual landmarks in the great field of Nature, where they shall always stand, rescuing our science from the hands of mere empyricism, and guiding us under the name of general principles, which are as applicable through all time, to the circumstances of any given modification, as others are to the most perfect integrity of the vital functions.

(a) See our Analysis of M. Louis on Typhoid Fever.

sary to diminish irritation, and in others to increase its activity. We are not, however, to suppose that the qualities of the purulent secretion are affected by these local conditions only; they are likewise modified by every alteration, whether physiological or pathological, which takes place in any other organ, no matter *how far removed* from the seat of suppuration, *even though it has no particular connection either of function or tissue*. Thus, we have all seen instances of the pus secreted by the surface of a sore becoming suddenly altered, both in quantity and quality, under the influence of a *simple moral emotion*, of the process of digestion, of the diminution or increase, whether natural or artificial, of any of the secretions; or, in short, of any supervening disease. Nay, farther; there are certain *constitutions*, certain *idiosyncrasies*, which modify the qualities of pus, and in which it constantly assumes a *peculiar* and *determinate* character. There are some persons, for example, whose organs when irritated never furnish any other secretion than a thin *serous* fluid; in others it is always *blood* more or less pure which is *secreted*; whilst in a third class of persons the place of pus is supplied by a grumous fluid, presenting the appearance of fragments of cheesy matter floating in an albuminous fluid."

Now, sir, what is your unbiassed opinion of the modifications of pus in scorbutic and scrofulous subjects; and must we heal their sores and improve the discharge by remedies addressed to the blood, or by altering the state of the solids?

Ans. "The last mentioned modification of the purulent *secretion* is chiefly observed in scorbutic and scrofulous individuals, in whom all attempts to modify the qualities of the suppuration by local treatment are utterly ineffectual; for it is the *system at large*, and not merely the *suppurating surface*, which is deranged in nutrition and secretion. If, then, we wish to alter the qualities of the pus formed in scrofulous or scorbutic persons, we must commence by endeavouring to modify the whole process of nutrition, innervation, and hæmatosis." (1)

And what do you think, sir, of the nature of the principle upon which the various modifications of pus, the effusion of serum, &c., depend?

Ans. "We do not know what the *peculiar modification* is, which the *texture* of an organ undergoes, so that in one case it allows the blood determined towards it to escape from its vessels;

(1) Mark, here, the order; nutrition first, hæmatosis last.

in another it *forms* pus, or exhales only a thin serum; whilst in a third it becomes indurated, softened, or ulcerated; but there is a *common link* which *unites* these different alterations; and hence it is, under the influence of apparently the *same causes*, we often see them produced indifferently, and not unfrequently replaced one by the other. But, in all this series of phenomena we can perceive, *throughout* the whole course of the irritation *one constant lesion*, namely, the *hyperæmia*, and a *succession* of *morbid alterations* in the *organic action* of the *tissue affected*, *producing*, *alternately*, the results already mentioned." (See a contradictory view at p. 180.)

Do you think, sir, that the phenomena of life depend upon chemical or any physical laws? We ask for no absolute committal upon this dangerous question, which involves so deeply the interests of materialism, and the subversion of Creative Power; but would like the important weight of your opinion.

Ans. "Until it is *proved* that the forces which, in a living body, interrupt the play of the natural chemical affinities, maintain a proper temperature, and preside over the various actions of organic and animal life, are analogous to those admitted by natural philosophy, we shall act consistently with the principles of that science, by giving *distinct* names to these *two kinds of forces*, and *employing ourselves* in calculating the *different laws* they obey."

Upon what, sir, do the symptoms of disease depend?

Ans. "In *every* disease not immediately produced by external violence, the symptoms that occur depend either on a *lesion* of the *forces* that *animate* every living part, or on a *lesion* of *organization*. The *former* is *primary* and *constant;* the latter is secondary, variable in its nature, and inconstant in its existence."(1)

"Are we able to discover the causes that produce those lesions of the vital forces;" and do they act *primarily* upon *them*, or upon the *blood?* This query embraces the gist of our argument, and we ask for an unequivocal reply.

Ans. "The vital forces appear to be affected *primarily* by a great many poisons, by the vegetable or animal emanations, known by the name of miasmata, and by various modifications of the external agents which are incessantly acting upon us, such as want of due exposure to the sun, too damp an atmosphere, and an unwholesome diet."

(1) We commend this to the necroscopical philosophers.

Do you think the solids, when diseased, are likely to exert any change upon the fluids?

Ans. "No one solid can undergo the *slightest* modification, without producing some derangement in the *nature* or *quantity* of the materials destined to form the blood, or to be separated from it."

We are satisfied. This is all sound philosophy, since it is founded on observation; and we suppose that you will agree with us that there cannot be a philosophy exactly opposed to it? Yet will we ask you, whether you will reaffirm what you have just said regarding the vital forces; and whether the vast variety of enumerated causes of disease (which, indeed, embraces the whole,) act *primarily*, as you have just asserted, upon those forces?

Ans. "If an individual breathes an atmosphere loaded with deleterious *miasmata*, or uses *unwholesome* or *insufficient food*, and becomes sick in consequence, *physiology* would lead us to conclude that in such a case the *blood* has been at least the *vehicle* of the *morbific* matter residing in the air or *food*."

Do you think that in the "experiments of injecting into the veins of animals different organic substances in a state of putrefaction, the blood, in such a case, is merely a vehicle to carry to the solids the deleterious substances that inflame them"?

Ans. "Its usual appearance leaves no room to doubt its being really altered in its nature."

Do you think, sir, that we can reason from those preposterous and cruel injections to the *natural* operation of *morbific*, and *curative*, agents?

Ans. "Various animal poisons, such as those of the snake tribe, and different mineral poisons, as mercury, for instance, act upon the blood in the *same manner*." "Those derangements of functions and organs produced by the experimenter, when he introduces different deleterious substances directly into the blood, are *likewise those* that are produced by the *sting* or *bite* of certain animals; they are *also those* that take place in *smallpox*, *measles*, and *scarlatina*, of a malignant nature, as it is called. They are the *same* derangements that appear in persons exposed to putrid emanations, vegetable or animal, and to miasmata from the bodies of other persons that are themselves diseased and crowded in confined places, &c. Lastly, *they* show themselves also in individuals whose blood is only imperfectly or badly repaired by *insufficient* or *unwholesome diet*."

You must believe then, since all the foregoing affections are "the same derangements" which the experimenter produces by putrid injections, that they are all alike in character, and that Magendie, as he affirms, (1) can not only "give rise, at his pleasure, to scurvy, and yellow fever," but that he may "call into being" smallpox, measles, and scarlatina. We will not, however, press this subject; but simply ask for an explicit declaration whether you will sustain your unequivocal statement that morbific agents produce their primary action upon the solids; and if not, then explicitly, whether, the blood be more than a "vehicle"? What is the absolute order in which the changes occur, in all the great diseases under consideration?

Ans. "A *vitiation* of the blood by the commixture of deleterious substances; next, in *consequence* of such vitiation, an alteration of the functions of the nervous system; and, *lastly*, (the blood that supports the organs, and the nervous system that animates them, having suffered a general injury,) a constant, though not always appreciable modification of these organs in *their functions*, or in their texture."

But, there are "diseases resembling many of the preceding, in their symptoms, or in the appearances discovered after death, not unfrequently occurring where *no deleterious substance* has been *introduced* into the blood, and in which there is no direct proof that any alteration of that fluid has been the primary cause of the morbid phenomena;" and admitting all the similitude in their phenomena with the preceding cases that is compatible with the differences which prevail betwixt "miasmatic fevers, smallpox, measles, scarlatina, the bite and sting of certain animals, insufficient or unwholesome diet, and the results of putrid injections into the veins," "what is the conclusion consistent with true logic and sound philosophy" as well as with your original statement, as to the more doubtful cases?

Ans. "Certainly this; that here, as in the preceding cases, it appears that the primary cause of the disease should be *referred* to the *blood*, which, in this case, has altered its nature under the influence of unknown causes, as it has in the others, in consequence of the commixture of various foreign substances."

We have neglected asking whether you still adhere to your truly philosophical doctrine as to the formation of pus and the various modifications it undergoes, and whether neglecting "the

(1) See p. 397.

play of the natural chemical affinities which are interrupted by the vital forces, we should employ ourselves in calculating the different laws which they obey," and whether, therefore, pus can be formed but by the agency of the vital forces and the extreme vessels?

Ans. "It appears that pus is (sometimes) formed in the torrent of the circulation," and "in coagulated blood." (1)

(1) Andral's Pathological Anatomy, vol. i. pp. 298, 54, 422, 423, 400, 401, 406, 407, 305. Vol. ii. p. 337.

"We question," says a distinguished Review, (a) "whether M. Andral's mind is settled on this point, (the origin of tubercle,) and whether his disposition to concede more to the dead, (Laennec,) than to the living, Broussais,) may not bias him." "Such a specimen of reasoning in a circle is not to be found in M. Andral's earlier works." (?)

It seems to us proper, that we should record here, in connection with the foregoing subject, the opinions of this distinguished man in relation to that singular delusion, animal magnetism. Science has a deep interest in ascertaining the mental constitution of those who may hold the sway in her dominions, and how far her lawgivers may be qualified by nature to expound her fundamental elements.

M. Andral, like many of his eminent associates in the walks of pathological anatomy, believes "that a state of ecstacy can be produced by an influence exercised by one individual on another, and that imitation and imagination are inadequate to explain these phenomena." He believes that when thus magnetized, the subject "has been known to have answered various questions and observations proposed to him by one individnal, whilst he remains insensible to the loudest noises, the most exciting remarks, of all the other persons about him. We can not only act upon the magnetized person, but can place him in a complete state of somnambulism, and bring him out of it without his knowledge, out of his sight, at a certain distance, and with doors intervening."

Nor were these convictions the result of any appeal to the senses; for, says our *clair-voyant*, "I have not seen the fact myself, but I have in the course of reading met with several instances of it so well authenticated, that I should not be justified in refusing to believe it." (b) Nor will we neglect saying, that where the understanding is thus surrendered to the imagination, it is important to hold it, on all questions, in more jealous suspicion than where opinions emanate from sources that are incapable of propagating error.

Strange as is the superstition of animal magnetism, it has, nevertheless, awakened the infidel to an approximation to the most momentous truths, where the *light of physiology* had failed.

Thus, M. Georget, in his dying declaration: "In my Physiology of the Nervous System, I boldly professed materialism; but scarcely was it given to the world, when new meditations on that extraordinary phenomenon, somnambulism, no longer permitted me to doubt of the existence in us, and without us, of an intelligent principle, altogether different from material existences. Let it be, if you will, the soul, or God. I have, in regard to this, a profound conviction, founded on facts which I believe to be incontestible. This declaration will not see the light until there can be no longer any doubt of my sincerity, or any suspicion of my intentions." And how many modern systems of Physiology are imbued with the same doctrines, and placed as text books, in the hands of the rising generation!

(a) British and Foreign Med. Rev. vol. iv. p. 319.

(b) Colquhoun's Inquiry into Animal Magnetism; vol. ii. pp. 137, 286, &c.

We now proceed with our second witness, the great head of solidism, whose obliging admissions in behalf of the humoral pathology form a part of the basis of this doctrine.

After saying, however, that Bichat has, *in no instance*, attempted to justify them *by a single fact*, but that the whole philosophy of his writings is utterly opposed to humoralism, we shall be content to allow any advantage that may be taken, after making also the following quotations:

"Examine," he says, "all the physiological and all the pathological phenomena, and you will see that there is no one which cannot be ultimately referred to some one of the properties of which I have just spoken. The undeniable truth of this assertion brings us to a conclusion not less certain in the treatment of diseases, viz. that every curative method should have for its object the restoration of the altered vital powers to their natural type."

"To what errors have not mankind been led in the employment and denomination of medicines? They created deobstruents, when the theory of obstruction was in fashion, and incisives, when that of the thickening of the humours prevailed. The expressions of diluents and attenuants were common before this period. When it was necessary to blunt the acrid particles, they created inviscants, incrassants, &c. Those who saw in diseases only a relaxation or tension of the fibres, the *laxum* and *strictum* as they called it, employed astringents and relaxants. Refrigerants and heating remedies were brought into use by those who had a special regard in diseases to an excess or a deficiency of caloric. The same identical remedies have been employed under different names according to the manner in which they were supposed to act. Deobstruent in one case, relaxant in another, refrigerant in another, the same medicine has been employed with all these opposite views; so true is it that the mind of man gropes in the dark, when it is guided only by the wildness of opinion."

And then our author is led to apostrophize: "Hence the vagueness and uncertainty our science presents at this day. An incoherent assemblage of incoherent opinions, it is, perhaps, of all the physiological sciences, that which best shows the caprice of the human mind. What do I say? It is not a science for a methodical mind. It is a shapeless assemblage of inaccurate ideas, of observations often puerile, of deceptive remedies, and of

formulæ, as fantastically conceived, as they are tediously arranged." (1)

Bichat affirms of the great class of inflammations, that "it is evident that the solids perform the first part in inflammations, and the fluids only the second;" and as to the highest examples in humoral pathology, he says, that "in putrid fevers the blood has a tendency to be decomposed, to become putrid. The index of the alteration of this fluid is always the general state of the forces of the solids; these have *first* lost their spring. In *proportion as the forces diminish in the solids*, this tendency is manifested." It is, he says, as in mortification; "there, is *always*, 1st, a desertion of the solids by the vital forces; 2d, putrefaction of the fluids. *The first* is *never a consequence of the second*." (2)

Now we put to the candid and enlightened humoralist, whether we have not shown that his doctrine is the same now as when Bichat wrote, and whether the reformation did not sweep away an absolute evil; whether it did not purify like the Euphrates of old, and bear aloft our noble science as the presiding genius to which all other sciences paid their respectful tribute? And what is the consequence of the new revulsion? Are medical philosophers now held to be the same great lights of science? Are not physiologists plucked on all sides by the chemists? Do they not press hard to aid our work of demolition, and to carry off the remnant of our late renown? Do we not hear a voice from the metropolis of France, that "medicine is now in its infancy;" and is not this cry rëechoed from all quarters? The causes are to be sought for in the false philosophy, and the crude speculations, which have grown out of our substitution of the physical laws for the laws of life, of our abandonment of the path of nature to seek for her vital actions in the forces of dead matter, or in the ruins she has spread around her, or in the illusions of the microscope, or in the crucible of the chemist, or in the hope of factitious renown.

The humoralists claim JOHN HUNTER as a disciple! And yet, when he does not answer their purpose, they affirm that "his book on the blood has done as much, if not more evil than any other that has been published during the last hundred years." (3) But let us hear Mr. Hunter on a great principle, and it may ap-

(1) Bichat's General Anatomy, applied to Physiology and Medicine, vol. i. pp. 17, 18. (2) General Anatomy, &c. vol. ii. pp. 28, 29. (3) Dr. Stevens, ut cit.

pear that his humoral concessions, like those of Bichat, were a matter of complaisance, which their exaggerated use no longer allows. "If bloodletting," he says, "be considered in a mechanical light, as simply lessening the quantity of blood, I cannot account for its effects, because the removal of any natural mechanical power can never remove a cause which neither took its rise from, nor is supported by it."(1) But it is clear, also, that Mr. Hunter did not regard the blood as a cause of disease in the humoral sense, since a greater objection, as we have shown, would apply to the humoral philosophy of bloodletting than to the mechanical view. The real amount of Mr. Hunter's humoralism is embraced in the following statement, which no solidist ever denied. "The blood," he says, "can receive and retain certain extraneous matter capable of destroying the solids by stimulating to action so as to destroy them."(2) This, as we have already said, is fundamental in solidism. The action of "extraneous matter" is equally then upon the solids as when introduced into the stomach.

Thus may we learn from a simple statement, which involves any great principle in physiology, the doctrinal views of disease which a consistent mind will entertain. But, as we have seen Mr. Hunter charged with inconsistency on some minor points, it may be well that we should state his opinion, explicitly, of the humoral pathology.

"Among *physical* people," he says, "we find such expressions in common use, as, the *humours are affected in the blood; sharp humours in the blood;* the *whole humours being in a bad state;* the *whole blood must be altered,* or *corrected;* and a variety of such expressions *without meaning*. They even go so far as to have *hereditary humours*, as gout, scrofula, &c. and make us the *parents of our own humours*, saying that *we breed bad humours*. Accidents, or even the application of poultices, have been supposed to bring bad humours to a part, because the part in which the accident happened, or to which the poultice was applied, would not readily heal. Humours are even supposed to gravitate to the legs slowly; and, in short, the *whole theory of disease* has been built upon the supposition of humours in the blood, or of the *blood itself being changed.* I cannot conceive what is meant, unless it be that *strong susceptibility to a specific disease exists;* as, smallpox may bring on

(1) Hunter on the Blood, &c. p. 345. (2) Ibid. p. 99.

scrofula, or a strain the gout." And as to the communication of specific disease by the injection of morbid secretions, &c. Mr. Hunter remarks, that "from his experiments we may draw the probable conclusion, that extraneous matters taken into the constitution do not produce any specific and permanent disease; but, that they operate as direct stimuli, producing immediate effects; and that when these are violent, they arise from too great a quantity of the stimulating or irritating substance being thrown in." (1)

From what we have now seen of Hunter and Bichat, it ceases to be remarkable that they are either denounced, or more generally neglected as obsolete in their doctrines.

We shall notice here an affirmation by M. Andral, since it is regarded by high authorities (2) as nearly closing the door against farther discussion. "Physiology," he says, "leads us to the conclusion that every alteration of the solids must be succeeded by an alteration of the blood, just as every modification of the blood must be succeeded by a modification of the solids. Viewed in this light, there is no longer any meaning in the disputes between the solidists and the humoralists." (3)

This is any thing but a fair statement of the great question at issue. It is not whether the blood becomes diseased by a morbid action of the solids; and the solidist is surprised that the defence of humoralism should often turn upon laboured attempts to prove what every body admits. Nor is it, whether vitiated blood, or putrid matter, will excite disease when injected into the veins. The question at issue is, *whether foreign morbific causes, and remedial agents, in their ordinary modes of operation, produce their primary effect upon the solids or upon the blood, and the latter become the cause of disease in the former; whether we "have hereditary humours, as gout, scrofula, &c.;"* and *whether we are "the parents of our own humours, and that we breed bad humours;"* whether they "gravitate to the legs," or are "brought to a part by poultices;" whether, according to Andral, "those derangements of functions and organs *produced by the experimenter*, when he introduces different deleterious substances into the blood, are *likewise those* that are produced

(1) Hunter's Lectures on the Principles of Surgery, lec. 11.

(2) Cyclopædia of Anatomy and Physiology, Lon., Art. Morbid Conditions of the Blood. p. 416. Cyclopædia of Prac. Med., Lon., Art. Fever, p. 198.

(3) Patholog. Anatomy, vol. i. p. 391.

by the sting or the bite of certain animals, and are also *those* that take place in smallpox, measles, and scarlatina, and are the *same* derangements that appear in persons exposed to putrid emanations, vegetable or animal, and to miasmata from the bodies of other persons that are themselves diseased and crowded in confined places, the *same* which show themselves in individuals whose blood is only imperfectly or badly repaired by *insufficient* or *unwholesome diet*;" whether, in other diseases, "where *no deleterious substance* has been introduced into the blood, and in which there is no direct proof that any alteration of that fluid has been the primary cause of the morbid phenomena," but where the symptoms and morbid appearances may have some resemblance to those of the foregoing affections, "it appears, as in the preceding cases, the *primary* cause of disease should be referred to the blood;" and whether "the whole blood must be altered or corrected" by "incisives, diluents, attenuants, inviscants, incrassants, revulsives, repellents, concoctants, deflectants, derivatives, depuratives, deobstruents, detergents, agglutinants, incarnatives, refrigerants," &c.? These are the questions.

But the most objectionable part of our author's statement, by which he calmly identifies solidism and humoralism, is the assumption that the blood is admitted by the solidists to be "modified" without the agency of the solids, and to become, in consequence, the cause of disease in the latter; or, in the words of our author, "just as every modification of the blood must be succeeded by a modification of the solids." We shall not dwell upon this *coup de main*, since we have just shown the artificial process by which our author can alone justify his ground. Nor shall we now controvert the induction which is made by Dr. Babington, that all beyond the foregoing statement is but little better than "unprofitable speculation."(1) We think we have sufficiently shown, and shall yet add other proof, that humoralism is subversive of physiology, and leads to the most deplorable consequences in practice. But we may again repeat, that, according to one or the other theory, the treatment must be applied, as admitted and maintained by the humoralists, either to the solids or the fluids, and, as avowed, that we must restore the schools of Van Helmont and Sylvius de la Boe, with all their practical fallacies.(2) And need we say what was the result of those specu-

(1) Cyclopædia of Anat. and Physiol. ut citat.

(2) Dr. Holland, in a work published since the foregoing was written, in speak-

lations when carried into the chambers of the sick? Need we confirm the importance of establishing a broad line of distinction between solidism and humoralism by referring to the well authenticated catastrophe, that "two-thirds of the population of the city of Leyden were consigned to an untimely grave by the errors of Sylvius de la Boe; who, having embraced the chemical doctrines of Van Helmont, assigned the origin of the distemper to a prevailing acid, and declared that its cure could alone be effected by the copious administration of absorbent and testaceous medicines?"(1)

M'Bride, yielding to the experiments of Sir J. Pringle, reared the hypothesis that fixed air forms the bond of union upon which cohesion mainly depends. He therefore maintained that the tendency to putridity of the blood in scurvy is owing to the loss of this principle; and that by restoring it, the disease will be removed. Dr. Percival,(2) and Wm. Hey,(3) carried their ideas so far on this subject as to administer enemas of carbonic acid in all "putrid fevers;" being just the antipodes of the saline treatment. This doctrine prevailed for some time in the navy, and finally gave place to the former treatment by acids, &c. Then we have the theory and treatment by Trotter, which is exactly opposed to that of M'Bride.(4)

We have evidences in all directions, that it is the present purpose of humoralism to carry its philosophy into practical effect. We have stated many indications of this nature, and may here add other specific examples. Thus, according to Dr. Bird, it is affirmed by Dr. Clanny, "that there is no certain method of distinguishing the slighter cases of epidemic cholera from severe cases of sporadic English cholera, unless recourse be had to the chemical analysis of the blood. And acting on this opinion, what does the doctor do? Why, instead of attacking the host of alarming symptoms, he draws some blood from the patient's arm, submits it to an elaborate analysis, and then administers his remedies,

ing of the humoral pathology, says we must go back to our ancestors on this subject. (Medical Notes and Reflections. 1839.) We are ready to return to them as it respects their habits of observing nature, and the treatment that was so remarkably founded upon that observation. We refer, of course, to the masters of our art.

(1) Paris's Pharmacologia, Historical Introduction. Percival gives the same account. Essays Med. and Exp. vol. i. Es. 1, p. 30.

(2) Percival's Med. and Experimental Essays.

(3) See Mr. Hey's Letters to Dr. Priestley.

(4) Trotter's Observations on the Scurvy, p. 140.

leaving the patient, perhaps, in the clutches of the grim tyrant, while the process of analysis is going on."(1) Yet who will deny that Clanny acts in conformity with the humoral doctrine? Again, in respect to the propriety of removing diseases of the skin, "the doctrine of the humoral pathology," says Plumbe, "has supported, and still continues to support, a contrary opinion."(2)

This, as we have said, is all philosophical. The whole treatment of disease should turn upon its pathology. It is this which distinguishes the philosopher from the empyric. If humoralism be true, we must look for "the seminal principle of disease in the blood, and to that we must apply our remedies." (p. 397.) This is the only dictate of reason; and the humoralist, who should act upon the principles of solidism, would, so far, not only be no philosopher, but would practically contradict his own theory. If he believe that in "the worst forms of fever the seminal principle of disease consists in a morbid state of the blood," he should then endeavour to restore the blood by "muriate of soda," &c., and should not be guilty of the unphilosophical practice of bloodletting, vesication, &c.

We see, day after day, lamentations over the poverty of animal chemistry; and there is an attempted concealment of its cause by ascribing it to a neglect of the subject. The disappointed would suspend the judgement of the public. But, have not all sorts of chemical philosophers been long and devoutly occupied with this investigation; and is not the wail of the humoralist rather for the want of facts, than the want of labourers? Have not all the existing resources of art been exhausted in the inquiry? Let us hear on this subject one who tolerates no other laws in physiology than such as operate in the laboratory, and yet admits that they are utterly inapplicable to the phenomena of life. What he states to have been done was the work of some quarter of a century past; and we need scarcely add, that the zeal for inquiry has not languished since, or that a single fact in relation to our present subject has not been since developed. We refer to Dr. Bostock, who says that "pathologists have minutely described the different appearances which the blood exhibits in different diseases, and the alteration which takes place in its physical properties; but we have scarcely a single fact on which we

(1) Lon. Med. Gaz. vol. xiii. p. 400. See records of the humoral treatment of this disease in Ibid. vol. x. pp. 717, 453; vol. xxii. p. 319, &c.

(2) Diseases of the Skin, p. 35. 1838.

can rely, that indicates any decided difference in its chemical constitution."(1) We shall have occasion hereafter, as we have already, to quote later eminent authorities to the same effect; many, indeed, as we have formerly shown, rejecting all analyses of the blood as fallacious, and unworthy of credit. It is even conceded by Dr. Stevens, that "when minute experiments are made in animal chemistry, even by the ablest chemists, the results which have been obtained are often diametrically opposite."(2) Yet, upon a most vague observation of the blood, it is not only decided by the humoralists that its composition, &c., is essentially altered in disease, but that this alteration is the first in the series of morbid changes.

"All that we can say is," says Dr. Stokes, "that the blood is altered in its condition; but, what the nature of that alteration is, we know not;" "we know almost nothing of this very interesting subject." "Notwithstanding all that has been written on the subject, there has been, as yet, no valuable *indication*, no *practical improvement*, founded on the state of the blood in fever."(3) How then can any conclusion be drawn as to its primary connection with the phenomena of fever; or what is there to warrant the opinion as expressed by our last author, that "there can be no doubt, that the condition of the blood, *if known*, would furnish a most important link in the chain of phenomena"? In the midst, too, of this admitted absence of all facts, it is the opinion of Mr. Laycock, that "pathological chemistry will soon rival pathological anatomy in importance."(4)

In our various disquisitions upon the powers and actions of life, we have cited numerous authorities who defend the chemical and humoral doctrines, but who at last admit that they are incapable of explaining any of the phenomena of life. We shall introduce, here, another of those candid admissions, in its relation to our present inquiry. "It is not unreasonable to expect," says the learned editor of Mr. Hunter's Lectures, "that modern chemistry will throw considerable light on this subject, (the humoral pathology,) and discover some general principle by which the scattered facts which now exist relating to the blood will be connected and explained. In the present *actual state* of science

(1) Bostock's Physiology, vol. i. c. 6, s. 7, p. 586. See, also, objections by Berzelius, in Ann. Phil. vol. iv. p. 402; and Daniel, in Children's Thenard, p. 358.

(2) On the Blood, &c. p. 20. (3) Theory and Prac. of Med. p. 365.

(4) Lon. Med. Gaz. Sept. 1837, p. 43.

these facts are practically *useless*." (1) And why are they "useless," unless what are put forth as "facts" are nothing more than conjecture? The hope that is expressed in the foregoing quotation is fully admitted not to be encouraged by long experience. It is therefore visionary. Few subjects, "in the present actual state of science," have attracted attention more than animal chemistry, and no efforts have been more fruitless. The reason is obvious. The whole subject belongs to another department of nature.

But, again, of those who are still in expectancy of the chemists. "Until we know," says Prof. Alison, "*something* of the laws according to which chemical affinities are altered in the living body, we cannot expect that the chemical examination of dead animal matter will give much assistance to pathology." (2) The first step in the process should be, to determine how far physiology and pathology sustain the same relation to *dead* matter that they do to *living*. The pathological chemists should wait till this is done by the vitalists, who will then invite their assistance.

We also agree, in a general sense, with another distinguished writer, that "if the results hitherto obtained (by "analysis of the blood in health and disease") are of too vague a nature to allow them to be applied to the improvement of the practice of medicine in the way anticipated by the experimenters, this is rather to be ascribed to the extreme difficulties which beset the investigation, than to any want of zeal and skilfulness on their parts. Ample materials are before us; but these materials require a master-hand to mould them into a proper form. The difference in the results of different chemists must be reconciled," &c. (3)

SECTION XIV.

NOTHING CAN MAKE HEALTHY BLOOD BUT THE HEALTHY ACTION OF THE SOLIDS.

We have shown that if morbific or therapeutical agents enter the circulation, they can only affect the blood by forming chemical

(1) Hunter's Lectures on Surgery, Lec. xi. *Note.* 1838.
(2) Physiology and Pathology, p. 53. 1833.
(3) British and Foreign Med. Rev. vol. vi. p. 431. 1838.

combinations with it, and thus undergo decomposition. But, that no such changes happen, has been shown by the humoralists themselves when endeavouring to prove their absorption by producing the agents unchanged both in the blood and secretions. It is possible, therefore, that this ground may be abandoned, and the hypothesis set up that the morbific and therapeutical agents act upon the *vitality* of the blood. But, here, although we have no doubt of the blood's vitality, the same difficulty arises as in the former case, though we think in an inferior degree. But, since there is a greater agreement amongst philosophers as to the vitality of the blood, than the existence of miasmata, (p. 542,) we are willing to admit that the former is sufficiently proved to justify its assumption as a ground of argument in the humoral pathology. This state of the question also adds to the more direct proof that the *living* blood is not susceptible of the imputed decomposition, fermentation, &c., but that that fluid, like the solids, resists, more or less, the agencies of chemistry. If this position should be taken by the humoralists, they must abandon the "household words," *vitiation, corruption, humours, fermentation*, and, what is worse, the test glass and crucible.

What, then, are the characteristic properties of vitality that appertain to the blood? It certainly shows no sign of irritability, sensibility, or sympathy, though we have no doubt it possesses irritability in a low degree. But, suppose that it has all the vital properties which belong to the solids; they are still so incomparably less developed as to lead many sound minds to a denial of their existence; while the blood itself is designed as a stimulus to the vital properties of the solids. How evident, then, if any foreign agent enter the circulation, it will exert its effect upon the highly susceptible solids. Nor should we forget, that in passing into the circulation, they are in contact, in a concentrated state, with parts endowed with those properties which the humoralist admits are highly susceptible of morbific impressions; (1) whilst he shows nothing in the inert blood which can justify an assumption that the morbific impressions are here rather than upon the susceptible and ever acting solids.

(1) We have a word more to say on the probable vitality of the blood. This conclusion is founded mainly on the blood being the immediate pabulum of the body, from coagulating out of the body, and its supposed capacity of becoming organized when extravasated. The vital powers of the solids are inferred from other various and imposing phenomena. No evidence of a like nature is supplied by the blood. Even its property of coagulating is held, in opposition to Mr. Hunter, and by such

But, although the difficulties we are about to present are as conclusive against any hypothesis that may respect the vitality of the blood, as against the existing humoral doctrine, we will not anticipate an error which is, prima facie, without the means of support; but return to the prevailing hypothesis of "vitiation," "fermentation," &c. We have said that every individual who may be exposed to the chemical influences of an epidemic atmosphere should give some indication of its deleterious action. But the contrary obtains in all epidemics, and, what is much to our purpose, the epidemic influence may be counteracted by causes which can only operate upon the solids. Nor should habit exert

as admit its vitality, to be only a negative proof. (a) "We might as well," says Ferriar, "ascribe the freezing of water to a living power."

It appears to us that a strong proof that the blood is endowed with vitality may be derived from analogy, although the facts mainly relate to those subjects betwixt which and the blood the comparison is instituted. We know, that throughout nature there is a connected series of causes and effects by which she advances from simple results to the more complex. The relation which the blood sustains to the elements of inorganic matter and the living solids of the animal body is a consecutive result of a long series of changes, and this result occupies the highest place in the order of phenomena before the great final cause is consummated. For conducting this connected series of changes we find a complex apparatus, beginning in the vegetable kingdom, and ending in that which is to complete the process in carrying up the simple elements of matter to the highest species of organization. In all this astonishing contrivance, we fancy, at least, the design of nature of gradually endowing the elements of matter with the properties of life, and thus avoiding the abrupt transmutation, at one step, of dead into the most perfect of living matter; which appears to require the direct Fiat of Creative Power. The conversion is a gradual process in the vegetable kingdom; and although the principles of life are extinguished before vegetable food is appropriated by animals, the new combinations remain, and are indispensable. They are thus in a condition to receive a higher impression of vivification during their conversion into blood, than the condition which appertained to the original elements. From this we also argue a higher degree of vitality in the blood than in the sap, and a corresponding difference betwixt the vital properties of the solids in both departments of nature.

But, after all, the vitality of the blood is wholly derived from the solids, directly or indirectly, is constantly subject to their control, and cannot become a part of them but by their actions. And yet, without any known manifestations of any one of the vital properties of the solids, there are eminent philosophers who assign to the blood a self-moving power, and some who believe that it is moved exclusively in the lungs by this inherent property of locomotion,—the right ventricle of the heart performing the office only of transmitting it to those organs. Even organic motion in other parts depends not only on the manifest power of irritability, but on the cooperation of several distinct tissues.

Dr. Philip objects to any relative degrees of the vital powers. (b) But his theory of life is entangled with galvanism and the chemical forces.

(a) See Med. Chir. Trans., Lon., vol. xvi. p. 295. Also, Thackrah's Inquiry into the Nature and Properties of the Blood.

(b) See his Reply to Dr. Prout, in Essay on Sleep and Death.

the least protecting power over the blood; since, in the first place, this construction would be apparently absurd, and, secondly, the blood is hourly undergoing a renewal. Nor can this principle be extended to the solids; for the hypothesis supposes that the solids are exempt, in a direct sense, from impressions of the foreign agents. But we know that the influence of habit is great; that we come, at last, to be insusceptible of epidemic disorders by living in the epidemic atmosphere.(1) And so of medicines. On the other hand, we know that sedentary habits, emotions of mind, &c., render us more liable to the invasion of epidemic and other diseases. Is this susceptibility formed in the blood or in the solids? If in the latter, to which would reason refer the action of the morbific agent? Nor should it be lost sight of, in our present argument, that humoralism necessarily excludes all the contingencies relating to the solids which we have now considered. The moment it regards them as taking an initiatory step, it opens a door for its own expulsion.

Nor can we neglect considering, in another aspect, though coincident with the principles as formerly exemplified, the different influences of causes that are clearly non-humoral upon different constitutions. Grief, and other moral emotions, always produce disease in certain individuals, whilst they never affect others in a like manner. So, the prick of a pin uniformly excites inflammation in one, but never in others, being always erysipelatous in one, and always phlegmonous in another; and so on. Here is the whole philosophy of our subject. Miasmata, in the same way, establish alterations in the vital forces of some, but not in others; whilst, if all be equally exposed, no reason can be assigned why the blood of one should not be as readily deteriorated as that of another, when the solids must necessarily suffer in the whole. The following is a striking exemplification of our doctrine. Of 840 Indians at Nantucket, 258 were attacked by a malignant fever, carrying off 222 of that number; whilst no white person was affected. A like coincidence took place at Martha's

(1) The foregoing fact is probably sufficiently admitted; but in conformity with our custom, we shall refer to individual experience. Johnson (*a*) says it is true of the endemics of the West Indies; and Bancroft, (*b*) Trotter, (*c*) Moseley, (*d*) and others, affirm the same of the yellow fever. The latter says, "the troops should first be sent to the healthful Bermuda Islands, a year, if possible, to acquire a proper seasoning for tropical operations."

(*a*) On the Influence of Tropical Climates, &c. vol. i. p. 206. (*b*) Essay on Yellow Fever, p. 265. (*c*) Med. Naut. vol. i. p. 336. (*d*) On Tropical Diseases, p. 65.

Vineyard.(1) Precisely the same philosophy is applicable to the different effects of remedial agents upon different constitutions, in health and in disease.

If we admit the humoral pathology in relation to miasmatic diseases, how will it dispose of objections like the following? "It was formerly a practice among the physicians of St. Domingo," says Dr. Rush, "to bleed whole regiments of troops as soon as they arrived from France, by which means they were preserved from the malignant fever of that island."(2) So, also, with the Dutch artillery. This plan of prevention was especially practised by Dr. M'Lean.(3) Chisholm, an unbending contagionist, and with all his opposition to bloodletting, says, "as a preventive of yellow remittent fever in strong plethoric constitutions, I am convinced a better remedy cannot be employed."(4) So, also, M. Chevalier;(5) and so Moseley.(6) Mr. Lee remarks, that "the practice at Rome, in the treatment of disease, inclines to the Broussaian. The abstraction of blood in small quantities is resorted to in the majority of cases as a preventive against malaria fever."(7) Rush believes that many thousands escaped the yellow fever at Philadelphia, by abstemious living, or by anticipating an attack by the use of bloodletting, calomel, rhubarb, &c., according to certain admonitory symptoms.(8) White, in Georgia, (1802,) "has frequently prevented an impending attack of fever by a timely bleeding."(9)

There is much more concurring authority upon the foregoing subject; and, in the cases to which the references are made, no doubt is entertained as to the preventive effect of bloodletting. We therefore ask, whether the abstraction of blood secured it from contamination; or whether there be any other rational construction, than that the moderate depletion which was practised lessened the susceptibility of the solids to the direct action of the morbific causes, or subdued a predisposition already formed? The principle is the same as that which relates to a diminution of animal food during the prevalence of epidemic fevers, (p. 624,)

(1) Philos. Trans., 1764, p. 386. (2) Medical Inquiries, &c. vol. iv. p. 181.
(3) Inquiry into the Nature and Causes of the Mortality at St. Domingo.
(4) Essay on the Pestilential Fever of the West Indies, vol. i. p. 345.
(5) Lettres sur les Maladies de St. Dominique, p. 27, &c.
(6) On Tropical Diseases, pp. 444, 435.
(7) On the Medical Institutions of France, Italy, and Germany, p. 61.
(8) Medical Inquiries, &c. vol. iv. pp. 29, 182, 183, 189, &c.
(9) New-York Med. Repos. vol. ix. p. 150.

and confirms the advantage, and our philosophy, of the latter practice.

The foregoing considerations enable us to understand, and to appropriate to our own use, certain coincident examples which are brought forward not only to sustain the humoral pathology, but the treatment which it inculcates. Thus, it is said that "where almost every individual on board a vessel was attacked with the African typhus, the only exceptions were two persons who took a small dose of Cheltenham salts every morning, as a preservative against the poison."(1)

Again, it is obvious that an absolute impression is established upon the vires vitæ of the solids, during the existence of epidemic fevers, before there are manifestations of fever, from facts like the following. Thus, Sir J. M'Grigor found, during the prevalence of an "infectious fever," that those soldiers, who were predisposed to it, would not receive the infection of smallpox, till the fever exploded and went through its course.(2)

We shall now urge some considerations to which we have already referred, that appear to us, independently of the various other proofs which we have alleged against the humoral pathology, to be fatal to the doctrine. These considerations will show us that *the blood is neither a primary cause of disease in the solids, in virtue of its own morbid condition, nor can it be an aggravating cause of disease when altered in its character by the morbid action of the solids.*

None will deny what is affirmed by M. Andral, that every morbid change in the action of the solids is probably followed by a change in the blood; whilst we fully agree with him, that any *primary* alteration of the blood, of a morbid nature, must, with greater certainty, produce disease of the solids.(3) The latter proposition is the basis of humoralism.

There is a specious parallelism about the foregoing propositions of which humoralism has taken no little advantage. It supposes that the blood and the solids sustain, reciprocally, the same relation to each other; when, in truth, the distinction is about as great as between an agent and the object acted upon. There is this difference, however. In the present case, in their natural state, the blood is the object, whilst it contributes to the support

(1) Dr. Stevens on the Blood, &c. p. 315. (2) Wilson on the Blood, &c. p. 232.
(3) Patholog. Anat. vol. i. p. 391.

of the agent; but were the blood to become primarily diseased, it would then assume the same relation to the solids as any other morbific cause, and even more so on account of the foregoing principle, or, in other words, its constituting the *pabulum vitæ*. The relation of the solids to the blood, as an object acted upon, would be then strongly developed; but the distinction betwixt the nature of the agents, and the results of their action, is still less analogous than in the former instance. From the germ to the adult, all the results of organic life have their origin in the organic actions. The nutritive fluid, from the time that organic actions begin, is either directly, or indirectly, the product of those actions; and the only sense in which the blood can be regarded as an agent, is that of stimulating the vital force of the solids so that they shall carry on the work of life, and appropriate the *pabulum* to their own uses.

Here, then, we must steadily regard the true relation of one to the other. The solids, which give being and vitality to the blood, become, in their normal state, it is said, the subject of its morbific action; and, according to the premises of humoralists and solidists, when the solids are diseased, the blood undergoes disease in consequence; and, since the blood was *originally* the cause of the morbid action of the solids, *every increasing degree of disease, according to the admitted premises, must be a cause of increasing disease in the solids.* This must be equally true of local as of constitutional diseases. No portion of the blood can be, long, morbidly affected more than the whole mass; and since, when universally diseased, it should produce one universal disease of the solids, it is manifest from the constant occurrence of local affections that humoralism is striving against the plainest evidence. But, if we allow the paradox, that a diseased state of the blood may produce a local disease of the solids, it will in no respect affect the other fundamental principle which we are now considering.

It follows, therefore, that the solids, in their natural state, having been brought into a morbid condition by a diseased state of the blood, and this condition of the solids perpetually advancing, according to the admitted premises, every disease *so beginning* must necessarily terminate in death. For, again, in the first place, we have shown the absurdity of attempting the restoration of the blood to its normal state by any direct action upon it by foreign agents; and secondly, by the admitted premises, *nothing*

and confirms the advantage, and our philosophy, of the latter practice.

The foregoing considerations enable us to understand, and to appropriate to our own use, certain coincident examples which are brought forward not only to sustain the humoral pathology, but the treatment which it inculcates. Thus, it is said that "where almost every individual on board a vessel was attacked with the African typhus, the only exceptions were two persons who took a small dose of Cheltenham salts every morning, as a preservative against the poison."(1)

Again, it is obvious that an absolute impression is established upon the vires vitæ of the solids, during the existence of epidemic fevers, before there are manifestations of fever, from facts like the following. Thus, Sir J. M'Grigor found, during the prevalence of an "infectious fever," that those soldiers, who were predisposed to it, would not receive the infection of smallpox, till the fever exploded and went through its course.(2)

We shall now urge some considerations to which we have already referred, that appear to us, independently of the various other proofs which we have alleged against the humoral pathology, to be fatal to the doctrine. These considerations will show us that *the blood is neither a primary cause of disease in the solids, in virtue of its own morbid condition, nor can it be an aggravating cause of disease when altered in its character by the morbid action of the solids.*

None will deny what is affirmed by M. Andral, that every morbid change in the action of the solids is probably followed by a change in the blood; whilst we fully agree with him, that any *primary* alteration of the blood, of a morbid nature, must, with greater certainty, produce disease of the solids.(3) The latter proposition is the basis of humoralism.

There is a specious parallelism about the foregoing propositions of which humoralism has taken no little advantage. It supposes that the blood and the solids sustain, reciprocally, the same relation to each other; when, in truth, the distinction is about as great as between an agent and the object acted upon. There is this difference, however. In the present case, in their natural state, the blood is the object, whilst it contributes to the support

(1) Dr. Stevens on the Blood, &c. p. 315. (2) Wilson on the Blood, &c. p. 232.
(3) Patholog. Anat. vol. i. p. 391.

of the agent; but were the blood to become primarily diseased, it would then assume the same relation to the solids as any other morbific cause, and even more so on account of the foregoing principle, or, in other words, its constituting the *pabulum vitæ*. The relation of the solids to the blood, as an object acted upon, would be then strongly developed; but the distinction betwixt the nature of the agents, and the results of their action, is still less analogous than in the former instance. From the germ to the adult, all the results of organic life have their origin in the organic actions. The nutritive fluid, from the time that organic actions begin, is either directly, or indirectly, the product of those actions; and the only sense in which the blood can be regarded as an agent, is that of stimulating the vital force of the solids so that they shall carry on the work of life, and appropriate the *pabulum* to their own uses.

Here, then, we must steadily regard the true relation of one to the other. The solids, which give being and vitality to the blood, become, in their normal state, it is said, the subject of its morbific action; and, according to the premises of humoralists and solidists, when the solids are diseased, the blood undergoes disease in consequence; and, since the blood was *originally* the cause of the morbid action of the solids, *every increasing degree of disease, according to the admitted premises, must be a cause of increasing disease in the solids.* This must be equally true of local as of constitutional diseases. No portion of the blood can be, long, morbidly affected more than the whole mass; and since, when universally diseased, it should produce one universal disease of the solids, it is manifest from the constant occurrence of local affections that humoralism is striving against the plainest evidence. But, if we allow the paradox, that a diseased state of the blood may produce a local disease of the solids, it will in no respect affect the other fundamental principle which we are now considering.

It follows, therefore, that the solids, in their natural state, having been brought into a morbid condition by a diseased state of the blood, and this condition of the solids perpetually advancing, according to the admitted premises, every disease *so beginning* must necessarily terminate in death. For, again, in the first place, we have shown the absurdity of attempting the restoration of the blood to its normal state by any direct action upon it by foreign agents; and secondly, by the admitted premises, *nothing*

can make healthy blood but the healthy action of the solids. Better had the chemist undertake the manufacture of blood *ab initio*, than the conversion of "fermented," "vitiated," or otherwise diseased, into healthy blood. And yet he admits that he cannot remotely imitate the most simple secretion, nor recombine its elements. Nay more; shall we not as soon attempt the manufacture of any organized part as that of the blood, or ~~of~~ its chemical transmutation from a morbidly altered to its natural constitution?

But, allowing to the humoralists what would be at once fatal to their doctrine, that remedial agents act directly upon the solids, it is wholly unphilosophical to suppose that they will be more curative than the primarily diseased blood will be morbific; especially since this fluid pervades every part, imbues every part with its own altered constitution, and becomes progressively more and more morbific.

Suppose, however, that remedial agents, by acting primarily upon the solids, arrest the morbific action of the blood,—hold it at bay, till they bring the solids into their normal state; we shall now be only where we began,—only a little more unwise for such an expedient. Why, again, should not the solids, originally, when in their normal state, have been more capable of resisting the morbific action of the blood, than they ultimately are, in a diseased state, of recovering their own natural condition, and of overcoming the morbific cause, converting it from a deadly to a friendly agent, and that cause the *pabulum vitæ*?

It appearing to us that the foregoing doctrine is based upon fundamental laws in physiology which admit of no "exception," it is manifest, that when the constitution of the blood is altered, or becomes diseased, in virtue of a diseased state of the solids, the blood so altered, is not an aggravating cause of disease, otherwise than has been explained. Indeed, were it under these circumstances a direct morbific agent, the same philosophy would hold as in the foregoing case; since the blood being wholly dependent upon the solids for its healthy constitution, the moment it becomes a morbific agent to the solids, the latter will have lost a control which it can never regain. These fundamental principles might have been inferred from the final cause of the blood, since it would seem to have been a radical defect in the animal economy, that a fluid which pervades so universally every part, which is intended for the growth and nutrition of the whole, which de-

pends upon those parts for its being and constitution, and is at all times in subordination to their existing state in the normal condition, should receive an impress from a part or the whole, or from any foreign source, which would not only defeat its great final cause, but give to it an ascendency over those powers and actions to which it is utterly submissive in their normal state.

The blood, in smallpox, it is said, or the virus of the pustule, will derange the healthy system in the same way as the subject from which they are derived. (p. 536.)(1) But, although the blood of the individual thus inoculated, acquire the specific character which appertains to the solids, even though it become "black and dissolved," and "the poison have killed all the salts," according to Dr. Stevens, and the quantity of purulent virus be generated in vast profusion, it is evident that neither are noxious; since, were it otherwise, the solids would never retrieve that ascendency by which they soon bring, spontaneously, the blood into its normal state. At the very moment, also, when suppuration is at its acme, and, according to the humoralist, the morbific matter is now expelled, the disease may rage with the greatest violence, and run on to a speedy and fatal termination; or, on the other hand, every gradation may follow betwixt that result and a rapid decline of disease; whilst the modifications that are induced by previous vaccination, or by variolous inoculation, illustrate the common principle upon which all the phenomena depend. And with these considerations should be connected the singular fact of the secondary fever, and that this fever takes place after the morbific virus is supposed to be wholly emulged, and that its intensity is often in the ratio of the greatest discharge.

There is an ever varying adaptation of the state of the blood to the varying condition of the solids, and this is brought about by the solids themselves. It proceeds, *pari passu*, with the changes of the latter. The properties of the blood, therefore, undergo changes corresponding with the alteration of the vital forces. The blood, in consequence, remains innoxious in all affections which begin in the solids. Its morbid state is the exact product of an antecedent change in the solids, by which they move on in harmony. It is owing to this correspondence be-

(1) This is also true of the virus of measles, as shown by Home, Cullen, Monro, Brown, Vogel, Tissot, Vaidy, Girtanner, Speranza, Wachsel, and others.

twixt the vital properties of a part and its secreted product, and even of parts which are only remotely subservient, that urine is inoffensive to the ducts of the kidneys and to the bladder, bile to its vesicle and to the mucous tract of the bowels, whilst they excite violent inflammation in other parts. The principle has a deep foundation in nature, and is one of the most important. It is one, too, which we submit to the consideration of the chemical and mechanical philosophers when they undertake an exposition of the laws of physiology.

Upon the foregoing principle, therefore, we may infer, *a priori*, that if perfectly healthy human blood be allowed to flow in any considerable quantity into the veins of a subject affected with fever, or scurvy, or inflammation of any important organ, or in chlorosis, &c., whilst the morbid blood flows out of other veins, such blood would unavoidably aggravate disease. (1) We need

(1) Magendie, with all his humoralism, remarks that, "as soon as the circulation was admitted, the profession was seized with the mania of supposing that the means of curing all diseases, and of rendering man immortal, were discovered. The blood was supposed to be the cause of all maladies. To cure them, therefore, nothing more was supposed to be necessary than to remove the bad blood, and to replace it with pure blood from a healthy animal." (*a*) (See p. 398.)

Even the transfusion of blood from one animal to another, where the species, and especially the genus, was different, has been fatal. This, of course, is ascribed by the mechanical philosophers to a difference in the shape or size of the globules. But we shall sufficiently show, in our remarks upon the microscope, that this hypothesis is entitled to no confidence. It is a remnant of the "error loci," and has guided the microscope to its conclusions. Admitting, however, that the difference exists, it is clear enough that the effect would not follow, considering the extreme minuteness of the globules, and that they are of a fluid nature. This conclusion is justified by experiments. Dieffenbach killed many pigeons by injecting into their circulation only a few drops of the blood of mammalia; whilst it is agreed that the sanguineous globules are smaller in the latter, than in birds. Dumas, Bischoff, and Müller, have also investigated this subject. The latter speaks of "the injection of blood with circular corpuscles into the vessels of a bird, (in which the corpuscles are elliptic and of a large size,) producing violent symptoms, similar to those of the strongest poisons," &c. He adds, that the symptoms "cannot be explained mechanically, since the corpuscles of the blood in mammalia are even smaller than those of birds." "The blood of fishes is said to be fatal to mammalia, as well as birds." (*b*) The other observers agree in this conclusion; and the whole tendency of their experiments is to show that the blood of one genus of animals is sooner or later fatal to another; and this through the whole range of the kingdom. Blundel killed the hardy dog by the transfusion of human blood. (*c*) The blood of oxen and sheep is fatal, in a few days, to cats and dogs.

Physiology, abstracted from its confusion with the physical sciences, supplies an interpretation of the foregoing phenomena. There is ample proof that the vital con-

(*a*) Précis Elément. &c. p. 340. (*b*) Elements of Physiology, vol. i. p. 141.
(*c*) Med. Chir. Trans. Lond. vol. ix.

not say, that this induction from philosophy has been practically demonstrated, even to the death, although the quantity of healthy blood transfused was small. What, then, would not be the fatality of any agents that should primarily remove a morbid condition of the whole circulating mass of blood where the solids have been primarily diseased? The properties of the renovated blood would have no adaptation to the altered state of the vital forces of the solids; and, if such a paradoxical condition could be effected, the immediate result would be an aggravated degree of disease in the solids, and a more highly morbid state of the blood.

Upon the same ground, the same results would follow were the blood primarily diseased, and the morbific agent *ab initio*.

The quantity of injected blood must bear a certain ratio to the whole quantity of the circulating mass, to produce the effects which we have indicated; and this is equally true of healthy blood in its relation to a diseased state of the solids, and of diseased blood in respect to a normal condition of the solids; just

stitution of different genera of animals differs in each, and it would be therefore absurd not to admit corresponding differences in the constitution of the vital fluid. (*a*) Hence arise the foregoing results. The properties of the blood of one animal not sustaining the necessary relations to the vital forces of another of a different genus, transfusions, as might have been expected on the soundest principles of solidism, would produce disease and death.

It is exactly upon the foregoing principle, that the blood of a healthy man will exasperate typhus, or other severe affections, or that diseased blood, in certain quantities, may produce disease in all healthy animals. As to the transfusion of healthy blood for the relief of disease, the experiments which have been made abundantly sustain our philosophy. Libasius, who invented the expedient, ridicules it at last. (*b*) Merclin condemns it in scurvy. (*c*) Purmann, and others, killed their patients by it. (*d*)

It should be constantly borne in mind, that in all the foregoing cases, whether it be diseased or healthy blood, a certain quantity must be injected to produce the imputed results; according to the natural differences amongst animals, or the nature and degree of the existing modification of the vital properties of the solids, of the altered blood, or the activity and proportion of any deleterious matter that may be combined with it. Thus, some 9 or 10 ounces of blood were tranfused with safety from the artery of a sheep into the veins of a healthy man, by Dr. Lower and Sir E. King; having previously bled the man to the extent of six or seven ounces. (*e*) On the other hand, Denys and Emerez killed a lunatic by transfusing the blood of a calf; and the death of a prince of the blood royal of France, from a like cause, was followed by a prohibitory edict of parliament against the practice.

(*a*) See Leacock, in Med. Chir. Journ. 1817, p. 276; Blundel, in Med. Chir. Trans. 1818; and Annales de Chimie, t. xviii and xxiii.

(*b*) Defensio Syntag. Arcan. Chym. &c. p. 8. 1615. (*c*) De Transfus. Sang.

(*d*) Haller, Element. Physiol. t. i. l. 3, s. 3, § 19; and the early Transactions of the London Royal Society. (*e*) Philos. Trans. vol. ii. p. 557.

as it is in regard to any other morbific agent when introduced into the circulation.

It follows, therefore, from the foregoing physiological principles, that blood, which is rendered morbid by the action of the solids, may excite disease when injected in certain quantities into the circulation of a healthy subject. It may be necessary, however, that the quantity should be large; and, as soon as morbid action follows, the whole mass of blood will become affected by that action, and be brought into a harmonious relation with the state of the solids. Convalescence may then begin spontaneously; or, art may so impress the vital forces, that they shall resist the irritation of the morbid portion which was injected, and recovery may begin before the whole mass is reduced to that harmonizing state with the vital forces of the solids, which would be necessary without the artificial impression. It is scarcely necessary to add, that the case which we are now supposing is utterly different from that in which it is affirmed that the whole mass of blood is primarily morbific. The injected portion is like any other morbific agent, nor does it assimilate to itself, any more than the latter, the circulating mass, which remains under the control of the solids, and receives from them its farther deterioration. Nor does it follow, that the injected blood will produce the same condition of disease,—least of all betwixt man and animals; though, were this universally true, the whole *rationale* comes entirely within the philosophy of solidism. If the disease be of a contagious nature, it may be that the specific virus will have been sufficiently absorbed into the blood, as we have already explained, to establish the same disease in the subject of the experiment,—so only the individuals be of the same genus or species. We have seen, however, that the experiment has generally failed, even in smallpox. But, there is nothing adverse to solidism, or favourable to the humoral pathology, in the supposition that the blood, in smallpox, measles, &c., becomes specifically affected by the action of the solids in those affections, and that it may thus, *per se*, propagate the same disease. We have no objection to this admission, though it may be difficult to show, if the disease be ever communicated by transfusion, that it is not owing to an absorption of the virus in the diseased subject, and its mechanical intermixture with the blood of the natural subject. From the foregoing explanation, it appears, also, why morbid blood, as in malignant fever, when injected into the cellular tis-

sue of an animal, may excite disease. There is no adaptation of one to the other. It is then a local irritant. But, when disease follows, the adaptation ensues, and then recovery may begin. But, in these cases, as we have endeavoured to show, the disease of the man and the animal may be wholly unlike. If the blood be injected into the cellular tissue, local inflammation and constitutional sympathies may follow. But this is not idiopathic fever. Still, however it may be regarded, our fundamental philosophy is not affected.

What we have stated in regard to the progressive adaptation of the blood in diseases to the existing alterations of the vital properties of the solids, and that, however varied the condition of the former, it does not become an aggravating cause of disease, is founded in an all-pervading law of the animal economy. We have shown its operation in the normal state of the organization, and we see it every where illustrated in disease. Various morbid secretions, which are offensive to the parts producing them in their healthy state, are innoxious to the same parts during their generation of the morbid products. And again, although these morbid secretions act with violence upon sound parts, as soon as the latter are brought into a morbid state, corresponding with that by which the virus was generated, the irritant is no longer hurtful. The surface which produces the syphilitic virus, or that of the smallpox, &c., is not offended by its presence; and yet, when applied to the abraded skin in its healthy state, it soon excites inflammation. But this, in its turn, assumes the suppurative condition, when it is no longer irritated by the offending cause; but, on the contrary, the vital forces may obtain an ascendency, throw off the enemy, and return spontaneously to their natural state. Had it been otherwise, nature would not have been true to herself, since the process would have been unceasingly destructive.

The foregoing products, in all the cases, are generated by the vital forces through their instruments of action. The products may exist without change; and having established, in the artificial cases, a specific alteration of the vital powers of a part, that altered condition, as we have said, may undergo no farther change from the continued presence of the morbific cause. But, should the morbid product, as the blandest pus for instance, become changed in its nature by the action of the atmosphere or other causes, a new substance is thus constituted, when other changes

will follow in the vital properties, and a new product will replace the purulent matter. And, although this product be more intensely irritating than pus to a healthy part, yet the vital forces being now adapted to its specific properties, it exerts no irritation. But, the irritant, having established another morbid condition, that disease may advance in virtue of the intrinsic nature of the vital forces; and, by the same constitutional disposition, they may undergo other changes spontaneously, or from the influence of any sympathies that may be in operation; and this, although the morbific agent be wholly withdrawn. Thence, especially, the fatal termination of diseases. If the agent be still present, the increase of disease, or its more absolute changes, will depend mainly upon the former causes. In many cases, indeed, as we have said, where the virus is of the most morbific nature, and becomes prodigious in amount, as in smallpox, &c., it not only ceases to be innoxious, but, whatever the condition of the blood, the vital properties may at last resist all tendencies to maintain their altered condition, and return to their normal state.(1) And here it may be worth remarking, that the morbific causes do not, like the blood, undergo any changes from the action of the solids. These principles belong to the basis of physiology in the science of solidism.

But, on the other hand, diseased blood, in the *humoral sense*, as we have already seen, comes not within the foregoing limitations. It is converted into a morbific cause, either by agents foreign to the vital powers, or by the vital powers themselves. In either case, the latter will have lost their control, since the blood is their natural stimulus, the pabulum of life, and depends upon a healthy state of the solids for its integrity. Disease must, therefore, advance progressively beyond the limit which it may be convenient for humoralism to assume; since no power remains by which the natural constitution of the blood can be restored, or its progressive deterioration arrested. Every act of the solids adds to the morbific influence of the blood, and brings a new and accumulating injury upon themselves. But if, again, on the other hand, in the sense of solidism, the alterations of the blood depend on the actions of the vital powers, its changes will be always suited to the existing condition of the powers of which they have been only a consequence; and since the changes are

(1) This class of diseases has been ably examined by Prof. Bigelow of Boston, under the name of "Self-limited Diseases." 1836.

thus exerted, the same powers, whatever their condition, can, either unassisted, or by aid of the solidist, replace those changes by others of every degree of approximation to the healthy standard. With such prerogatives who would not be a solidist? If we suppose, that in any serious local inflammation the blood becomes more or less altered, according to the foregoing principles, is it asked why the universal mass, being thus modified, is not detrimental to other parts? It is an obvious answer, that all other parts are now modified in their powers and functions, by the sympathetic influences of the local affection. In proportion as that affection is capable of modifying the blood, so does it exert its sympathetic influence over all parts of the organization. "Confluxio una, conspiratio una, consententia omnia. Juxta totius quidem corporis naturam omnia; juxta partem vero partes in unaquaque parte ad opus." (1) The modifications of the blood, and the constitutional derangement being produced by a common cause, the blood and the solids are universally adapted to each other. It matters not, therefore, how "black," "woolly," or dissolved, the blood may become in "scurvy," and "putrid" fevers; and that such patients ever recover is especially owing to the absence of healthy, stimulating blood.

Nature has endowed the living organization with numerous resources for its protection; some of which may be habitually dormant, but are called into action by many accidental causes that would constantly endanger life without them. It is for this reason that a degree of abstinence from food may be often advantageously borne in disease, which would be fatal in health; and, in this way must be explained the extraordinary changes of the hybernating animals. The bladder, bowels, &c. may not be seriously irritated by the morbid products of the kidneys and liver, that might excite inflammation were they injected into the former organs in their healthy state.

Finally, were not the foregoing all-wise provisions established in the constitution of organized matter, many diseases, which it may now throw off, would require for their removal the interposition of miraculous power. The morbific blood would not develope disease in one part alone, as strangely overlooked by the humoralist,(2) but throughout the animated machine; and, becom-

(1) Hippocrates, de Aliment. ver. 45.

(2) In all cases of humoral origin, every part should be diseased; not this organ alone, or that; but all universally. There should not be inflammation of the liver or

ing itself progressively diseased in the ratio of its morbific influence upon the solids, would hasten the general disruption in an increasing ratio. The blood of the victim of smallpox would poison more and more profoundly, whilst the purulent virus should excoriate the body, and lend its powerful aid in the general work of destruction. The miasma of fever would be but an *ignis fatuus*, for it has converted the whole mass of blood into a morbific agent, whose intensity increases as disease advances. The "leaven," too, waxes powerful, as the "fermentation" goes on. "The blood becomes blacker and blacker, and more and more dissolved." The various emunctories are of no avail, since the blood itself is the agent of destruction. We have the authority of modern humoralists for what we are saying. "The blood," say they, "is the pabulum of the solids, and when the whole of the nutritive fluids is deranged, as in fever, there must then be functional disease in every part of the system, and the *extent* of this is *exactly in proportion* to the *vitiated state of the circulating amount.*" (1)

And what hope will remain to the scorbutic patient, at least when he shall have entered the "second and third degree" of the disease? Cut off by the humoralists from all aid of the solids, whose province it is to form and vitalize the blood, and maintain it in subjection, where shall he look for succour? What can arrest the morbific progress of that fluid, give it a new constitution and new life, whilst it is preying upon its only fountain; and

lungs in one case, and the brain remain perfectly sound, &c. True, "there is no fact better proved in physiology, than that the different tissues of the body are acted upon by particular stimuli only." (*a*) To this rule, however, the blood is an exception in its normal state, and therefore in its diseased, if humoralism have any foundation. But, that it has not, is shown by the foregoing fact, and the physiological principle, which we are now considering. The principle, with the fact, conducts us to the true cause of disease. In the science of solidism, the varying susceptibilities of different parts are available. Here, too, is found a powerful agent in the force of sympathy, which may variously modify the condition of parts remote from the organ on which the morbific impression is primarily made; and those distant parts may become the seat of the principal development. This is readily illustrated by the operation of remedial agents upon distant parts when applied to the stomach.

(1) Stevens, on the Blood, &c. p. 178.

We have quoted other late authorities to the same effect. It is the old absurdity: thus saith the learned Hillary: "In putrid fevers, especially in the putrid bilious fever in the West-Indies, when the *whole mass* of blood is in a violent motion, and in a dissolved state in the beginning of it, and is continually dissolving, and *hastening on* to a more dissolved and putrescent state," &c. (*b*)

(*a*) R. Williams' Elements of Medicine, vol. i. p. 21.

(*b*) Inquiry into the Method of Improving Med. Knowledge, p. 333.

which, too, had no agency in the primary work of deterioration? The "blood runs thin and black, turns thick and of a dark muddy colour." It advances from bad to worse, till, "in the third degree of the disease, it is as black as *ink*, and though *kept stirring in the vessel for many hours*, its fibrous parts have only the appearance of wool or hair floating in a muddy substance." (1) The poison has reached the highest degree of intensity. It circulates in every part, through every fibre. "Every drop of blood is diseased," and is the cause of the affection of the solids to the last moment of life. (2) "Ἰατρὸς ἴᾶται θάνατος." "Death will be the physician that cures." (See last of *note*, p. 540.) But the humoralist, nothing daunted with the hypothesis, undertakes the restoration of the blood in the "third degree of scurvy," or in the dying stage of cholera, in all its natural constitution and vitality, by salt and soda, in virtue of their direct BLOOD-MAKING FACULTY. (3) "*Homines interdum lupos, et contra convertet.*" (4)

Who is that with test glass and crucible that comes to mock the suffering of the hydrophobiac? (5) He is the master of nature's inscrutable processes, lays open her deepest mysteries, and expounds by his art the varying shades of her most latent operations. By fire and acids he penetrates the labyrinth; and thus possessed of the secret, he rivals nature herself in those productions which she accomplishes only through an endless complexity of organization, and not without, also, the paramount aid of the more incomprehensible forces of life and vital juices. He professes what nature must in vain attempt. The blood is the morbific agent, its laboratory is in ruins, and reacts by increasing the pestiferous influence of the vital fluid. It is therefore the business of art to recompound and regenerate the *pabulum vitæ*.

If the chemist fail of the manufacture of blood, or of restoring

(1) Babington, on the Morbid Conditions of the Blood, p. 20.

(2) It is even maintained by the moderate humoralist, "If fluids be not the first to be affected by morbid agents, they are, when deteriorated, the sustaining cause of the disease." *Eclectic Jour. of Med Phila. vol. ii. p.* 30. 1838.

(3) "The transmutation of all the varieties of food into blood, from which all other fluids and all the solids of the body are formed, comprehends so many extraordinary particulars, that the most minute observations yet made, and the explanations yet given respecting parts of the process, even of such as, resting on chemical principles, seem the most to be depended upon, leave us in a state of ignorance the most humiliating and profound." (a) (4) Pliny, l. 8, c. 22.

(5) This is no fiction. It has actually occurred in real life. (See p. 638.)

(a) British and Foreign Med. Rev. No. 4. p. 378.

its morbid to a healthy condition, it is because his process is open to observation and exposure. But the humoralist works in the dark, and can be consistent. He therefore applies himself to *drugging the blood.* He is nothing daunted, though the blood be of a "tarry nature," or "dissolved," or "putrid," or "woolly," or "turned to an infected jelly." The solids will take care of themselves, so only he provides them with the right sort of *pabulum.* He argues, that, if foreign agents produce a diseased state of the blood, they should be equally capable of restoring its healthy condition, give it a new constitution and a new life. But, however fallacious this induction, he argues consistently that art should attempt the impossibility. "Morbi solidorum a fluidis vitiosis producti sanari nequeunt, nisi sanato prius fluido;" whilst it is not less true that, "ita contra fluidorum morbi vitio solidorum facti non curantur, nisi solidis ante curatis.(1)

If we contemplate, for a moment, the spectacle of a subject in the advanced stages of scurvy or miasmatic fever, in whom the rich blood of health has been substituted, either by transfusion or by necromancy, for the "black and dissolved poison," we shall suffer the mortification of seeing this healthy blood manifest the same relation to the diseased solids as an equal amount of morbid blood to the healthy solids.

Does the humoralist resort to bloodletting; he carries off the poison by abstracting some dozen ounces from its circulating mass. But, is this conformable with fact or with the hypothesis, since the great bulk of the poison remains unaffected, and since, also, at the beginning of the disease, the poisoned mass was wholly less morbific? Our humoral friends affirm, indeed, that an inappreciable quantity of miasma, on entering the circulation, throws the whole mass into a ferment, and that this goes on progressively increasing; nay, that one drop of blood thus affected is sufficient to contaminate the whole mass;(2) and yet the enemy may be fully slain, after all his battlements are raised, by a single plunge of the lancet. But the doctrine of the humoralist admits no other philosophy of the effects of bloodletting than what re-

(1) Baglivi, de Fibra Mortice, l. i. Corol.

(2) It is so altered at the beginning of the West India fevers, says Dr. Stevens, "as to paralyze the heart;" and, as the disease advances, the strongest epithets are employed to express the pravity of the blood. "And when we examine the body in such cases, after death, we find the heart crammed with a *thin, black, half-putrid fluid*, that can only be distinguished from the black vomit by chemical agents." (a)

(a) Dr. Stevens, on the Blood, &c. p. 288. What is the test?

lates to the diminution of the general volume of the poison; or however otherwise, whilst any of the morbific blood remains, there can be no termination of disease.

But, say the humoralists, "is not brandy and water a very different thing from brandy without water? It is true, we cannot detect by smell, taste, or otherwise, the presence of the rattlesnake virus in the blood; but since we can combine the two former substances, there can be *no doubt*, that in the latter case, *the virus is taken into the circulation, and combined in the same way with the blood.*"(1) This is no fiction. It is humoralism, the doctrine and the proof, of which we have given many parallel examples. We introduce the present on account of the high quarter from which it emanates, as well as to supply a climax to the many other illustrations we have quoted, and because it may seem to some an argument beyond refutation. We must, therefore, be guilty of a serious commentary upon the supposed analogy. The humoralists affirm that the poison exerts great changes in the blood, chemical and vital. But this is not the only want of parallel with the mixture of brandy and water. To make the least progress towards a restoration of the solids, we must remove the entire mass of blood; whilst, in the brandy affair, there is scarcely more than a simple intermixture, and by a process almost as simple we may separate the component parts. But let it be marsh miasma, or any other substance, with which the water is contaminated; the same principles will apply. Suppose, however, to make the cases more coincident, that the water is composed of as many elements and compounds as are imputed to the blood, (some 45 it is said;) that, like this fluid, it possesses vitality, and, that the brandy alters entirely its composition and vital properties. Let it be also supposed, that the containing vessel is animated, and that it becomes diseased in proportion to the deterioration of the water, whilst its morbid state reacts upon the water, and increases in a corresponding ratio the deterioration of the fluid, and its morbific influences. The new combinations, therefore, among 45 constituents, must be liable, as in all humoral diseases, to an almost infinite variety. Will any one now attempt a restoration of the water, unless by emptying the vessel and filling it anew? And yet the humoralists tell us that the foregoing is the actual condition of the blood, and its relation to the solids in disease; and that nothing more may be neces-

(1) Lon. Med. Gazette, vol. xvi. p. 816. Our Italics, as is generally the case.

sary in the "worst forms" of malignant fever than muriate of soda, "the simplest of all means, and which is always at hand." (p. 397.) So, also, spake Van Helmont of his famous nostrum, the *precipitatus diaphoreticus Paracelsi.* He would disfranchise any physician who could not cure a fever with it in "four days."(1)

But, it is not always practicable to hold the humoralist to his own peculiar philosophy. He is sometimes guilty of the inconsistency of attempting the cure of diseases which form the strongest examples in humoralism, and these, indeed, more than any other, upon principles which guide the solidist. Shall we, however, in such a case, believe that the remedial agent, in its action upon the solids, will be more recuperative than the blood is morbific? That it will not only counteract the great agent of destruction, but bring back the solids and that agent to their natural standard? This would involve the absurdity of supposing that drugs and morbid properties are better capable of resisting the morbific mass in its highest advances of deterioration, than were the healthy properties when the deterioration was only in its incipient state. Admit, on the other hand, that the blood is not morbific, that the solids do not lose their control of this fluid, and the whole philosophy of medicine, the whole science of physiology, become simple and sublime. Then may we understand how bloodletting, or an emetic, or an emotion of mind, will suddenly arrest a miasmatic fever, or break up the ravages of scurvy.

Again the ground is shifted, and we hear the humoralist descanting after the following manner: "In the fevers that are produced by the *animal* aërial poisons, there is but one continued struggle from beginning to the end; but in the *marsh remittent* paroxysm generally succeeds paroxysm with alternate rest, until the poison be thrown out of the system, or until it be neutralized by the use of proper remedies."(2)

Here we have nothing as to the morbific agency of the blood, and nothing as to the action of remedies upon the solids. We

(1) Opera, c. 12, 6.—Helmont was a man of the world, and knew that wine would give a greater currency to his specific. It was better, also, to accompany it with a little of his usual philosophy. He therefore says, that "wine, as a vehicle, is a proper messenger to be sent on such an errand, as it knows the road, is well received wherever it comes, and readily admitted into the most private apartments of the human body."

(2) Dr. Stevens on the Blood, &c. p. 267.

must act upon the miasma itself. But without inquiring how far this is humoralism, or in what way a dose of quinine, or the sixteenth of a grain of arsenic, expel or neutralize the poison, we may ask if there be any sort of parallel between "throwing a poison out of the system," and "neutralizing" it in virtue of a chemical change, by common agents? Does not such vagueness of diction betray the imaginary nature of the whole hypothesis? Is not, however, the imputed existence of the miasma in the torrent of the circulation precluded by the fact that an intermittent may persist for many months, notwithstanding all the copious perspirations and other evacuations, besides a profusion of the neutralizing agents? Nor will we again repeat the experiments of Professor Herring, and others, by which they have shown that all substances injected into the circulation are entirely eliminated in a few minutes, or a few hours.

How different the practice of the solidist,—how enlarged his philosophy, — how various his remedies, — how consistent his doctrine, — how important to humanity! Let a single example illustrate and confirm his theory. According to the nature of the predisposing and exciting causes, he cures opthalmia, by an emetic, or cathartic, or by bark, or arsenic, or iodine, or mercury, or bloodletting, or leeching, or blisters, or electricity, or local sedatives or stimulants, and by light or darkness.

In concluding this part of our subject, we cannot forbear saying that we have been at all times sensible of its inherent "dryness" to many readers; and that there may be found in this consideration a motive for whatever may appear to have been irrelevant to our facts and argument. And, although the humoral pathology have been ostensibly our theme of remark, we have constantly made it subordinate to our higher object of illustrating the most important principles in physiology. We shall be still actuated by this paramount motive in our subsequent observations; as, indeed, we may add, in our various essays.

From the manner in which we have spoken of Bichat, (p. 633,) and from the circumstance that M. Andral begins his systematic defence of humoralism by appealing to an admission of that vitalist in favour of the hypothesis, it may be worth while to say,

that it is stated by M. Andral, that "this idea was in a manner lost in his work, (*Anatomie Générale,*) without being followed up by himself or his cotemporaries."(1) The conclusion is therefore obvious, without regarding the direct affirmations which we have quoted; and Bichat and science should have the benefit of it.

(1) Patholog. Anat. vol. i. p. 388.

APPENDIX I. TO THE HUMORAL PATHOLOGY.

(See pp. 585, 590.)

SCURVY AND DIABETES.

"Si extincta aliquid, nisi nomina, restat,
Et gracilis structos effugit umbra rogos;" — (1)
Still Dunce the second reigns like Dunce the first." (2)

We have no disposition to trample upon the mistakes of genius, or upon time-honoured prejudice, were it even in our power, or however inviting the opportunity; and we should have closed the examination of the humoral pathology with our last section, — reiterating the sentiment, "*sit honor antiquitatis, sit ingentibus factis, sit fabulis quoque*," — were there not a special intrenchment which may refuse to surrender.

Dr. Babington (3) goes so far as to affirm, with Lind in his presence, that "it seems to be the *universal* opinion of those who have seen and written on scurvy, that it owes its *origin* to a morbid change in the fluids, and especially in the blood; and even those, who have been the most *strenuous opposers* of the humoral pathology in general, among the *most celebrated* of whom we may reckon Willis, (4) Hoffmann, (5) Boerhaave, (6) Cullen, and Sir John Pringle, (7) have made an exception in favour of this disease."

The citation of the foregoing champions of the humoral pathology as "its most strenuous opposers," is, *prima facie*, conclusive that real solidists who have written systematically, and most intelligibly, upon the scurvy, have not made the imputed exception; whilst it shows the slender resources of humoralism.

M. Andral, also, remarks that, "there is a disease which has shaken the belief of most of the solidists in *their* doctrine; I mean the scurvy." (8) And yet again: "No one doubts," says Dr. Tweedie, "that scurvy depends on vitiation

(1) Ovid. Tristium l. 4, el. 10. (2) Pope.

(3) Cyclopædia of Anatomy and Physiology. Art. Morbid Conditions of the Blood, p. 416. Our typography.

(4) Willis was not only humoralist in a general sense, but he was more remarkable for hypothesis than any other distinguished medical writer.

(5) Deducting Hoffmann's peculiar theory, all his medical speculations were purely chemical and mechanical.

(6) "The whole of Boerhaave's remarks upon the scurvy," says Lind, "consist of scraps taken from different authors." (a) But, Boerhaave "a strenuous and celebrated opposer of the humoral pathology."!

(7) Pringle says that even "the *heats* of summer tend to produce diseases in Autumn, by disposing the humours to *corruption*." (b) He is the greatest of all champions of living "putrefaction." Cullen "excepted" several diseases.

(8) Andral's Patholog. Anat. vol. i. p. 412.

(a) Lind on the Scurvy, p. 24. (b) On Diseases of the Army, Part 2, c. 3, p. 110.

of the blood in consequence of improper diet, or living in an unwholesome atmosphere." (1) Dr. Hall calls it "the *pons asinorum* of the solidists. The *dissolved* state of the blood is continually reproduced by the humorists, and pertinaciously neglected by the solidists." (2) (p. 451.) Dr. John Clark disposes of the question by saying, that "by the united consent of physicians, scurvy is considered a putrid disease." (3) Mr. Jennings affirms that the "alteration" of the blood in scurvy, "is evidently the cause of most of the symptoms present." (4) And so Alison, (5) and many other writers of the present day.

We shall proceed, in the first place, to show that a mistake, important to the interests of mankind, of science, and of the dead, has been made in the foregoing statements. We shall relate opinions briefly, at present; and our subsequent examination of the subject, if not what we have already said, will show the grounds upon which they may be sustained.

Lind, who probably saw more of the scurvy than any other man, remarks, at the close of his experience, "I am fully confirmed in my opinion, that whatever weakens the constitution, and especially the organs of digestion, may serve, without any other cause, to introduce this disease, in a slighter or higher degree, even among such as live on fresh vegetables, greens, or the most wholesome diet, and in the purest air." (6)

Sir G. Blane says, "it has *long appeared* to me, that the scurvy is owing rather to a defect of nourishment, than to a vitiated state of it." He assigns "the proximate cause to a defect of the living tone and irritability of the fibres in general, particularly those of the *vascular* system, and also a diminution of their simple elasticity and cohesion." (7)

Milman asserts, that "the scurvy is not a disease of the fluids, but of the solids; that its proximate cause consists in the gradual diminution of the vital power by the remote causes of the disease." "The causes of scurvy, the actual state of the blood, and the secretions, in the disease, all concur to make me believe that it does not consist in a putridity of the blood; nor is there anything characteristic in the state of the blood." (8) He exposed the fallacy of Pringle's experiments and hypothesis.

Rouppe, also, attributes the disease wholly to the solids. "The condition of the blood is the effect, and not the cause of the disease." (9)

Fordyce, after arguing that "it is depression of strength that occasions putrefaction in scurvy," concludes from that, and other considerations, that "the depression of strength is the cause of the putrefaction of the fluids in fever, and not the putrefaction of the fluids the cause of the depression of strength." (10)

Brown says, "scurvy is an universal disease of debility." "The pretence of its being cured by greens, roots, sourcrout, and similar things, is derived from a *noted blunder* among physicians, who take up the greatest falsehoods, or such facts as have a very narrow foundation in truth." (11)

(1) Tweedie in Cyclopædia of Prac. Med. Lon. Art. Fever, p. 195.
(2) Hall in Cyclopædia of Prac. Med. Art. Blood, p. 274.
(3) Clark on Diseases of Long Voyages, p. 420.
(4) On the Chemistry of the Blood, in Provincial Trans. 1835, vol. iii. p. 78.
(5) Outlines of Physiology and Pathology, p. 626. (6) *Postscript* to his Work on the Scurvy.
(7) Blane's Observations on the Diseases of Seamen, p. 509, &c.
(8) Inquiry into the Source and Seat of the Scurvy, &c. c. 6.
(9) Rouppe de Morbis Navigantium, s. 2, c. 2. (10) Fordyce on Fever, vol. ii. Dis. 3, p. 88.
(11) Brown's Elements of Medicine.

"The scurvy," says Girtanner, "concerning the nature of which medical men have formed so many false and ridiculous theories, is owing to an accumulation of irritability." (1)

We need not say that Baglivi maintains the dependence of all changes in the fluids upon the action of the solids. "A neglecto solidorum studio in morbis curandis, plures apud medicos erroneas, et falsas opiniones vigere hodie non sine animi mœrore video." (2)

"They, who have attempted to investigate the nature of the vital functions," says Pinel, "and the nervous action of parts, know with what extreme circumspection we ought to submit to the chemical explanations which are given of the phenomena of the animal body. They have motives, the most important, for not regarding the alteration of the fluids in putrid diseases, except as subordinate to the forces of life." (3)

Heberden's opinion of scurvy may be inferred from the following remark: "The more we know of the human body, the more reason we find to believe that the seat of diseases is not to be sought for in the blood, to the sensible qualities of which they seem to have very little relation." (4)

Broussais has been claimed as admitting the humoral pathology of scurvy. (5) He appears to have yielded, at one time, to the popular opinion. But his prejudice could not withstand his deliberate judgement. "For ourselves," he says, "we have become more convinced, the more we observe of this disease, that it is often exceedingly complicated, and that the nervous system and the viscera are *primarily* affected in it." Again, "may we not, from these facts, advance, that the first modification upon which this disease depends, has its seat in the internal membrane of the alimentary canal?" (6)

De Haen controverted Pringle's experiments, and his doctrine of putridity. Of the scurvy, he says, "necdum etiam mea experimenta me convincere de vera putridine, etiam nondum alcalina, in vivente corpore animali." (7)

Darwin was utterly opposed to the humoral pathology in all its aspects. In concluding his Theory of Fever, he says, "thus have I given an outline of what may be termed the sympathetic theory of fevers, to distinguish it from the mechanic theory of Boerhaave, the spasmodic theory of Hoffman and of Cullen, and the putrid theory of Pringle." (8)

Bampfield says that, "In my opinion, the phenomena of scurvy are too various and manifold to be briefly explained by a single principle." (9)

We have already shown the opinion of Hunter and Bichat as to the whole merits of the humoral pathology. (pp. 633, 634.) The illustrious Rush is also another distinguished advocate of solidism. It was amongst his last recorded observations, that if any inscription be made upon his grave, he could wish it to be, that "he was an advocate for principles in medicine." How unjust to his memory to mutilate that inscription!

Moore (10) commends Milman for the success with which he refuted the hu-

(1) Girtanner, on the Laws of Irritability. (2) Baglivi Op. omnia de Morb. Solidorum, p. 311.
(3) Nosographie Philosophique, t. 1, p. 194. See, also, t. 3, p. 309, sur le Scorbut.
(4) Heberden, in Medical Trans. Lon. vols. 2, 4.
(5) See an able article on Medical Philosophy in Med. Chir. Rev. Lon. vol. 30, p. 397.
(6) Broussais' Physiology applied to Pathology, pp. 420, 423.
(7) Prob. de Scorbuto, in oper. t. 2, p. 440. (8) Darwin's Zoonomia, vol. 2, p. 625.
(9) On Scorbutic Dysentery, with obs. on Scurvy, p. 311.
(10) Moore's Medical Sketches, p. 226.

moral doctrine of scurvy. He has done it, he says, "with the acuteness of a philosopher."

Mills (1) made no "exceptions" in favour of humoralism. Kérauden (2) considers scurvy "an atonic affection of the vascular system." Monchy, Mateer, Lieutaud, and others of whom we shall speak, do not admit the humoral nature of scurvy. Lieutaud says, "some practitioners embrace the hypothesis of Eugalenus with both arms, which makes fools of the unskilful." (3)

It is remarkable that Woodall, the first English, and one of the earliest and best writers on scurvy, (4) considers it a primary affection of the solids; consisting in "obstructions of the liver or spleen, or both; as also it appeareth that the head is much diseased, for that the eies not onely looke evill coulored, but also the gums putrifie, and the teeth grow loose, and all the sinowie parts of the body beare their part in the disease;" &c. Then, as to the cure; "the first part of it is in the opening of obstructions. If the party be strong, open a vein." A cathartic, also; then acid fruits, and "comfortable spoone meate," a little wine, &c. Various other remedies are suggested; but, "the simple and apparent qualities of medicines are not alwaies alone to be respected, but rather their mysteries and hidden vertues." He had no specific; but "admonished young men to be wise and carefull to make right use of the various remedies, and as neere as they can, to respect in the use thereof, time, place, age, quantity, quality, temperament, climate, cause," &c. "Touching the cure of ulcers in this disease, (anticipating the modern doctrine,) untill the obstructions of the liver and spleen be removed, those ulcers give no place to good healing. Wherefore, since they must be carefully attended for conscience sake, I advise that all sharpe and violent applications be shunned, and all soft and anodyne things applied that you knowe or can learne, provided they bee warrantable medicines; for, otherwise, they not only strive against a streame, but put your patient to needlesse disquiet, and thereby increase this disease." (5)

Even Trotter, as we shall see, did not regard the scurvy as a humoral *dis-*

(1) Mills, on the Utility of Bloodletting in Fever, pp. 6, 74, 75, &c.

(2) Réflexions sommaires sur le Scorbut, 1803. (3) Lieutaud's Synopsis, &c. p. 87.

(4) Woodall had great experience with the vegetable acids, and was one of the first who pointed out their utility. But, he did not consider them, as Blane did, "sui generis, nil simile nec secundum;" (a) knowing that change of air and proper food were more generally useful.

(5) Woodall's Surgeon's Mate, pp. 180, 197, 201. 1617.

This was thirteen years after the work of the fanciful Eugalenus, and seven before that of Sennertus. The Surgeon's Mate embraces the most philosophical treatment of scurvy of any extant, not excepting Lind's. We have no better system for the "Constitutional Treatment of Local Affections," than the following.

"I have ever observed," says Woodall, "that TUMOURS in any outward part of the body, growing either by repletion, obstruction, fever, &c.; yea, even in pestilential and venemous fevers in good bodies, not being pockie nor too olde, are easily healed by any understanding artist, that can joyne *reason* and *experience* together, many several waies; namely, for one, consider what might be the cause thereof as neere as you can. If you find it to be fulnesse of the bodie, or costivenesse, you have divers present remedies that way to flie unto." He then indicates a variety of constitutional means. "It is requisite that the naturall temperament of the part be carefully preserved; and this is done by a certaine carefull regiment of the body, which is universall or particular. The universall regiment consisteth in purging medicines, ("blue pill"!) a sober and frugall dyet, abstinence from wine, venerie, and stimulating meats, and moderate use of good nourishing sustenance." "Nature is content with small things, and the cause removed, the accidents or effects cease. I wish rather a surgeon should heele gently, thus avoiding accidents, which very many, in the height of their great conceited skill procure; which, were it but onely the guilt of conscience, if they feared God, they should not dare to doe." *pp.* 198, 149, 155.

(a) A Brief Statement of the Improvement of the Health of the Navy; in Med. Chir. Trans. vol. vi. p. 288.

ease; and his peculiar doctrine, as we have stated in a former section, had some distinguished disciples.

Authors, who have imputed the scurvy to a diseased state of the blood, have copied each other without examination, (1) or have been wild in their conjectures. Their hypotheses, however, in some respects have had no sort of unity; every copyist thinking it important to lend the aid of his own imagination. Hence we have the various designations of the *rancid* scurvy; scurvy from *fixed salt; acid* scurvy; *alkaline* scurvy; *sulphuric-saline, salino-sulphurous,* and *nitro-sulphurous* scurvy; *saponary* scurvy; the *muriatic* scurvy: the *brine* scurvy; the *rancid,* and the *oily* scurvy; the *hot,* and the *cold* scurvy; the *phlegmatic* scurvy; the *acido-austere,* and the *fœtid-alkaline* scurvy. Eugalenus has forty-nine species, and Boerhaave only a few less; according to the supposed conditions of the fluids. (2)

And then, as to the anti-scorbutic remedies; Mr. Kerr, who adopts the humoral pathology, says "they constitute a very formidable and extraordinary catalogue." (3)

Such are the authors who have "made an exception of this disease in favour of the humoral pathology." "One tells us," says Beddoes, "that the blood is *polluted,* another that it is *contaminated,* a third that it is *acrid,* a fourth that it is *putrescent,* without ever recollecting that to employ terms expressive of phenomena, such as the senses may recognise, and to reason upon such phenomena alone, are indispensable conditions in philosophising." (4) According to Cicero, "it is either from the weakness of age, from complaisance, or from some prejudice, that those people give things as truths, of which they have not the least possibility; and that they adhere to their opinions as firmly as they would to a rock on which they might chance to be cast in a storm." Down to the present century, those writers, who are said to have made "the exception," supposed that "the scurvy took all possible shapes, and was masked in such diseases as fevers, jaundice, dysentery, and scrofula. In late times, however, by one of those revolutions in opinions so frequently to be met with in the history of medicine, writers of the present age keep great silence on the subject." (5)

Having thus set aside the great defence which has been made to the claim of scurvy to the rank of a humoral disease, it might be that our object is attained.

(1) We have already stated that Lind makes this affirmation of Boerhaave; and "as to those who succeeded Eugalenus, who is looked upon at this day," says Lind, "as a standard author on scurvy, they did little more for some time than copy him." "They followed him most religiously and minutely." "Ten different practitioners pronounce ten cases to be scorbutic, which, upon examination, do not bear the least resemblance or analogy to each other." (*a*) Sydenham comments on this abuse, and remarks that "scurvy will obtain universally, and comprise most diseases." Sennert, who collected what had been written on this subject, is led by the confusion which prevailed to say, that "there is altogether so great a variety or mixture of complaints concurring in this disease, that there is hardly any other which assumes so many shapes and changes." (*b*)

(2) Hippocrates (*c*) and Pliny (*d*) derive their names for the scurvy from the part supposed to be most affected; the former calling it σκλην μεγας, and the latter stomacace and scelotrybe. Ægenita (*e*) the same as Pliny. Avicenna (*f*) calls it ægritudo splenicis. Celsus (*g*) also appends it to the spleen.

(3) Cyclopædia of Prac. Med. Lon. Art. Scorbutus, p. 678.

(4) Beddoes' Observations on Consumption, &c. p. 123.

(5) Dr. Mateer, in Dublin Jour. Med. and Chem. Science, vol. vi. p. 410.

(*a*) Lind, on the Scurvy, pp. 6, 13, 37. (*b*) Sennertus, Med. Prac. l. iii. s. 6, c. 5.
(*c*) Intern. Affect. s. 34, and 49; and Pror. l. 2. (*d*) Nat. Hist. l. 25, c. 3, s. 6. (*e*) L. 3, p. 69.
(*f*) Canon. l. 3, Fen. 15, tr. 2, c. 5. (*g*) L. 4, c. 9.

But, since scurvy is the main pillar and last hope of the humoral pathology, we are willing to concede that it may require other blows. But we will say, in the first place, that even those solidists who have been entrapped into the admission of the humoral nature of scurvy, either contradict themselves by their unqualified general doctrines, or by a specific avowal that their admission had no foundation. Of the latter class is Dr. Good, who finally says, that "though the fluids of the body are loose and coagulable, the muscular fibres are equally loose and incontractile; so that the latter, as justly observed by that excellent practical writer, Dr. James Lind, are as much affected as the former; and if we attend to the course of the symptoms as they arise, we shall find that *they are affected soonest;* for the *earliest signs* of the disease are those of languor, debility, and dejection." (1)

The next great proof which has been offered of the humoral nature and origin of scurvy relates, as in "putrid" fevers, to the appearances of the blood. But these appearances, as we have stated in regard to fevers, are only presented after disease has begun in the solids; and they may therefore be assumed as *prima facie* evidence of their dependence upon the morbid condition of the solids. We have examined this question, however, at large. All authors agree in stating many precursory symptoms appertaining to the solids, in a general sense, before there is any appearance of an altered state of the blood. These symptoms are given with sufficient detail, for our purpose, in the humoral account of the disease in the "Cyclopædia of Practical Medicine," to which we refer the reader. The hypothesis of humoralism, however, overlooks them entirely, and takes its start from the second stage of the complaint.

An example of the state of the blood is generally taken from the account by Lord Anson's surgeons; (2) and this is often copied from Lind without noticing the counter-statements of this author. But even those observations were vague; and Dr. Babington concedes that "we have only that general observation upon the blood in scurvy which was made by these surgeons." (3) It is important to recollect, also, that in this instance, the disease was of uncommon malignancy, destroying like the plague.

But let us look at enlarged experience upon this subject, and let the fate of solidism, as humoralism must, abide the test of the blood where disease has been fully developed.

"Is the blood," says Lind, "really in a state which tends to corruption? I have bled at different times above a hundred patients, in *all the different stages* of the disease; having even ventured in the last stage to take away an ounce or two of blood in order to inspect the condition of the fluid in dying persons. And, upon the whole, I have observed that the blood of those who are seized with the scurvy, *after a fit of sickness*, or a *fever* of *long continuance*, was generally of a soft or loose texture. But, the blood of *most other scorbutics* was in a *natural state.* There was, generally, after it had stood some time, a perfect separation of the serum from the red *concreted* mass. The latter, even in the *last stage* of the disorder, was *firm* and *compact*, and often covered with some

(1) Study of Medicine, Art Sea-Scurvy.

(2) This instance of the scurvy has become, in consequence, as notorious as "the plague of Athens," or, as the case by du Hamel, (See p. 534, and end of this vol.) It has been taken as the text by almost every humoral writer upon the scurvy since Lord Anson left the bequest to science.

(3) On the Morbid Condition of the Blood, Ut. Cit.

white streaks of what is commonly called the gluten or size of the blood." (1) For these, and other reasons, Dr. Lind remarks, that "it hence appears that this disease does not depend on any particular and obvious state of the blood; for its appearances out of the body are various, and the opinions of such as have inspected the blood only of one or two persons in this disease, (clearly referring to Lord Anson's surgeons) are, on that account, not to be admitted without great caution." (2) Why is not this experience of the most able writer upon scurvy noticed by the humoralists; especially when they quote Lord Anson's surgeons out of Lind's commentary? Lind also states, that the blood of scorbutics does not putrify sooner than healthy blood, nor does the urine become sooner fœtid; and this, too, when some of the organs may have been reduced to a state of gangrenous inflammation.

Trotter confirms the foregoing observations. "The blood coagulates; and it does not grow sooner putrid than other blood in the same temperature." It is affirmed, also, by Trotter, that "the only change in the state of the blood that is properly authenticated by writers on scurvy appears to me to be in the colour." (3) Beddoes is of the same opinion. (4) Rouppe found no remarkable changes in the blood. He finally "concludes from his dissections, his examination of the blood, and a review of the symptoms of the disease, against the opinion of there being a *thinness* or *fusion* of the blood; observing that this can only happen in the last stage, being rather the *effect* than the cause of the scurvy." (5) He adds that he never saw the blood "thin and dissolved as was observed amongst Lord Anson's crew." (6)

We confess, indeed, that we are surprised at the testimony of Lind, Trotter, and others upon this subject; since, upon the principles of solidism we should have looked for greater changes in the vital fluid, where, as in scurvy, disease of the solids is apt to be so universal. The sanguiferous system suffers especially. The heart, according to Lind, "is often found in a white and softened state; and, in all who died suddenly, the auricles were as big as one's fist." (7) Charleton says "it might be a task fit only for Jove himself to give an accurate account of scurvy and all its symptoms." (8) Woodall remarks, that "it is manifest that divers of those who have been opened after death have their livers utterly rotted. And others have their livers swollen to an exceeding greatness; some, the spleen extremely swollen; others have been full of water and stunk whilst they have lived; the cure whereof resteth only in the hands of the Almightie." (9)

The extensive influence of remote causes upon the solids, and especially the

(1) Does not this account for the "woolly" appearance of the blood, of which Babington speaks, when stirred in a vessel?

(2) Lind, on the Scurvy, p. 512. (3) Observations on the Scurvy, pp. 75, 125.

(4) Observations on the Scurvy, &c. p. 48.

(5) Just so it is stated by a defender of the humoral pathology, in respect to cholera. "There is no disease in which the blood undergoes more remarkable changes [?] than in the malignant cholera; *not*, indeed, in the *incipient* stage, as affirmed by Dr. Stevens, but in *direct proportion* to the *intensity and duration of the collapse*." So, Dr. Moore, in regard to puerperal fever. "In the *latter* stages of puerperal fever, the blood frequently presents the same phenomena as in other adynamic fevers. From the indisposition to coagulate, both during life and after death, *being always greater in proportion to the adynamia*, it has been argued that the vitality of the blood is diminished in the *latter* stages and *worst* forms of the disease." (a) The same is true of purpura hemorrhagica.

(6) De Morbis Navigantium, l. 2. c. 2. (7) Op. Cit. pp. 240, 241. (8) De Scorbuto.

(9) Surgeon's Mate, p. 182, 1617. See a narrative of this kind, in Philos. Trans. vol. xxvi. p. 223.

(a) Moore's Enquiry into the Pathology, &c. of Puerp. Fever, p. 186.

laws of sympathy, are entirely overlooked in regarding the various lesions which occur in scurvy. Even Andral admits, that "there is not a single one of the alterations that may not be in *other cases purely and simply a local affection.*" (1) "What, then, is the conclusion consistent with sound logic and true philosophy?"

If we regard, for a moment, the remote causes of scurvy, we shall find them all of a nature that denotes their primary action upon the solids. In the first place, the disease is wholly most prevalent at sea, where the atmosphere is not suspected of containing poisonous matter that may contaminate the blood. Secondly, cold is a highly predisposing cause. Thirdly, in a vast proportion of cases, old, indigestible salt beef or pork, (2) without vegetable food, are amongst the exciting causes, leading, as admitted by all, to a primary derangement of the digestive organs. Of more than forty authors, whose opinions are quoted by Lind, nearly the whole agree that salted food had an essential agency in the production of scurvy. Hence sprung up an hypothesis directly in opposition to the present, that the salt contaminated the blood. To this day, however, it is maintained by some that salt, *per se*, has a tendency to induce the scorbutic diathesis; (3) though we think the opinion was abundantly exploded by De Haen, Monro, Lind, Milman, Blane, and others. It is now well known, also, that scurvy may happen where vegetable food is employed, as at the Milbank Penitentiary, the Cape of Good Hope, &c.; the predisposing and exciting causes being very various. But we hold this fact to be conclusive against the humoral doctrine. Lind enumerates a variety of indigestible substances that apparently induce the disease. "An intense degree of cold, and moisture," are also principal ones; "the latter being of itself sufficient to dispose the constitution to the scurvy in any climate." Other causes of which he speaks are indolence, a sedentary and inactive life, grief, and other passions, and care, some antecedent disease, bad air, and all such causes as enfeeble the body; "the most common occasional cause being a gross, viscid diet." (4) In the new province of Queen Adelaide, in 1836, the disease broke out among the troops from no other apparent cause than "exposure to the cold night air." "In no instance did the disease appear amongst the officers." There was an abundance of good fresh meat, but few vegetables. (5) Trotter says, that "persons of a melancholic temperament are peculiarly disposed to scurvy whenever exposed to its exciting causes." (6) Alison, a staunch humoralist, after saying that "the main cause of these symptoms, (scurvy) is certainly the use of salted provisions," remarks that, "precisely similar symptoms are seen, in a few cases, at all peri-

(1) Patholog. Anat. vol. i. p. 412.

(2) An opinion, originating with Cocchi, (*a*) and Backstrom, prevailed for a long time, that "the true and primary cause of scurvy is a want of fresh vegetables." (*b*)

(3) See Mateer, in Dublin Journ. of Med. and Chem. Sci. vol. vi. pp. 409, 416. Burrows's Gulstonian Lecture, 1834. Paris on Diet. Alison's Physiol. and Pathol. p. 626. Mateer, however, maintains that "in all these cases it is the *solids* that are affected." Dr. Speer takes the same view of the subject as to salted food. "The digestive organs become impaired in their tone; the stomach and liver suffering both in their fabric and tone." — *Dublin Hospital Reports, vol. iii. p.* 185.

(4) Op. Cit. pp. 87, 44, 296. (5) Dr. Murray's Annual Report from the Cape of Good Hope.

(6) Observations on the Scurvy, p. 41. Again, as to the influence of mind: "Hot climates administer certain death to a 'mind diseased;' and where there is in the 'memory a rooted sorrow, or 'written troubles of the brain.'" And so of "nostalgia." Mosseley, ut cit. pp. 141, 142

(*a*) Bagal di Pisa, p. 253, *note*. (*b*) Backstrom, Obs. circa Scorbut. p. 12, et seq.

ods of life, in persons who have not lived on salted food, sometimes in persons previously quite healthy, often *repeatedly recurring* in the same individuals, and from causes altogether unknown."(1)

Again we see the philosophy of the operation of morbific causes illustrated by the following sequence as it respects the antecedent condition of the vital forces; "the earliest subjects of scurvy being those who are convalescent from some other affections, by which the whole body, as well as the organs of digestion have been greatly weakened; next, the indolent and lazy; then, the former subjects of the disease; and, finally, it becomes more universal." (2) However much the blood may be "purified" or otherwise restored, "it is necessary," even according to the humoralists, "to persevere for a considerable time in the counteracting regimen to prevent a recurrence of the disease." (3) Lind states that those who have before suffered rheumatism, pain from strains, bruises, &c., are apt to have repetitions of their former suffering, when exposed to the causes of scurvy; and this, without any other morbid phenomena; or, the former affections will simply reappear in a more aggravated degree.

It appears, indeed, from the testimony of all observers, that there are often numerous concurring causes of scurvy; but one, or more, of the whole is frequently absent. There is no specific predisposing agent, as in miasmatic fever; and there are now no pretensions to cure, as in that disease, by any remedy specifically addressed to the blood. All the causes are of a nature which can alone act upon the solids; and this, we shall see is equally true of the curative means.

Humoralism has received a shock from its unguarded pretensions to medicate the "black" blood in miasmatic fever by alkaline remedies; since it happens that the same condition of the blood is best medicated in scurvy by acids. (4) Here, too, salt is well known to aggravate the disease. In the former case, the blood becomes more florid in proportion to the quantities of acid employed, whilst saline substances render the blood "darker" and increase its "fluidity." "In seemingly the most desperate cases," says Lind, "the most quick and sensible relief was obtained from lemon juice." And how do this, and other acid fruits produce these effects? "That they strengthen the power of digestion," says Lind, "appears not only from the quick increase of appetite occasioned by them, but, from the belchings of wind which follow each dose." (5) And, is it not by a protecting power which they exert upon the digestive organs, that scurvy is now unknown amongst the fleets and armies which are provided with lemon juice and fresh vegetables? How do these articles of food exempt the blood from the contamination of morbific agents? "The cure is also entirely the same, whether the disease be contracted on land or at sea." (6)

(1) Alison's Physiol. and Pathol. p. 626. (2) Lind, Op. cit. p. 74.

(3) Dr. Kerr, in Cyclopædia of Prac. Med. Art. Scorbutus, p. 700.

(4) Dr. Stevens has no other resource here, than to flatly deny the fact, and to affirm that the disease, under his observation, "was *decidedly* brought on by the excessive use of citric acid." (a) This, we think, very probable. "Its excessive use" would be very apt to derange greatly the digestive organs. Lemon juice is a very common cause of cholera morbus. But the Doctor never saw but one case of scurvy. Upon the strength of this case, or rather the hypothesis, Dr. S. concludes that muriate of soda should be the cure. This, as we have seen, (p. 638,) was the reasoning of Silvius de la Boe, which desolated a great city.

(5) Lind, ut cit. pp. 236, 290. (6) Ibid. Op. cit. p. 53.

(a) On the Blood, &c. p. 451.

But, as we have seen, there are various other agents which will cure the scurvy, whose primary, and only action, can be upon the solids. A single blood-letting will stay the disease. Lind, and others, state that the subjects of scurvy, in its formidable conditions, often recover immediately on reaching land, without the aid of other remedies. Woodall remarks, "that to a man of judgement it may seem a wonder, how a poore miserable man, comeing on land from a long voiage, even at the point of death, scarce able to breathe by reason of strong obstructions, yet, in a few daies shall receive the fulnesse of former health; yea, with little or no medicine at all."(1) So Lieutaud,(2) and even so Dr. Stevens;(3) and a part even of Lord Anson's crew testify to the sudden effects of change of air at the island of Tinian;(4) whilst an elaborate defender of the humoral doctrine informs us, that "recovery from scorbutus, to use an expression of Lind, presents a *remarkable instance* of the *quick diminution of the effect* from the *cessation of the cause;* an observation which has been so amply confirmed by experience, that it may be regarded as a *most important axiom* in the *therapeutic* consideration of the disease. There are remedies, nevertheless, which expedite and even effect its removal under disadvantageous circumstances, and this with a degree of *rapidity*, which, considering its apparent ravages, seems most extraordinary and *peculiar to itself*, — the more so from our knowledge of its nature being very imperfect, and the *salutary operation of such remedies by no means obvious.*" These facts are irresistible. They cannot be explained away; and, therefore, the intelligent writer whom we quote, "rejects theory from the consideration of *this part* of his subject."(5) It would appear, therefore, that scurvy, least of all diseases, is entitled to humoral reputation.

Mr. Boyle, reflecting profoundly on facts like the foregoing, observes, that "I, who have gone through great and dangerous fits of sickness, scarce ever found any more violent than that occasioned by the *motion* and *scent* of a ship, together with the *air at sea;* and yet this violent illness, as it was not caused by any *peccant humour* in the body, was quickly removed by a quiet shore and change of air."(6) And another distinguished humoralist, Dr. Mead, in speaking of the scurvy, recognises the necessity of some aid from the solids in the work of restoration; though in such ambiguous language as shall least impair the hypothesis. "The sudden good effects of fresh air," he says, "affords a plain proof of what we have before said, that besides the blood, the animal spirits themselves are much affected in this disease; for such immediate relief could only be given by the means of this active fluid, the main instrument of all vital motions."(7) Divesting the "main instrument" of its hypothetical character, how is the air to rescue the vital properties from the morbific action of their natural agent, but by its influence upon the former? (See p. 487.)

But, the predisposing causes of scurvy are, manifestly, often of a malarious nature; but even then, the curative effects of change of air are equally great and sudden. On the other hand, Lind states a remarkable instance, at the siege of Thorn, where the disease was induced by moral causes, and an unwholesome diet; the besiegers enjoying an immunity from the disease. But

(1) Op. cit p. 182. (2) Synopsis, &c. p. 88. (3) On the Blood, p. 301.
(4) See Voyage. (5) Dr. Kerr, in Cyclopædia Prac. Med. Art. Scorbutus, p 693.
(6) Boyle's Phil. Works, vol. i. p. 93. (7) Discourse on the Scurvy.

the moment the besieged obtained a supply of suitable food, the endemic disappeared. (1)

We have already shown that indigestible food and moral emotions operate primarily by deranging the chylopoietic viscera; and, if the cause continue to be applied, the most extensive and injurious influences may be sympathetically exerted. It is also a rational conclusion, that where the healthy actions of the system have been deranged by such causes, there will be a melioration of disease when the causes are removed. Exhilaration of mind, and suitable food, operate beneficially through the same medium which had been the seat of the operation of their antagonists. How is the blood to be corrected, how new blood supplied, in these cases, but by a previous revival of the vital powers of the digestive organs? And does not that primary restoration bring the whole system, more and more, at every moment of its progress, under its salutary influence?

We will take an instance, indeed, where imagination alone cured and extinguished a most formidable endemic scurvy, and this, too, notwithstanding the continued use of indigestible food. The statement is given by one of the attending surgeons. This instance is also remarkable for having been the *origin of* HOMŒOPATHY in all its real attributes, and we shall, therefore, give a literal account of it.

Two hundred years ago, or in 1625, "the city of Breda, from a long siege, suffered all the miseries that fatigue, bad provisions, and distress of mind, could bring upon its inhabitants. Among other misfortunes, the scurvy made its appearance, and carried off a great multitude. The Prince of Orange, unable to relieve the garrison, contrived to introduce letters addressed to the men, promising them the most speedy assistance. These were accompanied with medicines against the scurvy, said to be of great price, but of still greater efficacy. The effects were indeed astonishing. Three small phials of the medicine were given to each surgeon. It was also publicly given out that two or three drops were sufficient to impart a healing virtue to a gallon of water. We now displayed our wonder-working balsams. Nor were even the officers of the garrison let into the secret of the cheat. The inhabitants flocked in crowds about us, every one entreating that a few drops might be reserved for his use. Cheerfulness again appeared in every countenance, and an universal faith prevailed in the sovereign virtue of the Prince's remedy. The effect of the delusion was inexpressibly astonishing; for many were quickly and perfectly restored. Such as had not moved their limbs for a month before were immediately seen walking in the streets, with their limbs sound, straight, and whole. Many, who had declared that they had been rendered worse by all former remedies, recovered in a few days, to their inexpressible joy, and the no less general surprise, by taking our coloured water, which we affirmed to be their gracious Prince's cure." (2)

(1) Op. Cit. c. 2, p. 395.

(2) Frederick Vander Mye, de Morbis Bredanis, p. 32.

As our printer is about reaching the close of this volume, the January No., 1840, of the New-York Journ. of Med. and Surg. has been published. At the end of that No. is a faithful summary of the doctrines and pretensions of Homœopathy. The writer, Prof. Lee, after saying that one drop of a homœopathic tincture in its 30th dilution, ("often used by Simpson, the most judicious writer,") "would form a mass of alcohol larger than the whole solar system," remarks very justly, that, "were the human system as susceptible to the action of medicines, as is claimed by the homœopa-

DIABETES.

Next to the scurvy, M. Andral, adopting the opinion of Willis, as well as other humoralists, regards diabetes as a barrier in humoral pathology which it would be absurd to assail. This is also one of the diseases of which Mr. Lawrence and others say, that "we are principally indebted to chemistry for its theory, and the debt should be acknowledged." (1) We shall soon see, however, that such an acknowledgement is no more due to chemistry than to humoralism.

We reluctantly renew the discussion in respect to organic chemistry; but it becomes indispensable in examining the merits of our present subject. There have not been wanting those who have set forth the blood in diabetes as offering different results in the crucible from such as are manifested by the blood of healthy subjects; and that such will continue to be the case till enlightened chemists shall have rescued their science from this species of quackery, we have no doubt. But, whilst we have shown that we are authorized by organic chemists themselves to hold this language, we certainly do not mean to imply that they are not, generally, among the most profound, and that inorganic chemistry is especially indebted to them for its high advances. We speak of facts abstractedly, and these we must regard according to their intrinsic merit. We know of no other method by which error can be arrested; and we have felt that we were but speaking to the invader, when organic chemistry, and its ulterior results, have come in collision with the laws of life.

Mr. Kane has stepped forward in behalf of the dignity of chemical science, and it is to such philosophers that an "acknowledgement" is due from physiologists. In respect to the blood of diabetics, he remarks, that "the results of these analyses show that in diabetes, the relative proportions of the organic principles remain quite within the limits of the composition of the blood *in perfect health*. In fact, the blood cannot, as far as these experiments go, be considered as at all affected in this distressing malady." (2)

This is exactly the conclusion at which Dr. Prout had arrived, as to the blood in confirmed diabetes. "Our results," says Dr. Prout, "very nearly coincide with the proportions ascertained by Marcet and Berzelius to exist in the serum of healthy blood." "The albumen yielded precisely the same results as albumen from the serum of healthy blood." The same was true of the cruor. And still more recently we find it stated in the British and Foreign

this school, it is very evident we could not withstand the influences of the ordinary causes and agents, to which we are constantly exposed through the medium of food, drink, and the atmosphere." "Finally, we may state, not by way of argument, (see our p. 663,) but of illustration, that, notwithstanding the years that have now elapsed since the doctrines of homœopathy were published, not one distinguished member of the profession in any country has adopted them. This is not to be wondered at; for, between these and medical science there is no such thing as a compromise. They stand in direct opposition to each other. If homœopathy be true, what is to become of the accumulated wisdom of ages! That the credulous should become converts to what is new is by no means strange; that the unprincipled should seize upon it as the stepping-stone to gain popularity, is almost too common to excite contempt. The reflecting and honest mind, while it is fully alive to the uncertainties of medicine, still recognizes it as a science whose foundations were laid centuries ago, to gain strength with the progress of time."

(1) Lectures on Physiology, &c. p. 71.

(2) Mr. Kane, in Dublin Journ. of Med. and Chem. Science, vol. i. p. 24.

Med. Rev. (1) that "the only well authenticated fact, in respect to the blood, appears to be, that in this disease (diabetes) there is a diminution of the proportion of red globules;" a fact which is very apt to attend many other diseases.

Now, we do not intend to avail ourselves of the advantage which is thus offered by these distinguished observers, any farther than to say that they afford additional proof that there is no dependence to be placed upon animal chemistry, at least in relation to any changes which may happen in disease; since no intelligent physiologist can doubt that the blood must undergo considerable alterations in diabetes from the morbid state of the solids. But we may fairly take the statements of the chemists as to what they do not find, where substantial materials are the objects of pursuit. We may say, then, that many of the present and ablest chemists have toiled in vain, as might have been expected, to detect the presence of sugar in the blood. There has been, indeed, an universal failure, if we except the affirmation of Ambrosini, who supposed that he had detected it in a minute quantity. (2)

Again, says Mr. Kane, "it is evident that the idea of this disease consisting in a conversion of urea into sugar is untenable, and that the secretion of that vegetable principle goes on without influencing, in any degree, the secretion of the other constituents of the urine." (3)

Even Dr. Prout admits that urea is always secreted in diabetes, though it may be in a diminished quantity. By the general consent of chemists, however, the quantity is not reduced. Nor has the presence of starch or gum been detected in the blood.

Notwithstanding, also, the familiar assumption that the urea is converted into saccharine matter, it is overlooked that the former substance has probably no existence in the blood, and depends for its own formation upon the action of the kidneys. (4) It has been sought in vain by Marchand, Gmelin, and

(1) Oct. 1838, p. 446.

(2) In Annali Universali di Medicina, 1835.

Since the foregoing was written, we have seen it stated by Mr. M'Gregor, in a late number of the London Med. Gazette, that sugar is readily found in the blood of diabetics. But Mr. M'G. goes too far, since he also affirms that it exists in the blood of healthy people who live much on vegetables. It should therefore follow, if there be the least foundation for the humoral doctrine, that the urine of the latter class, equally with that of diabetics, should contain sugar. Vegetables, too, it seems, are necessary to its formation. How, then, shall we explain the undiminished production of saccharine matter in many diabetics who confine themselves exclusively to animal food? This question must be answered. How has its existence in the blood hitherto defied the analyses of Berzelius, Vauquelin, Thenard, Wollaston, Bostock, Ségalas, Marcet, Prout, Kane, and others, if it exist in any proportion corresponding with the quantity eliminated by the kidneys, or even in the proportion as represented by Mr. M'Gregor?

Still more recently Dr. Rees (*a*) appears with a statement that he has detected a minute quantity of sugar in the serum of diabetics, — 1.80 out of 1000 parts. But is it certainly sugar? Admitting, however, that it is the genuine *saccharum*, is it at all remarkable that so small a quantity of an unirritating substance should be absorbed from the bladder, where it exists so abundantly? Is there any kind of ratio between the greatest admissible quantity in the blood and the product of the kidneys?

(3) Op. Cit. p. 21.

(4) Two exceptions, mentioned in Guy's Hospital Reports, vol. i. occurred in cases of diseased kidneys. There is much reason, however, to believe that there was a mistake as to the existence of urea in the blood. Nysten supposed that he had found it in the serum of blood, and Christison (*b*) in a dropsical effusion. Bostock (*c*) discovered something "possessing *peculiar* properties, which seemed to approach to those of urea" in the serum of a dropsical patient.

Marchand, for the express purpose of discovering urea in the blood, prepared a dog by feeding

(*a*) Guy's Hospital Reports, Oct. 1838. (*b*) In Edin. Med. & Surg. Journ. vol. xxxii. pp. 271, 274.

(*c*) In Bright's Med. Reports, vol. i. p. 94.

Tiedemann, Mitscherlich, and other eminent observers. Besides, how is the conversion affected, if at all? Certainly by the kidneys. Then why are not these organs capable of forming either of these compounds out of the elements

him for about two weeks upon sugar and water exclusively, and by tying the renal nerves. By the aid of *nitric* acid, upwards of 4 grains of what was supposed to be urea were generated from three pounds of blood. Another very equivocal result rests upon the modification of the crystals of muriate of soda by urea. (*a*) Rainy, (*b*) seeking for urea in the blood of a cholera patient, had no difficulty in obtaining about a grain from an ounce of blood, — all others, excepting Mr. O'Shaughnessy, having failed. Mr. Jennings says "the examination of the serum is very difficult." (Op. Cit. p. 81.) Herman ascribed the symptoms mainly to uncombined acetic acid, and Clanny to free carbon in the blood. Is there anything in cholera, unless it be its mysterious character, that is more likely to afford urea in the blood, than any other disease? Nay, even less, — for although the secretion of urine be suspended, the serous portion of the blood is carried off with a rapidity which has no example. Why does the urea stay behind? It has been lately affirmed by others, after a strict analysis of cholera-blood, that it contains nothing which is not found in health.

But, by what tests shall we say that this or that is urea when obtained from the blood in its minute quantities? The most undoubted of all, crystalization, is liable to objection, especially where microscopical objects are concerned. Or, who shall say that the substance in question is not formed during the analysis? It is said, indeed, that urea has been manufactured by Wheeler. But is it not also affirmed by the very able Edwards of Paris, and other eminent physiologists, that "it appears possible artificially to imitate the principal conditions of the secretions." (See p. 62.) Did not Dr. Good maintain that there "is iron enough in the blood of 40 men to make a good ploughshare"? And, whilst some eminent chemists deny its existence in the blood, although a metal of easy detection, others affirm that it is peculiar to the colouring matter, although Dr. Bird (*c*) finds it abundant in pus.

Here is another, and a plain problem. "Mr. Brande," says Mr. Bird, (*d*) "in a paper published in the Philosophical Transactions for 1809, first demonstrated the existence of albumen in saliva and mucus; in which secretions it was not then suspected, nor is it even now believed, by many, to exist." Mr. Bird thinks he has confirmed Mr. Brande's analysis. But let us look at the facts. They are important, as one of the numerous examples which show the nature of chemical facts and inductions in relation to animal chemistry. "This philosopher, (Mr. Brande,) states," says Mr. Bird, "that whenever an albuminous fluid, as white of egg diluted with water, is traversed by an electric current of considerable intensity, coagulation takes place at the negative electrode; and that when mucous secretions, as saliva, mucus of an oyster, &c. are submitted to the action of a similar current, a deposition of coagulated albumen invariably occurs at the negative side of the battery." Now for the proof of their identity. "This result is more particularly interesting, in consequence of the nearly *total failure* of the ordinary chemical reagents in detecting its presence in these secretions." And our author very candidly observes, that "to these experiments it may be objected, that the albumen, instead of being separated by electric action, is actually *formed* at the expense of the animal matter present in mucus; but this objection can scarcely, I conceive, be tenable, when we recollect the exceedingly low tension of the electric current employed in most of my experiments."

But, if the galvanic fluid be sufficient to separate the albumen, why not to *form* something like it out of other materials? Besides, do not the chemists say that all our secretions, &c., are effected by inappreciable quantities of galvanism? Must we not have a consistent rule?

It is an undeniable fact, that all the animal substances are variously modified in different parts of the body, according to the peculiar modifications of the vital powers in different parts. It is also admitted by the chemists that the elements of animal substances combine upon different principles than those of inorganic matter, as we have shown in our essay on the Vital Powers.

Christison says the best mode of detecting the presence of albumen in urine is by "nitric acid, corrosive sublimate, and ferrocyonate of potash; all of which separate a flaky precipitate. But the most convenient and most conclusive are the first two, namely, heat and nitric acid;" &c. We have no doubt of the precipitate, though we think it problematical how far it may be genuine albumen. Indeed, Dr. C. admits that there may be "sources of fallacy" arising from these tests. This, too, is allowed by Rayer, in his late work. And then we see it affirmed, by Dr. C., that, in "granular kidneys" "M. Solon has *committed the error* of supposing the blood to be *always deficient* in its albumen. So little is this an invariable rule that albumen is sometimes actually superabundant." This is the usual agreement. It is even a contested point whether coagulated albumen be one thing, and fibrine another. See p. 530, *note*, and our Essay on *Digestion*.

There is another important fact, also, to be considered in relation to albuminous urine. This

(*a*) Muller's Archiv. 1837, p. 440. (*b*) Lon. Med. Gaz. Jan. 1838.
(*c*) Guy's Hospital Reports, No. 6, 1838, p. 42. (*d*) Ibid. pp. 36, 40, 47, 48, &c.

of the blood? Nor should it be forgotten that it is affirmed by the chemists, that there is no urea or lithic acid in alimentary matter. (p. 584.) Although we have examined this subject in preceding sections, in relation to vital prinples, we have some other considerations to add.

The composition of urea and sugar excludes the chemical hypothesis; urea containing nearly half its weight of azote, whilst sugar contains none at all. And in connection with this fact, we would state a case which fully demonstrates the dependence of urea for its formation upon the vital action of the kidneys. Six or eight pints of urine, says Dr. Prout, were passed daily, "containing a large proportion of very white sugar, and very little urea." A few grains of opium diminished the quantity of urine, in about sixty hours, to two pints, "the saccharine matter disappeared, and was superseded by urea, the quantity of which had become excessive." (1) Here are three points to be considered; 1st, the rapid diminution of the urine from the influence of opium; 2d, the immediate substitution of urea for sugar, whose chemical composition

condition is the result of morbid action, and by every principle in physiology, therefore, it is a vital product; and again, it should otherwise appear in healthy states of the system. By the same rule, also, the product is not the same with healthy albumen, but necessarily modified according to the nature of the morbid changes in the organs upon which its manifestation depends. In confirmation of this view, we may say that the modifications of albumen are sometimes so great as to present very characteristic distinctions without the aid of chemical reagents. (a) The varieties, indeed, appear to be as considerable as the modifications of renal action would lead us, upon vital principles, to suppose. Dr. Bostock believes that albuminous urine exists in most persons, in perfect health, and that it may be increased by slight causes. It is said to be produced abundantly by mercury and cantharides. Graves, Mateer, Macintosh, and others, deny, entirely, that any dependence is to be placed upon albuminous urine as diagnostic of structural disease of the kidneys; whilst it is generally admitted that such urine occurs in the progress of many acute diseases.

We should not, indeed, be surprised to meet with almost anything in the urine. We have assigned reasons for this in our text; and from immemorial time the constituents of this fluid have been well known to be constantly varied by almost any changes of action that may occur in the body. We believe that the whole subject, in a practical sense, will continue to be prosecuted as it ever has been, by a reference, mainly, to vital signs for the true pathology. A new substance, *kiestein*, is said to have been lately detected in the urine of pregnant women, and is a diagnostic sign. Does this, too, exist in the blood?

The work of Dr. Christison, from which we have just quoted, has reached us since the preceding remarks were made. We here learn that Dr. C. finds the blood sometimes "loaded with urea." (b) Nevertheless, for various reasons which we have stated in this essay, we have little faith in the natural existence of urea in the blood, unless absorbed from the bladder, and that physiologists will ultimately settle down in the conviction that it can be generated by nothing but a kidney, which is neither a "sieve" nor "a chemical apparatus." Whoever will observe the stern determination of Dr. C. to refer albuminous urine exclusively to granular degeneration of the kidneys, will be disposed to make some allowance as to the urea with which he "loads" the blood.

An apology has been made till recently for the uniform failure to detect urea in healthy blood, that there must exist a certain quantity to be susceptible of detection. Certainly. But is it not a matter for philosophical doubt, if the urine derive its abundance, already formed, from the blood, that no trace of it should have ever been discovered, excepting when sought for in affections of the kidneys? Why the failure of some able observers to detect it in the blood after extirpation of the kidney? Why no vital signs of the presence of the urinary constituents in the blood in cases like those to which we formerly referred? (pp. 602, 603.) Why is the blood "loaded with urea" in "granular degeneration of the kidneys?" Is there any ratio betwixt such a "load" and the great quantity which is often secreted, and its apparent non-existence in the blood in all times of health? These are questions as well for the understanding, as for the laboratory; especially when we consider the complex organization of the kidney, its vital endowments, &c. Least of all should courtesy, or sympathy with the learned, prompt the physiologist to surrender the principles of his science, or the exercise of his own reason upon those principles.

(1) On the Nature, &c. of Diabetes and Calculus, p. 82, *Note.*

(a) See Willis on Urinary Diseases, &c. p. 164.

(b) On Granular Degeneration of the Kidneys, pp. 8, 40, 45, 54, 61, 82, 98.

is so wholly unlike; 3d, the "excessive quantity" of urea which was so suddenly produced. Did the opium exert all these changes upon the blood, or were they induced in virtue of its action upon the vital properties of the kidneys?

Here is another instance of the same import. "In a case of diabetes, under the care of Dr. Forbes, the first application of leeches to the *epigastrium* was *immediately* followed by a change in the urine to the *healthy* appearance." (1) So, Dr. Marshall speedily recovered a violent case of the disease by the application of tartar-emetic ointment to the spine. (2) And then, on the other hand, a morbid condition of the urine always coinciding with the derangement of the vital actions of the kidneys, not only in this, but in all other renal affections, is conclusive in favour of solidism. In "granular degeneration" of those organs, "the condition of the urine *varies* materially at *different stages* of the disease, but is at *all times* essentially morbid." (3)

We see, therefore, as well as from former considerations, how little dependence can be placed upon the experiments of Prevost and Dumas, Mayer, Vauquelin, Ségalas, and others whom we shall have mentioned, which led them to conclude, after extirpating the kidneys, that urea was generated in the blood. (4) A simple fact, like the foregoing, is pregnant with the most important physiological consequences, exalts the dignity of nature, rescues the function of secretion from the speculations of the mechanical philosophers, and establishes the laws of life and the complex organization of the glandular organs in their proper aspects.

Again, if the kidneys be indispensable to the formation of urea, we should look for an appropriate apparatus in all animals that generate urine; and such is found to be universally true. Chevreuil, (5) and Audouin, (6) found it in many insects, and, within the apparatus they profess to have discovered, at all times, uric acid. And just so is it with the stomach and liver. (See pp. 56, 57.) They are said by the mechanical philosophers to be allotted to all animals, that can be seen with the naked eye; and the microscopists affirm that the former organ at least, exists in many of the *invisible* race. And just so it is with the testes, &c. (7) The same principle is beautifully shown in the bladders of fishes, apparently like the lungs, stomach, or bladder, of some other animals, and yet secrete nothing but different gases, whilst each gas is always the same in each species. (See p. 58.)

(1) Cyclopædia of Prac. Med. Art. Diabetes, p. 546.

(2) Prac. Observations on Diseases of the Heart, &c., p. 64.

(3) Christison, On Granular Degeneration of the Kidneys, p. 32, 1839.

(4) The first attempt of this nature was made in 1822. See Bulletin des Sciences par la Société Philomatique, Juin, 1822.

(5) Strauss — Duerkheim's Consid. Générales sur l'Anat. des Animaux, t. 4. p. 251.

(6) L'Institut. 135.

(7) "*In insects*, the forms presented by the testes are infinitely various;" corresponding in this respect with other peculiarities of organization and vital endowments that are peculiar to each species. Still we observe the prevalence of a general law, not only as it regards the appropriation of this organ to the formation of semen, but, "the fundamental character of all is, that a *large secreting surface* is obtained in a small space, and all the *varieties of form* by which this end can be realized are here displayed." And then, as to another great and universal law, "however various the form of their elementary parts, all secreting glands, without exception, (not only those of the human body, but all met with in the animal kingdom,) follow the same law of conformation, and constitute an uninterrupted series from the simplest follicle to the most complex gland." (a) These,

(a) Muller's Elements of Physiology, vol. i. pp. 454, 456.

Müller concludes that "urea is not formed originally by the organs which excrete it, namely, the kidneys," but in the blood itself, from which it is strained off by the kidneys; and the "reason," he says, "why this substance is not found in healthy blood is, that it is separated from it by the kidneys as fast as it is formed." (1) This is not only an assumption without the slightest proof, but a summary mode of disposing of a most important principle in physiology. If urea exist in the blood, and the kidneys are mere strainers, no reason can be assigned, why this substance, in a fluid state, should not be strained off by any other secreting organ; or, where no urine is formed, it should certainly not only be detected, but should abound in the blood. We may also state the important fact, that it has been often observed that absolute degenerance of the kidneys is attended by a diminution of urea. One kidney, it is well known, is often wanting, and Andral admits "one instance of complete absence of both kidneys in the adult, which is that recorded by Klein." (2) Now, can it be entertained, that in such cases, and in others of great degeneracy of those organs, if the constituents of the urine really existed in the blood, they would be compatible with life? (See pp. 602, 603, 608.) Must all knowledge be renounced for mere speculation? "Every body knows," says Andral, "what serious symptoms appear in animals when their ureters are tied." (3) Here has been a real *generation* of urine going on, and it is now as it were forced into the circulation. (See pp. 526, 549, 556.) Yet, we doubt not, as overlooked by our author, that much of the general injury arises from the direct violence done to the kidneys. But, "we know," as Bichat remarks, "that urine destroys every part it touches."

Even Berzelius has assigned the best reasons for supposing that the sulphates and phosphates of the urine are formed by the kidneys. Whilst Brodie, (4) and others state that various acids and other substances are generated by the kidneys, in injuries of the spinal cord. As to all these products, however, since the urine has no vitality, no one can affirm that they have not been more or less the result of chemical changes after the elaboration of their constituents under some other combinations of which chemistry can furnish no account. This conclusion, indeed, is sustained by analogy in its relation to vital parts. (p. 57, *note.*) We have no doubt, as we have said, that chemical changes take place in the secretions which, like the urine, are strictly excrementitious, and this, perhaps, at the very moment after their vital elaboration. But, in respect to those fluids which, like the bile, have an agency in organic processes, the proof is greater that they are so constituted as to resist all changes in their constituents. The latter are combined in their elements more universally according to the specific agencies of the vital forces; the former consist more or less of the extraneous substances which arise from the perpetual decomposition of the body, or which may find their way into the circulation.

Again, Müller and others look upon the kidneys in the light of mechanical auxiliaries to the skin. Let us hear a farrier upon this subject. "Is it in accordance," says Percival, "with the other operations of the animal economy, to set down such a complete organic structure as the kidneys, with all its appendages,

and a thousand other facts, of a like nature, which we have stated, demand the serious consideration of the mere physical philosopher.

(1) Op. Cit. p. 151. (2) Patholog. Anat. vol. ii. p. 388. (3) Ibid. vol. i. p. 415.
(4) Brodie, in Med. Chir. Trans. Lon. vol. xx.

as nothing but a waste to the skin; a part with which in structure it will admit of no analogy whatever, and between whose secretions there is just as little affinity!" (1)

When the secretion of milk is suppressed, do we find that the saccharine matter is accumulated in the blood, or do we find a trace of it there, or is its secretion "replaced" by any other part? Or shall we go on believing with Puzos, Leveret, Sauvages, Van-Sweiten, Selle, Astruc, Raulin, and many others, that it is generated by the legs, and forms the proximate cause of phlegmasia dolens? Or, when the secretion of bile is suspended, do its peculiar constituents appear in the blood, or their elaboration devolve upon any other part? We have shown that it is not so. Would you believe the oath of any one who might swear that he had detected semen in the blood, or saliva of a eunuch! (2) Shall we admit that the virus of the rattle-snake, the viper, the bee, &c. exists in the blood? Do we find the peculiar odour of the skunk, the beaver, &c. in that fluid? Thus might we go on with a thousand different formations, which, if admitted to exist in the blood, would, of course, assign to that fluid as many component parts. But if, on the other hand, it be absurd to suppose that the latter formations do not depend upon their peculiar emunctories, why is it not equally so to imagine that urea, or cholesterine, &c., are merely strained off from the blood? Besides, nothing is gained by the humoralist or mechanical philosopher, by denying their dependence upon the vital agencies of the secerning organs, since they must have depended upon vital agencies if they actually exist in the blood. Moreover, the secretions only take place in a certain part of the glandular structures, never in their nutritive department.

Finally, as to urea, about which humoralism has been so much concerned in the philosophy of diabetes, we may say, that Le Canu, whose analysis of the blood is admitted to be the best, agrees with former chemists in denying its natural existence in that fluid. (3)

From the foregoing considerations, and others of a like nature which we have set forth in former sections, we arrive at the conclusion, that the humoral hypothesis of diabetes cannot be sustained; though in this discussion we have more in view the fundamental principles of vitalism. But, did either of the foregoing substances exist in the blood, it must be conceded that the quantity would not be at all commensurate with the amount of sugar which abounds in the urine of diabetics. Negroes, who gorge themselves with sugar-cane and its sap, have their blood as destitute of sugar as those who subsist on animal food; nor are they liable to diabetes, whilst "England, above all other countries, is that where diabetes is most prevalent." (4) And is it not in the highest degree probable, if sugar exist in the blood, it would be taken into the circulation in proportion to the quantity consumed? On the contrary, it is converted by the stomach into an albuminous and oleaginous substance. The sugar of milk is only generated, like diabetic sugar, by a particular organ. It depends on an unusual action of that organ; and wherever that action is established, this product follows in all animals. Does it accumulate in the blood when the gland is dormant? Here, then, is a strong analogy. And what is peculiarly interesting in this case, the action of the mammary gland is established by the

(1) Lectures on the Veterinary Art, Part 3, p. 94.

(2) And yet it is affirmed to exist in the blood. See p. 587, note.

(3) Etudes Chimiques sur le Sang. Humain, p. 15, 1837.

(4) Dr. Marsh, in Dublin Hospital Reports, vol. iii. p. 458.

sympathetic influence of the womb; thus setting at defiance not only the humoral pathology, but the mechanical and chemical doctrines of life.

It is a well known fact, that in a great proportion of cases, the most perfect diabetic urine has continued to be voided by patients who have been restricted to a diet of animal food. Indeed, there have been many cases in which diabetic urine has been formed for a series of years, under every variety of diet. In twenty successive cases, Dr. Prout had never known the urine to become natural. (1) Such is also the experience of Dr. Marsh: "It appears probable," he says "that however completely a diabetic patient may appear to be cured, the urine will ever after retain a slightly saccharine impregnation." As to food, he says, that "even with the few who can endure an animal diet, it will be found to effect but little towards the removal of the disease." (2) We have found it to exasperate the complaint, whilst in common with many others, we have seen the disease yield to a farinaceous diet and bloodletting.

We may appeal, also, to morbid anatomy, which discloses, invariably, some alteration, consisting at least, according to Snowden (3) and others, in a turgid state of the capillaries of the kidneys. It should be recollected, too, that the kidneys are not alone the seat of morbid action. So universally does the system participate, that Aretæus has observed of it, "diabetes qui vocatur affectus, *miraculum quoddam est*, haud ita sæpe hominibus visitatus, carnium, ac membrorum, in urinam colliquatio." (4) The digestive organs are probably the primary seat of disease, and the kidneys sympathetically affected. (5) It is worthy of remark, however, that a sagacious observer, Dr. Joseph H. Hunt, having made a careful anatomical examination of eight cases of diabetes, states that his "convictions are, that diabetes mellitus is a symptom of cerebral affection, *primarily;* and that death is the consequence of lesion of the brain." (6) We

(1) On the Nature, &c. of Diabetes and Calculus, p. 81.

(2) In Dublin Hospital Reports, vol. iii. p. 431.

(3) London Med. Gaz. April 22, 1837. (4) De Signis et Causis, &c. l. 2. c. 2.

(5) We believe this to be generally true of that disease which is described by Christison, Bright, and others, under the name of "Granular Degeneration of the Kidneys;" at least, we have scarcely met with such a disease as a primary affection in this country. Severe dyspepsy, and diarrhœa are stated by Dr. Christison to be very common attendants. It appears to us that morbid anatomy, (as well as chemistry,) has played its part too often in this disease, as in the "typhoid affection" of Paris. As in the latter disease, the "secondary affections" of the former are very numerous and serious. "They are the principal sources of danger." Christison states that the renal symptoms are frequently obscure, and that "cases are often met with where many months elapse in a state of quiescence." Indeed, he has met with instances where the *true primary* disease remained unknown altogether till the inspection of the dead body betrayed the condition of the kidneys, &c. When the liver is also diseased, as is frequently the case, "it is often exceedingly difficult," according to Christison, "to say which of the two is prior in its origin;" and the affection of the kidneys "is apt to present itself in constitutions sapped by various other diseases."

As to the complication of dropsy with renal disease, we have never seen an instance in a large practice for a quarter of a century. We believe it to be extremely rare in America. On the contrary, Christison "has not met with a single case of inflammatory dropsy during the last nine years, where there were not unequivocal signs of the kidneys being diseased." (a) We have seen numerous cases of "dropsy following scarlatina," but no renal affection. In the foregoing cases Dr. C. appears to have depended upon the chemical tests. But, as we rely more upon the vital, this may account for the difference in our experience.

(6) Boston Medical and Surg. Journ. June 12, 1839, p. 288. Marsh states that "the earliest disturbance in the general health could be distinctly traced to some cause acting upon the skin, &c." (b) The details of cases, however, show that the morbid condition of the skin is probably symp-

(a) On Granular Degeneration of the kidneys, pp. 28, 31, 39, 80, 81, 85, 87, 102, 105, 113, 115, 145, 155. 1839.

(b) Op. Cit. p. 432.

have witnessed no cerebral symptoms in this affection, other than occasional headache, till near the close of life.

Upon the principles of solidism, there is no difficulty in explaining the phenomenon of diabetic urine. Carbon and water are the constituents of sugar, and these abound in the blood. A peculiarly modified action of the kidneys, as we see in the mammary gland, may, therefore, combine them in such a manner as to result in the characteristic product. And since this product does not take place in the living body under any other circumstances, or from any other parts, it supplies a forcible illustration, that the combinations of elementary principles in organized beings are the result of vital, and not of chemical agencies. (See pp. 62, 67, and Essay on DIGESTION.)

But, let it be admitted that the eliminated saccharine matter existed in the blood; by what process is it generated? Will humoralism answer?

tomatic of abdominal disease; and, they continue to confirm the statement of Watt, that "there are, perhaps, few cases of diabetes without some affection of the abdomen, particularly in the epigastric region." "Some morbid change in the alvine excretion, especially costiveness, always accompanies the diabetic habit." (*b*) This has been our experience.

(*b*) On *Diabetes*, p. 47.

APPENDIX II. TO THE HUMORAL PATHOLOGY.

(See pp. 565, 590.)

ENDOSMOSE AND EXDOSMOSE.

"How little, mark! that portion of the ball,
Where, faint at best, the beams of science fall."—*Pope.*

ENDOSMOSE and EXDOSMOSE are, undoubtedly, terms of high import; but, fortunately for science their etymology does not imply that a dead animal substance or oiled silk are the same as a living tissue. This is a part of the great system of materialism by which many eminent physiologists, as we shall ultimately see, are endeavouring to consign to chance the origin of matter itself. We are concerned about it at present on account of the manner in which endosmose has been pressed into the service of humoralism. Its paternity appears to be claimed by Dr. Stevens, and we are certainly disposed to agree with him, that it is not "an American discovery." (1)

"The property of permeability by fluids possessed by tissues *even after death*," says Müller, "depends upon their invisible porosity, and is termed imbibition." (2) But, however this may happen "*even* after death," no proof has ever been offered that it happens "even" during life; whilst a great variety of facts, as we have hitherto shown, contradict the conclusion. These "invisible pores" are completely shut up by the vital forces, during life, against the operation of chemical and all physical powers; and it is only after the cessation of the former, that endosmose and exdosmose play their part. (3)

The absorption of nitrogen and oxygen may even take place by way of the

(1) On the Blood, p. 99. (2) Elements of Physiology, vol. i. p. 243.

(3) We owe an apology to Professor Draper for not having, in our Essay on the Vital Powers, excepted his paper on Endosmose and Exdosmose, (a) from our general remark, "that it is the whole aim of the Professor's series of papers to defend the application of the laws of inorganic chemistry to the phenomena of life," at least as we understand them. The paper, to which we now refer, is an able exposition of "proofs that the peculiar force known to chemists and physiologists, under the title of endosmose and exdosmose, has no existence," that "it bears no peculiar relation to organization; but, that it is a manifestation of capillary attraction. That, so far as the examination in this memoir has extended, there is no case upon record in which endosmosis has effected a real and undoubted chemical decomposition;" and "that those reputed instances of the production of secreted fluids by dead membranes, through this power, are fanciful illusions." "Urine is readily separated from arterial blood in the kidney; yet would any one expect, on placing blood upon a kidney, that urine would drop through it? A candid examination of many of the fashionable applications of endosmosis to physiological functions, will discover no wide difference between them and the hypothetical case." And may we not add, in refutation of our author's doctrine of catalytic forces, and all other chemical theories, that we shall scarcely obtain urine by *injecting* blood into a dead kidney. We must, also, persist in alleging the Professor's own affirmation (see p. 75,) against a remark with which he concludes this paper, "that chemical changes of all kinds occur in tissues and glands, is not to be doubted;" and we are the more disposed to press this fair objection, in consideration of the Professor's expedient to carry a controverted doctrine by imputing to his opponents an ignorance "of the processes of philosophical reasoning," and who, therefore, "take umbrage at any attempt to bring *physiology* under the *dominion* of *physical sciences*." It is true, the physiologists, who have taken the imputed "umbrage," have not adopted the "philo-

(a) Amer. Journ. Med. Sci. Aug. 1838, p. 302.

skin; since these gases are natural to the body. But, as we have endeavoured to show in our essay on animal heat, the process is wholly of a vital nature; and this is farther evinced by its not taking place after death, not even in the lungs. If endosmosis were founded in philosophy, the absorption of those gases should be greatest after the extinction of life. We may also say, in regard to the doctrine of "capillary attraction," it appears to be equally untenable, since the absorbent function of the lymphatics and other vessels ceases as soon as death takes place.

Müller carries the doctrine of *endosmosis* beyond any other physiologist. "It does not, however," he says, "explain the absorption of *all fluids* by the animal tissues." He has many hypothetical explanations of the vital phenomena, founded upon the various results of endosmose as occurring in dead matter. He often concedes, however, in reference to certain functions, that they are vital; whilst he remarks that, "the question whether the blood in capillary vessels, or those vessels themselves, exert on certain substances an attraction which differs in its nature from any *accounted for by physical laws*, is quite distinct from the one above discussed, (endosmose). There is only *one part* of the body in which this kind of attraction (absorption by *organic* attraction,) certainly exists, and that is in the capillary system of the placenta." This, however, he finally resolves into endosmose, "which undoubtedly takes place," he says, "between the fœtal and maternal blood." But this is slight compared with some of the violent transitions of our author. The common laws of endosmosis will not embrace the phenomena in such cases; and of this our author appears to have some suspicion, since he constructs a new philosophy for these examples, and affirms that "it is an organic and *vital* endosmosis *totally different* in the laws which regulate it from the *chemical* process of imbibition described by Dutrochet;" (1) so "totally different in the laws," that it would seem that our author has been employed in a burlesque of physiology. Where "the laws are so totally different," there is an absurdity in the common use of the term, and an inexcusable departure from philosophy in attempting any analogy in the processes. On the whole, however, we do not regret this termination of the matter; since, having passed the ordeal of chemistry, we see our own vital powers coming out under the new denomination of "*vital endosmosis*." Still there are some functions, as respiration, which our author sets down to simple endosmose; (2) and he also states that "the nerves have no influence on *inorganic* imbibition," because "there was, in my experiments, no perceptible difference in the absorption of poisons whether the nervus vagus was divided or not." (3) Here our author apparently attempts to identify the laws of inorganic and organic endosmose, by regarding the stomach as "inorganic" after the division of the par vagum. We see, therefore, how embarrassing our position may be, when we assume

sophical reasoning" of the chemists; but does it thence follow, that they have not reasoned from the phenomena of nature, and that in having formed all their inductions from such phenomena, they "have inverted the Baconian philosophy, with a success that is no unusual concomitant of the synthesis of causes"? (a)

We have seen that M. Raspail regards endosmose as a "soaking" process.

(1) Elements of Physiology, vol. i. pp. 248, 249. (2) Ibid. p. 230.

(3) Ibid. p. 250. It will be recollected, however, that the experiments of Dupuy, Brachet, and others, show exactly the reverse of this.

(a) Amer. Journ. Med. Sci. Nov. 1838, p. 60.

the identity of phenomena that may have some vague points of resemblance, and attempt the explanation of each by a common principle, and yet that principle admitted to involve "totally different laws." We feel also bound to repel the implied conversion of the stomach to an "inorganic" substance by the division of the nervus vagus.

Nor are we a little disappointed, on reading farther, to find that our author appears, at last, to have abandoned the idea of a "vital endosmosis;" since he states, that "the formation of any one of the peculiar secretions, the essential proximate constituents of which do not exist in the blood itself, presupposes the operation of a *special chemical apparatus, whether this be a membrane or a gland.*" Among the chemical formations, he immediately after mentions *semen* as one. (1) Our author also thinks it "a matter of doubt whether the absorption which goes on in the substance of the different textures of the body is *chiefly* performed by the blood-vessels or the lymphatics; that "it is quite uncertain *into which* order of vessels the matters are *first* received." (2) We have seen that the former supposition is most convenient for his defence of the humoral pathology. But the difficulty consists here, in comprehending any other use for the absorbents; and, more especially, if the "matters" of our body be "*first* received" into the blood-vessels, how they should afterwards enter the lymphatics.

We shall now enter, for a moment, upon a very important innovation of our author upon a fundamental point in physical organization, as well as in physiology. This we do the more so, on account of its almost universal recognition by the soundest observers. And since the microscope is *mainly* at the bottom of this new philosophy, we refer our readers to remarks which we have made upon that delusive instrument in an Appendix at the close of this volume.

"In accordance with physical laws, the effusion of fluids," says our author, "in the animal body *after death* is the effect of mere gravitation," and "*during life*, absorption effected by an attraction of a vital nature counterbalances *this* transudation of fluid through the membranes of the body; but in disease, different causes destroy the balance of the two processes, and then the water, with the animal matter and the salts dissolved in it, collects in the cavities of the body," &c. (3) Here the transudation which takes place after death, the secretion "during life," and its modifications in disease, are made to depend upon the same physical cause, viz., "mere gravitation." Such, too, is the doctrine of M. Andral, and many others. (p. 180.) We shall see that it has acquired strength since the "illusions of the microscope" have again superseded the phenomena of nature.

In respect to the transudation of blood, our author affirms that, "the coats of the vessels must be changed in texture, and in many cases, as for example in hæmoptysis and bloody expectoration which accompanies inflammation of the lungs, if not *in all*, there is *rupture* of the minute vessels or capillaries." Here we come to our author's paramount doctrine that the capillaries have no orifices, and by which physicians are made to regard, and to treat, all hemorrhages as arising from ruptured vessels. The globules of blood are too large for the "invisible pores," and hence this mechanical conclusion. The doctrine of sanguineous secretion would be fatal to the *porous* hypothesis. But how is it in

(1) Elements of Physiology, vol. i. p. 431. (2) Ibid. p. 253. (3) Ibid. p. 255.

menstruation, where the pores of the vessels are equally invisible? A rupture, or not? Is it compatible with our author's hypothesis, that "the serous membranes are, of all textures, the *most prone* to the effusion of the liquor sanguinis," and that "the reason of this is, perhaps, that they possess the least proper assimilating tissue?" (1) How do the red globules escape in these cases? Because they are not assimilated? How do the globules of lymph and the nuclei of the red globules escape from the vessels in the process of nutrition? Our author of course says they do not, and has an assumption to get rid of the difficulty. All facts, indeed, are thrust aside by this hypothesis. No globules can escape, and none can rëenter. What will the advocates of purulent absorption from open surfaces say? What will Mr. *Gulliver* say? What shall be said of the undeniable elimination of pus by the kidneys? Milk, (2) mucus, and the seminal fluid, must be formed after their elements reach the surface. Indeed, this is necessarily one of our author's assumptions: "Their globules could not have passed entire through the coats of the vessels." "The elimination, by the kidneys, of globules of pus *which had found their way into the blood*, is quite an impossibility." (3) But how did they "find their way into the blood?" Surely not by the "invisible pores; and Müller affirms that "no kind of globules can penetrate the walls of the capillary vessels." (4) It is, therefore, not remarkable that our author should "confess that the act of absorption in other parts, as well as in the intestines, is to me quite an enigma." (5) But he thinks "the presence of globules in the chyle might be explained even without the necessity of permeation of the coats of the lymphatics, *and without pores existing* (!) if Doellinger's hypothesis were adopted. Doellinger supposes that the *villi* of the intestines are constantly undergoing *solution* on their interior, so as to *form* the chyle of the lacteals, while they are reproduced on their external surface by the aggregation and apposition of particles from the chyle contained in the intestines," &c! (6) And all this has come of the microscope. But Müller states, that after removing a portion of a sheep's intestine, he "tied one extremity and strongly distended it with milk by means of a syringe, when the lacteals immediately became filled, and the milk moved very rapidly through them; (7) but "mercury cannot in this way be made to enter the lacteals at all." Again, "all good observers agree that there are no visible openings in the villi of the intestines." (8) Now, as to the conjecture that some part of the intestinal mucous membrane must be lacerated, this would never explain the admission of the milk into the lacteals universally, its exclusion from the cellular tissue, and the exclusion of the mercury. The experiment proves conclusively the existence of open vascular terminations. The mercury presses down the villi, and thus prevents its entrance. If laceration of the membrane took

(1) Elements of Physiology, vol. i. p. 415.

(2) Its globules said to be often twice the size of the sanguineous; and so of pus.

(3) Ibid. p. 257. Mr. Gulliver, by the aid of chemical and microscopical manipulations, affirms that "he has detected *globules of pus* in the blood in almost every instance where there had existed during life either suppuration or great tumefaction of the external parts without the presence of pus." We leave this question with those who maintain that the globules of pus are only formed after exposure to the air. Le Galois mixed a large quantity of pus with blood, but could find no trace of it afterwards. But Mr. Gulliver finds a wandering globule.

(4) Muller, ut cit. pp. 262, 278, 552. (5) Ibid. p. 280. (6) Ibid. p. 263.

(7) Magendie collected half an ounce of chyle from the thoracic duct of a middle sized dog in five minutes. — *Journ. de Physiol.* t. viii. (8) Muller, ut cit. p. 266.

(a) British and Foreign Med. Rev. vol. vi. p. 553.

place, it should certainly be effected by this substance; when it should flow even more freely into the lacteals than the milk, if this were the true philosophy.

It is remarkable that Müller, "after exposing the error of Rudolphi, who believed that the villi were devoid of blood-vessels," and after showing that they have not only been "beautifully injected," but that "he has seen them with the naked eye filled with blood,"—it is remarkable, that he should say in the next paragraph, that the "assertion of Bleuland and others, that the villi have an opening at their extremity, was refuted by Rudolphi," who merely asserts that "they have never any *visible* opening." (1) Is Rudolphi's testimony any better in this case than in the former, when the blood-vessels of the villi could be "seen by the naked eye?" But Bleuland's statement is confirmed by as accurate an observer of nature as ever flourished. R. Jackson, in speaking of the *enlarged* blood-vessels of the villous coat of the intestines, remarks that "in some instances, the *mouths* of the canals were visible at *different points* in the interior surface, yielding a dark coloured fluid by pressure." Again, upon a point where accuracy and veracity were deeply concerned, he says, "the mouths of ducts,—not blood-vessels,—were discovered on the interior of the colon, containing a dark coloured fluid." "Proceeding farther with the investigation, similar canals discharging a tar-like fluid into the interior of the stomach, more especially near the upper orifice, were in like manner discovered in *almost all cases* where black vomiting had been a conspicuous symptom of the disease. The appearances were noted, and they were often verified by inspection." (2)

Quite a dispute has also arisen whether there be any *cavity* in the villi, because it cannot be *seen*, notwithstanding they absorb the chyle, and Schwann injected them with quicksilver. But it turns out that Professor Krause, of Hanover, has *seen* the villi well filled with chyle in a young man who had been executed; and "the lacteals which issued from each villosity arose by several smaller branches, of which some terminated by a free extremity, and others anastomosed with each other." As to their open terminations we are not insensible of our solitary position; since it is stated that "almost every modern physiologist has now abandoned the idea that the absorbents commence by open mouths on the villous coats of the intestines." (3) This opinion we are certainly bound to respect, so far as it is supported by any facts, or is not contradicted by others.

But, at last, we have found what we had been looking for; an admission from our author of a fact which entirely subverts the assumption of closed vessels. "Brachet," he says, "divided the renal artery of a dog, and then connected its two portions by means of a canula, so that the *renal nerves* were divided, but the supply of blood maintained. The fluid which flowed from the ureter during several hours succeeding the operation was red, and separated into a *fibrous coagulum* and *serum*. Repetition of the experiment was attended with the same result." (4) This, then, is the *experimentum crucis*. Instead of urine, we have nothing here but a secretion of blood, consequent on an inflammation

(1) P. 268. Muller denies the communication of *lymphatics* with small veins. "If, however," he says, "I should ever *see* a direct communication of a lymphatic external to the glands with a small vein, I would acknowledge it as a thing *evident to the sight*;" &c. (pp. 273, 274.)

(2) Febrile Diseases, vol. i. pp. 79, 80, and *note*.

(3) British and Foreign Med. Rev. No. 9. p. 105. 1838.

(4) Physiol. Vol. i. p. 471.

excited by a division of the renal nerves. (p. 476.) There is no rupture by art, but a fair chance for nature. The experiment, too, settles the doctrine of capillary hemorrhage. Our author repeated this experiment; when, in one instance, he got the sanguineous effusion. It is also well known that cantharides often produces bloody urine, when it would be equally absurd to suppose that either the vessels of the kidneys, or of the bladder are ruptured. But, we want not the aid of experiment to settle this question. It is notorious, that bloody urine, and blood, *per se*, are discharged from the bladder in a great variety of diseases, and even epidemically. M. Salesse, a physician of the Isle of France, affirms that three fourths of the children of that island are sooner or later affected with hæmaturia.

Again, our author admits another corroborating fact, too common to be denied; viz: "Effusions of blood, vicarious of the menstrual flux, have certainly occurred." (1) Will any one believe that our author considers either the result of ruptured vessels? Certainly he does not. But, in respect to the menstrual secretion, he resorts to the physical doctrine of relaxation. In opposition to himself, he maintains that the red globules escape, which every one knows to be true. But then, "it must be supposed that, at the period of menstruation, the *texture of the vessels of the uterus becomes so* loose as to allow the escape of the red particles" (2)—through the "invisible pores." Is this physiology; or is it unmeaning hypothesis?

On some other occasions, our author throws aside the microscope, and reasons upon our own grounds. Thus, "the different *ducts* of glands," he says, "are lined by a mucous membrane, which has on its exterior an extremely thin layer of muscular substance. The existence of muscular fibres cannot, it is true, be demonstrated anatomically, (cannot be seen by the microscope,) but *physiological observations place it beyond dispute.*" (3)

After all, however, the denial of the open termination of vessels will appear less remarkable, when we consider that Dumeril, (4) Morand, (5) and even John Hunter, (6) and a hundred others, maintained that the leech has no *anus*, because they could not see it. The latter says it is an exception to the general law. Indeed we see it is stated by Müller, that "in the suctorious worms there is no anus," although they have an intestinal canal. (7) "Duplex autem est sensus culpæ; aut enim destituit nos, aut decipit." "Magno prorsus errore asseritur, sensum esse mensuram rerum." (8)

As to the denial of open orifices to the capillary vessels, because they cannot be seen, or injections will not pass, and predicating of this the necessity of physical results which are attended by phenomena that plainly declare them to be of a vital nature, it might be equally affirmed that there are no *pores*, since these cannot be seen, nor will injections pass, and therefore that all secretions, like effusions of blood, lymph, &c., are the result of rupture. The assumption, in all respects, is as sound in one case as in the other; and the argument from physical necessity is just as legitimate in respect to open terminations, as the "invisible pores." The utter uselessness of the microscope in deciding this question is especially shown by the general failure of observers to discover the

(1) Muller's Physiol. vol. i. p. 474. (2) Ibid. p. 256. (3) Ibid. p. 475.
(4) Zoological Anatom. p. 298. (5) Mém. de l'Acad. Royale des Sci. 1739, p. 355.
(6) Philos. Trans. Abd. vol. xvi. p. 5. (7) Op. Cit. p. 488.
(8) Bacon, Distributio Operis, t. i. p. 15.

orifices of the lacerated vessels when the brain is broken up by extravasations of blood, whilst, in other hemorrhages which our author ascribes to lacerated capillaries, observers have equally failed to discover the orifices. The failure of sense, and of mechanical experiments amount, therefore, to nothing. The operations of life are inscrutable and inimitable. The whole philosophy reposes upon other facts; and may we not ask, with Bichat, "what can we conclude from an isolated phenomenon, (especially a negative one,) which is in contradiction to all those which nature daily presents"? And may we not yet again affirm with him, that "the difficulty of distinguishing the orifices of vessels is no reason for denying their existence?" (1) And thus Dr. Davies: "it must be admitted that the functions of secretion, with a single exception (?) take place by means of the orifices of the capillary extremities of the arteries; but these orifices are so small that they are beyond microscopic powers to discover. We infer their existence by reasoning, but cannot prove it by demonstration." (2) But, what shows our philosophy to be correct is the demonstration, by M. Poiseuille, that water cannot be forced even along inanimate tubes of a small diameter; whilst others have only effected the injection of a yet larger series in the living subject by dividing the medulla oblongata.—(See p. 604, *note*.)

Our author is sensible of some of the obstacles to the foregoing hypothesis, and such as he cannot surmount by assuming ruptures, the conversion of the intestinal villi into chyle, &c., he anticipates by way of impairing their force. He "does not explain the power by which the secretion is thrown off from the secreting surface, but *merely the possibility* of the fluid finding its way through the coats of the vessels and membrane." He fully admits that the power of the heart is not equal to the latter effect; this being plain enough, since no mechanical force will produce the phenomenon. And "another difficulty to be solved is the cause of the escape of the secreted fluid *solely* on the *surface* of the membrane. Why does not the mucus, for example, collect as readily *between the coats* of the intestine as exude from the inner surface? Why does not the bile permeate the *walls* of the biliary ducts, and escape on the *surface* of the liver, as readily as it forces its way outwards in the course of the ducts? Why does the semen collect on the inner surface only of the tubuli seminiferi, and not on their exterior,—in their interstices? The elimination of the secreted fluid on one side only of the secreting membrane, namely, on the interior of the canals, is one of the *greatest enigmas* in physiology." (3) Our author attempts to solve this "enigma" by a "hypothesis," which he admits is "quite unsupported by facts." All these difficulties are owing to the supremacy of the microscope; whilst the "invisible pores" are only allowed an existence to avoid a physical absurdity. But, for our own part, we think the absurdities are in no respect diminished till we allow that the fluids traverse the full extent of the vessels, and finally issue from their open terminations. This, and this alone, will solve all the "enigmas" of the hypothesis.

The microscope has detected no absorbents in the brain, and some other tissues; but the phenomena of life and Unity of Design establish their existence more indisputably than might even the sense of vision. The same af-

(1) General Anat. vol. i. p. 341, and vol. 3 p. 150.
(2) Diseases of the Lungs and Heart, p. 327. (3) Muller, Ibid. p. 463.

firmation may be made of the communicating vessels betwixt the maternal and fœtal portions of the placenta; though this has been physically demonstrated by Chaussier, Beclard, Williams, and others. And the same of the vascular system of the serous membranes, the cartilages, ligaments, and tendons, in the natural state of which the microscope can detect no vessels; but which may present, through injections or inflammation, a splendid net-work of tubes.

APPENDIX III. TO THE HUMORAL PATHOLOGY.

(See pp. 611 — 613, 630, 625.)

FASTING.

"Nature well known, no prodigies remain,
Comets are regular, and Wharton plain:
Yet, in this search, the wisest may mistake,
If second qualities for first they take." — POPE.

IT is not our purpose to annoy the reader with a practical disquisition upon diet. We have far other objects in view. We may say, however, that we reprobate all ultraism in respect to food under the ordinary conditions of health, and salubrity of air; and we would extend this remark to the moderate use of wine, in temperate climates. Nay, if we may hazard the doctrine, we would allow to the northern labourer in the field that "dram of grog" which was formerly wont to invigorate the powers of nature, at the rich moment of enjoyment, beyond any expedient but protracted rest. In respect to food, we believe that man is omnivorous, and may indulge himself with the bounties of nature which are *naturally* within his reach. The error lies in too great a quantity and too great a variety at each repast. But, it is obvious that nature has placed a certain restraint in some climates, whilst in others she allows the greatest latitude. She has planted but few animals in the tropics, no vegetables in the arctic regions, and has crowded the temperate with both kingdoms. And how well has the experience of man demonstrated the final cause of this dispensation. Contrast the great and common longevity of the black man of the tropics, who lives wholly on the fruits of the soil, or the equally abstemious white man, with the miserable victims of disease and an early death who addict themselves to stimulating food, and stimulating liquors. (1) On the other hand, we are assured by Dr. Aikin and others, that the hyperborean can subsist only on animal food. But, in zones between these extremes, everything

(1) "Not only the caution of individuals, but the habits of nations," says our philosopher Miller, "may be distinguished in the comparative exemption from diseases, which they derive from abstemiousness. The French and Spaniards of the West Indies, and in other warm climates, avoiding the use of ardent spirits, and retaining their usual habits of thin and spare diet, are observed remarkably to escape the dangers incidental to such situations; while the British, carrying with them, wherever they go, not only their plethoric and vigorous habits, but likewise their national predilection for a gross and stimulating plan of living, suffer all the havoc of those baneful countries." (a) And so Annesley in his Diseases of India, R. Jackson, and all other enlightened observers. Maddox relates, that few of the grave stones at Jamaica record an age beyond 40 years; (b) and we had the curiosity to confirm this observation. Moseley remarks that "the ridiculous notion that people are to die of putrid diseases in hot climates unless they keep up their spirits, and embalm their bodies, by the assistance of an additional quantity of wine, strong liquors, and living well, as it is vulgarly called, has caused the death of thousands." Here, "*sub curvo nimium propinqui solis*, the mind, enervated with the body, is roused to short lived actions, by efforts that cannot last; and sinks again under the oppression of climate, to which all things in nature yield." (c)

(a) New-York Med. Repository, vol. i. p. 189. (b) Twelve Months' Residence in the West Indies.
(c) Moseley, Ut Ck. pp. 98, 199.

abounds, and most things are salubrious. Nor is this owing to any remarkable difference in the constitution of man in different parts of the globe. It grows out of the temporary influences of climate, and the difference in the exciting causes of disease, which operate according to the artificial susceptibilities of the vital forces. The moment the northern or the tropical man enters the temperate climates, he may comply at once with the more liberal provision of nature. And what farther shows that man differs *naturally* but little in his constitution, in a general sense, is the fact that he is everywhere liable to many diseases of a common, however modified, character, and which equally require common methods of treatment.

The effects, however, of any great departure from the foregoing law of nature may be slowly developed, especially in temperate countries, where man appears to be left to select from the profusion around him. Here it is that we shall find endless examples to convict the humoralist of error in imputing disease, in times of scarcity, *primarily* to an "impoverished state of the blood."

We have already noticed extraordinary instances, on a large scale, of the protecting power of abstinence in times of great epidemics; and we confess our astonishment, considering that the blood is the *pabulum vitæ*, and derived wholly from our *ingesta*, and the habit of the stomach in relation to food, that extreme privation is so very rarely a direct cause of disease. It is this principle about which we are, at present, alone concerned; and shall, therefore, take no farther part in arguing the truth of our introductory remarks. We will say, however, that observation prompts the belief that early habit may reconcile the constitution of man to a very limited variety of food in temperate climates, even to a diet exclusively vegetable; but, that it appears to be equally true that a sudden change under circumstances of health, from a mixed to vegetable aliment, is, in a general sense, a predisposing cause of disease. It is mainly our object, however, to show that abstinence, *per se*, is rarely a *direct* cause.

We shall state a few instances which exemplify the error of humoralism. Schmaltz relates a case where a female, aged 41, took no solid food for six years, and very little fluid nourishment. Ricci, of Turin, records another instance where a female lived two and a half years in the same way.(1) Ponteau mentions a case where a young lady lived exclusively, for 18 months, upon syrup of adiantum, and during the time grew two inches and a half.(2) We, ourselves, had a patient, (Capt. Porter of the United States Army,) who lived exclusively upon arrow-root and water for nearly a year. The quantity taken daily was less than a quart. The diet was continued unnecessarily, without our knowledge, for several months. The patient had been affected with chronic gastritis, which had reduced him excessively in flesh; but he was always able to take a large amount of exercise. The effect of the diet was an immediate improvement of health, and the patient became a fleshy, robust man under its use;(3) and this was his reason for the long adherence to

(1) Journ. de Prac. Heilkund. 1828.

(2) Œuvres Posthumes. See, also, Voltelen's Memorab. Appositiæ Septennis Hist. and Boccarius in Comment. Institut. Bonon. t. ii. p. 1.

(3) This case exemplifies the power of habit, and how a low diet which might produce emaciation and great susceptibilities of the system, if begun in health, may be continued with impunity after health is restored, if the diet be commenced in sickness and be suitable to the condition of disease. The

it. We have a son, now 10 years of age, who subsisted exclusively upon human milk, extracted by the breast pump, and whey, till two and a half years old, and for five years afterwards upon arrow-root with water, during which time he consumed some hundreds of pounds. From having been the constant subject of very dangerous disease, and existing in the state of a skeleton, he emerged from that condition, and became robust and fleshy whilst living upon the arrow-root. He is now very stout, eats a variety of vegetables, but has had no animal food. (1) In these instances, however, the abstinence was begun when nature demanded it, and the power of habit enabled the system to bear the long privation after health was restored. The facts are concerned about important principles in physiology which are dark to the humoralist.

There are many cases of extreme abstinence mentioned in the Philosophical Transactions, for which an allowance must certainly be made, though they are said to be authenticated by many witnesses. Thus, Dr. Blair states that a boy, aged 15 years, took no kind of food for three years. In another, by Dr. Mackenzie, a woman is said to have lived "without the smallest particle of food for nine years." We may safely conclude that the abstinence was very great. In another, by Mr. Campbell, a man lived 18 years exclusively upon clarified whey and water. (2) Sir William Hamilton witnessed the case of a girl, aged 16, who was buried for 11 days under the ruins of a house at Oppido, whose health was in no respect impaired. (3) In the same work (4) is an account of four colliers, who were driven into a recess of a mine by springing a vein of water, where they remained safely 25 days "without a morsel of food." They had water, but its analysis showed nothing "but a scarce perceptible calx." "Mary Thomas, who is now 84 years of age, has been confined to the bed 63 years; and during this long period has lived nearly without eating or drinking. For 10 years she was supported absolutely without food of any kind." (5) The case of Ann Moore is well known. (6) Whatever imposition may have been practised, either in this, or in any of our other examples, there can be no doubt that there was a degree of abstinence sufficient for our purposes. Hasselquist states that a caravan of 1000 Abyssinians subsisted two months on gum Arabic alone. (7) Adanson says that the Nomade Moors live almost exclusively on gum. (8) So, Humboldt, that the Ottomaques, on the

same philosophy teaches us that a full liver would be in danger of disease, were he to make any great and sudden change of diet under circumstances of health.

(1) Before Robert Jackson succeeded in substituting the antiphlogistic for the stimulant treatment of fevers, in the British Army, the mortality was, of course, excessively great. But this was officially charged to his having lessened the rations "of the sick soldiers, which was a pound of beef more, daily, than for a soldier on duty in barracks." "He was considered, by certain persons, meddling and officious; and although his reforms were great improvements, they made him many enemies." By diminishing the allowance of "salt beef and rum" to the sick, he saved the British Government nearly $400,000 per annum. "He was a man of great talents, extensive learning, great philanthropy, disinterestedness, and humanity." (a)

We must go back to Hippocrates, Celsus, &c. on the subject of diet in the treatment of disease.

(2) Vol. xxxi. p. 28. Vol. lxvii. p. 1. Vol. xlii. p. 240. See other remarkable cases in Acta Eruditorum, Dec. 1, t. ix p. 178. D. 3, t. iv. sup. p. 259. D. 5, t. vi. p. 272, and t. x. sup. p. 226. Also, Hildanus Op. Cent. 4, ob. 30.

(3) Philos. Trans. vol. lxxiii. p. 169. (4) Ibid. vol. xiv. p. 577.

(5) See London Med. and Phys. Journ. vol. xxi. p. 96.

(6) Ibid. p. 61, and vol. xx. pp. 402, 527. (7) Voyages in the Levant, p. 298.

(8) Mém. de l'Acad. des Sciences, 1778, p. 36.

(a) Dr. Barnes's Biographical Sketch; in Trans. of the Provincial Med. and Surg. Association, vol. iii. pp. 413, 419, 429, 430.

Orinoco, live upon pipe clay, getting rarely besides a small fish, or a fern-root. (1) Labilladdier affirms the same of other savages. (2) The Nova Scotians feast upon steatite, in which Vauquelin could detect no organic matter. (3) They therefore manifestly got something else. In a famine at Degernä, in 1832, Retzius found that the inoffensive food of the inhabitants was composed of silicious earth and organic particles. (4) In New Zealand, the fern-root furnishes a principal part of the food of the common people, at some seasons of the year. (5) In California, "the acorns, which abound, constitute the principal food of the Indians;" (6) and the inhabitants of the Atlas mountains, between Algiers and Morocco, "subsist upon acorns." (7) In New Holland, three millions of natives are sometimes limited, with impunity, to "grubs and reptiles taken in the hollow of decayed trees of the forest." (8) Millingen thinks that "a lower scale of diet is by no means as injurious as it is generally supposed. The English prisoners made by Tippoo Saib, though kept upon a scanty pittance of bread and water, found themselves in *better health than before*, and some of them were cured, during their captivity, of liver complaints of long and severe duration. (9) We also learn from this writer, as well as from others, that in times of scarcity the Germans have subsisted comfortably upon the wood of beach, birch, lime, poplar, fir, and various other trees, "which may be converted into a very palatable article of food." "In the monastery of Macham, in Egypt, a monk of the name of Jones was beatified for having lived until the age of 85, working hard in the garden, without any other food than raw herbs and grass steeped in vinegar." "St. Hilarius only ate fifteen figs and six ounces of barley-bread *per diem*. St. Julius Sabus retired to a cavern, where he only luxuriated once in the week on millet-bread, with salt and water; and St. Mecarius resolved to outdo him by restraining his sustenance to a few cabbage leaves every Sunday. Not only did these gastric martyrs attribute their holy visions to abstinence, but they considered it as the source of their longevity. (10) The annals of the monastery are full of similar examples. Cornaro's abstinence and longevity are well known.

But, does abstinence, amounting to fatal starvation, produce sensible lesions of disease? Dissections have been rare in these cases; but such as have been made are utterly fatal to the humoral hypothesis. There are enough where patients have died from diseases which abstinence, and other means would not cure, and where the lesions have been imputed to the most indispensable remedy. But we must look for our examples in the healthy subject; where the humoralist points us.

"Opportunities," says Haller, "of dissecting persons who have died of hunger very seldom occur. Yet, in 1728, I saw a man in low circumstances, middle aged, tall, corpulent, and of a very good habit of body, who wilfully starved himself to death in a stable. *All the parts were entirely sound;* but the stomach and intestines were perfectly empty, without so much as the least remains of excrement; a most extraordinary circumstance, seldom or never met with in bodies emaciated under the most lingering diseases." (11)

(1) Tab. Phys. des Regions Equator. &c. (2) Voyage chercher le Peyrouse, t. ii. p. 214.
(3) Humboldt, Op. Cit. (4) Poggendorf's Ann. B. 29, p. 261.
(5) Ellis's Polyn. Resear. vol. i. p. 34. (6) Beechy's Voyage to the Pacific.
(7) Desfontaines in Bull. Univ. 1830, p. 469. (8) Ellis, ut supra.
(9) Curiosities of Med. Experience, p. 16. (10) Ibid. pp. 79, 93.
(11) Patholog. Obs., Ob. 24.

The following is a coincident case by our able Professor M'Naughton, of Albany. Joseph Kelsey lived on water alone fifty-three days. "For some weeks he had eaten very little; but on the 2nd of July, he declined eating altogether; assigning as a reason, that when it was the will of the Almighty that he should eat, he would be furnished with an appetite." "His parents think that the quantity of water he took in twenty-four hours did not exceed, if it equalled, a pint. On one occasion, he went three days without taking even water." "On the 11th day of his fast, he replied to the expostulations of his friends, that he had not felt so well, nor so strong, in two years, as at that moment, and consequently denied the necessity of taking food. For the first six weeks he walked out every day. His walk was steady and firm, and his friends even remarked that his step had an unusual elasticity. He shaved himself until about a week before his death, and was able to sit up in bed to the last day," "and to go about the room." "His mental faculties did not seem to become impaired as his general strength declined; but, on the contrary, his mind was calm and collected to the end." "During the first three weeks of his abstinence, he fell away very fast, but afterwards he did not seem to waste so sensibly." "His general appearance was so ghastly, that children were frightened at the sight of him." (1)

The following is a different example; the effects of starvation being connected with an organic lesion of more than four years duration, as known by "a circumscribed hardness of four or five inches in diameter," and other symptoms. "During the last nine weeks," says Dr. Plympton, "we were unable to procure any alvine evacuation, or to administer food in any form whatever." "She remained able to converse with her friends to the last moments."

Dissection was performed 24 hours after death, "in the presence of a number of medical gentlemen, which revealed *no indications of disease*, except in the abdominal viscera." There was no ulceration of the mucous coat of the intestinal canal. "The omentum was wholly consumed;" and the residue of the morbid appearances consisted mainly in old adhesions. (2)

Prof. Alison attributes any slight accidental appearances of disease after death from starvation to causes which have no connection with the direct effect of fasting. (3) Finally, it is stated by Dr. Beatty, as the result of his researches, that "the eyes are open and red, the tongue and throat dry, and the intestinal canal is empty. The gall bladder is usually filled with bile; the lungs are shrivelled, and all the other organs are *healthy*." (4)

So much, then, for the argument in favour of humoralism as drawn from the effects of starvation. The vital forces are undoubtedly reduced to a state of great susceptibility; and slight morbific causes, even excess in eating, will readily induce disease. But it appears that abstinence, *per se*, is not a morbific cause. This is farther shown by the speedy restoration of those who have suffered extreme abstinence as soon as a supply of food is obtained. Privation of water, however, especially when accompanied by that of food, has probably a tendency to produce gastric inflammation.

Secondly; we learn from the foregoing facts that the lesions which occur in

(1) Transactions of the Albany Institute, No. 4.
(2) Boston Med. and Surg. Journ. July 3, 1839, p. 333.
(3) See Outlines of Physiology, &c. p. 178.
(4) Cyclopædia of Prac. Med., Art. *Persons found Dead*, p. 338.

disease, where abstinence has been employed as a curative means, have been erroneously imputed to this important remedial agent. Nine instances of gangrene of the lungs are said to have arisen from *voluntary* abstinence in M. Guislain's hospital.(1) So, Dr. Williams affirms that it is the tendency of tartarized antimony, "as far as his observation goes, to produce mortification of the lungs."(2) So, too, it is affirmed that pulmonary gangrene "seems to arise pretty generally from the influence of noxious gases, which *directly destroy* the vitality of the tissue of the lungs," &c.(3) It is important, however, to say that this disease is probably of very rare occurrence, and is often confounded with other degenerations of the lungs. The fœtid odour is assumed as the principal diagnostic mark;(4) but this often attends the morbid secretions of those organs when they have been a short time in contact with the air.(5)

Thirdly; that the experiments of starving animals to death are entitled to no confidence in their application to the human species; nor are those in which the animals were restricted to a simple substance. Redi made a great number of these experiments; the animals all dying, when they were sufficiently

(1) Gazette Méd. de Paris, Jan. 16, 1836. (2) Elements of Med. vol. i.

(3) Dr. Williams on Diseases of the Chest, Lec. 19.

(4) See Stokes, on Diseases of the Chest, p. 236; — Bright's Medical Reports, vol. i. p. 136; — Morton's Illustrations of Pulmonary Consumption, p. 108, &c.; — Williams's Exposition, &c. of Diseases of the Lungs and Pleura, p. 97 — and Gerhard, *ut infra*. The odour, however, is sometimes wholly wanting. (See Morton, *ut supra*.)

(5) Laennec very rarely saw or heard of pulmonary gangrene; and Dr. Forbes "never met with a case, nor can he refer to one recorded in our medical literature, the nature of which was unequivocal." (*a*) There is not one recorded by Bonet (*b*) in 620 Observations of pulmonic disease; and we find but one or two, and those doubtful, in Morgagni. Louis nowhere speaks of the affection. (*c*) Morton has but three cases, and appears to have reported all within his knowledge. (*d*) All that we have from Sir J. Clark is, that "portions of the walls of an abscess are occasionally found in a state of mortification." (*e*) Bright records only three, or at most four, "unequivocal cases." (*f*) Williams says "gangrene is a rare termination of Peripneumony." (*g*) Hodgkin mentions a case which was attended by the extremely unusual phenomenon of gangrene of the pleura. (*h*)

On the other hand, it is proper to say, that Dr. Davies "has met with 14 or 15 cases of gangrene of the lungs." (*i*) Boerhaave and Van Sweiten speak in a loose way of pulmonary gangrene; (*k*) and it has appeared with unusual frequency at the Philadelphia (Blockley) Hospital. "Within the last nine months four cases have presented themselves." (*l*) Gerhard also speaks of an "unusual number of cases of gangrene of the lungs at the Philadelphia Hospital in the winter of 1835—6." This able observer has met with ten cases. (*m*) See, also, an account in the Medico Chir. Rev. Lon. (*n*) of two cases, emphatically entitled "*genuine* gangrene of the lungs." In 222 cases of puerperal fever, Tonnellé (*o*) found gangrene of the lungs in three, which had supervened on inflammation of those organs. It is manifestly most apt to arise in malignant fevers, attended with pulmonic inflammation. It is said to be frequent in these affections, by Eisfield, (*p*) Lind, (*q*) and Sabater and Ramas. (*r*) A remarkable case was noticed in the "Spotted Fever" of our New-England States. (*s*) In a malignant fever at Normandy, Dr. Moucet met with a case in which both lungs were gangrenous. (*t*) Stokes relates four cases, two of which are republications, and grew out of rum. (*u*)

(*a*) Laennec on Diseases of the Chest, pp. 227, 237.

(*b*) Sepulchretum. On examining l. ii. s. 1, obs. 23 and 25, and s. 7, obs. 3, 38, and 49, it will be seen that there was no gangrene, as might be suspected from the index.

(*c*) See his Works on Phthisis, and Typhoid Fever. In the typhoid disease, "the lungs were affected in a greater part of the patients." (*d*) Illustrations of Pulmonary Consumption, p 14, 102, 106. 1834.

(*e*) On Pulmonary Consumption, p. 103. (*f*) Medical Reports, vol. i. Cases 47, 48, 49, 50.

(*g*) Rational Exposition of Diseases of the Lungs and Pleura, p. 97.

(*h*) Lectures on Morb. Anat. of Serous and Mucous Membranes, p. 135.

(*i*) Lectures on Diseases of the Lungs and Heart, p. 178.

(*k*) Van Sweiten's Commentaries, &c. vol. viii. s. 844, p. 261 — 264.

(*l*) American Med. Intelligencer, vol. ii. p. 136. 1838.

(*m*) Amer. Jour. of Med. Sciences, vol. xix. p. 294, and vol. xviii. pp. 313, 322. (*n*) July, 1836, p. 221.

(*o*) Des Fievres Puerp. a la Maternite, 1829, in Archiv. Gen. 1830. (*p*) Essay on the Typhus of Leipsic, 1759.

(*q*) Diseases of Hot Climates, p. 125. (*r*) Berthe, Precis de la Malad. d'Andalusie.

(*s*) New-England Med. and Surg. Jour. 1814, p. 212. (*t*) Vandermonde, ix. p. 70.

(*u*) On Diseases of the Chest, p. 236, &c.

starved. (1) "They were cruel and unjustifiable," says Dr. Percival. (2) It was an early blot on physiological inquiries. Laurenzo, (3) Bautigny, (4) and others of their day, employed themselves in these acts of inhumanity. In later times, that work has been mainly carried on by M. Magendie; and,

"All with one consent, praise the new gawds,
Though they are made and moulded of things past."

But whilst he produced *sore eyes* in dogs by feeding them with bread and water, Celsus, who commends good living, says "there is more nourishment for man in bread, than in any other food;" (5) and in this Galen, (6) Ægenita, (7) Lusitanus, (8) Wainewright, (9) &c. agree; and in our day, it has acquired the designation of "the staff of life." Half the French nation get nothing else, and two thirds of the Irish nothing but potatoes and whiskey. True, a diet of bread may give sore eyes, or something worse, to cats and dogs, since it is an absolute violation of their natural habits. We should "defalcate such anomalies," instead of carrying them up by way of analogy, like our author and others, to omnivorous man. The dogs also got *sore eyes* when he fed them with *sugar*, or *butter*, and starved to death from the 30th to the 36th day. Some of them held on a little longer. It is therefore argued because cats and dogs cannot subsist on sugar or butter, bread and water must be fatal to man. It was also an inference, "je considerai des lors le sucre comme incapable seul de nourrir les chiens." (10) Tiedemann and Gmelin repeated Magendie's experiments upon geese; restricting one to sugar and water, another to gum and water, and a third to starch and water. Here, the food being utterly unnatural, all the geese gradually starved to death; though we hear nothing about *sore eyes*.

"———— And thus we spend
The little wick of life's poor shallow lamp,
In playing trick's with nature. ————"

It is curious that Stark (11) subjected himself ~~and his fellow-man~~ to a diet of sugar and water for many weeks; and, as might be anticipated from so indigestible a substance as concentrated sugar, it inflicted disease upon his stomach, and ultimately upon his whole system. This is the distinction betwixt total abstinence, and a substance irritating to the digestive organs. In one case there is a morbific cause, in the other there is none.

Although man, in a general sense, is omnivorous, and may therefore make out tolerably well upon bread and water, as the infant does upon milk, there are some things which he can no more digest than dogs. But almost all animals are limited in this respect, being, indeed, generally adapted only to food either from the animal or vegetable kingdom. Dogs and cats are carnivorous, though the slow influence of habit will enable them to bear a proportion of vegetable food. But confine them exclusively to this species, and it will inflict

(1) Osservazioni interno agli Anim. Viventi, No. 3, 4.
(2) Mem. Manchester Philos. Socy. vol. ii. p. 475.
(3) Disceptatio an possit Homo naturaliter diu absque cibis vivere, 1652.
(4) Ergo diu potest Homo absque cibo et potu vivere quidem, sed non valere, 1750.
(5) L. 1. c. 1, p 20. and l. 2. c. 18. p. 97. (6) L. 1. de Aliment. cap. de tritico.
(7) L. 1. de Remed. c. 73. (8) Prax. Hist. l. 2. c. 12. (9) On the Non Naturals, p. 162.
(10) Précis. Phys. pp. 390, 393, &c.
(11) Experiments Dietetical and Statical, 176. Magendie's experiments with sugar and water have been lately repeated by Dr. Marchand; and, although the dogs became "very thin," there were no sore eyes. Lon. Med. Gaz. June 22, 1839, p. 477.

disease upon the organs of digestion, and by sympathy, give them "sore eyes," &c. It is manifest, also, that the whole philosophy of this subject is adverse to the humoral pathology. The proper experiments would have been to have fed the dogs exclusively with simple fibrin, and fowls with some species of grain; and who will doubt that they will not then thrive? This, indeed, is fully shown by comparing two of Magendie's experiments. Thus, he fed an ass on dry rice, and then exchanged to boiled rice. The food not being suited to the nature of the animal, he died, either from that or some other cause, in fifteen days. But, on the contrary, he fed a cock with boiled rice for many months, and here the food being natural, the cock grew fat. Pp. 512, 514, 515.

Again, these experiments proceed upon the supposition that there is no difference betwixt a man and an ass. But a difference is at least established by the fact that one lives on meat and the other upon hay. The physiological distinctions, as we have endeavoured to indicate in another place, betwixt man and animals are almost as great as betwixt reason and instinct. They should lead us to distrust the applicability to man of physiological observations upon animals. They have been the fruitful source of important errors. We cannot, indeed, reason with any certainty from animals of one genus to those of another. Peculiarities of organization and of constitution are constantly diversifying the results of the same experiments; (p. 650, *note*,) and of this we shall see something in our Essay on Animal Heat. Sir A. Cooper tied simultaneously the carotid and vertebral arteries of a rabbit, when death occurred almost on the instant. But a dog survives the experiment. (1) M. Piorry, who made experiments on dogs, and reasons analogically in these cases with Magendie, finally states, that "an extremely low diet will kill a dog in twenty-five or thirty days; yet a man will bear this abstinence for months." (2) Dr. Stevens relates that he saw a large rattle-snake fat and frisky that had not tasted food for nine months. (3) Some plants are a deadly poison to man which are food for certain animals, yet fatal to others. Arsenic has but little effect upon a cat. Every poisonous herb has its peculiar insects, &c. Will it therefore follow, because air when injected into the jugular vein of animals may be less fatal than has been supposed, according to Magendie, Blunden, (4) Nysten, &c. that it will not immediately destroy a man?

We conclude, that the whole reasoning upon the foregoing subject, extending as far as the curative effects of abstinence, has grown mainly out of the neglect of a proper consideration of the forces of life.

(1) Guy's Hospital Reports, vol. i. p. 473.

(2) Collection des Mémoirs, &c. p. 273. "La diète absolue est mortelle pour un chien du vingt,—cinqiéme au trentiéme jour, et on la fait subir des mois à l'homme malade.'

(3) On the Blood, p. 35.

(4) Blunden in Med. Chir. Trans. vol. ix. See, also, Bouillaud, in Bulletin de l'Acad. Roy. de Méd. t. 2. Velpeau, in Gaz. Méd. 1838. Cormack's Inaug. Dis. on the Presence of Air in Circulation, 1838, and Hunter on the Blood, and his Lectures on Surgery, lec. xi.

APPENDIX ON THE MICROSCOPE.

[See pp. 209, 544, 686, vol. i. ; and Essays on Inflammation, and Spontaneous Generation, vol. ii.]

"Whilst thou so modest, (wonderful to tell!)
On *lunar trifles* art content to dwell;
To bid the vermin on the journals crawl,
Hop, jump, and flutter, to amuse us all;
* * * * * * *
In short, delighted with the world of *little*." — PINDAR.

"All philosophy is founded on these two things, viz. a great deal of curiosity, and very bad eyes; and thus it happens, that a true philosopher passes his life in not believing what he sees, but in labouring to guess what is altogether beyond his sight." — *Fontenelle, Pluralité des Mondes, Conver. I.*

"It may be remarked that, to whatever science or object physicians particularly directed their attention, this they generally interwove in their theory of diseases. The anatomist pretended, that by dissecting the smallest minute fibres, he should reach the cause and recess of disease and learn the method of cure." — *Black's Historical Sketch of Medicine and Surgery.*

"Why has not man a microscopic eye?
For this plain reason, man is not a fly.
Say what the use, were finer optics given
To inspect a mite, — not comprehend the heaven?" — POPE.

THE microscope having been extensively employed in the interpretation of vital phenomena, and now threatening more than ever a subversion of physiological science, it is not only important as it regards the true theories of secretion, and of inflammation, but to many other conclusions at which we have arrived through the phenomena of nature, that we should briefly consider the claims of that instrument to the consideration which has been assumed in its behalf.

The objection to microscopical investigations rests upon a variety of premises; upon the minuteness of the objects, upon the opposite results of different observers, and upon the conclusions that have been drawn at variance with the phenomena of life, and even the laws of physics. (See APPENDIX on *Endosmosis.*)

"Microscopical deceptions," and "microscopical phenomena," have become technical terms amongst those who employ the instrument most. Whoever will follow the observers in regular order will find that each has but little to approve in the discoveries of his predecessors. Each, indeed, has the modesty to ask the world to believe that he alone is right; many affirming that their instruments are better than others, or that their habits of observation are more accurate, and admitting, at the same time, that the microscope is worthy of little trust excepting as employed by themselves. The world had become, for a long time, obtuse to these speculations; but not till credulity had passed its ordeal. The well disciplined generation, however, has passed away. The pre-

sent is composed of novices in this art of renown, and its aspirants have become more than ever numerous; and this, because the sober objects of sense are more than ever exhausted. There is, probably, no radical cure for these evils, any more than for animal magnetism. They are ultimately seen and felt; but, when laid at rest for a generation or two, they again spring up to awaken new admiration, that is again blighted by disappointment, chagrin, and a loss of intellectual reputation.

"Philosophers and physiologists agree that the sense of *seeing* is the source of a thousand illusions." (1) How greatly, then, may these "illusions" be multiplied by the "deceptions" of the microscope! We shall notice some of the most important collisions, and the adverse opinions of profound philosophers.

In the first place, a word as to one of the important subjects to which this inquiry is an appendix. We have already seen that different observers have arrived, by the microscope, at opposite conclusions as to the state of the circulation in the instruments of inflammatory action. Marshall Hall has abandoned all confidence "in experiments of any delicacy upon the circulation in the mesentery;" (2) and Müller, who is certainly as much for artificial as natural results, says that "the direct experiments, to determine the action of different substances on the capillary vessels, have left our knowledge of the subject in the greatest confusion." (3)

Hodgkin, Lister, and many others, discover that the ultimate structure of the primitive tissues is fibrous. They reject the theory of globules as having "its sole foundation in an *optical illusion*." (4) "Whether," says Meckel, "the muscular filaments are or are not formed of globules, are they solid or hollow? This question has been answered in one way, and sometimes in another; but *always in accordance with some theory*." (5) Valentin unites both hypotheses; affirming that the muscular fibres are composed of globules in the young embryo, but, that the fibres ultimately become uniform cylinders. (6) Monro, as is well known, discovered that "the nerves of all the structures of the body are composed of fibres, *enclosing spiral tubes*, which run zigzag from one side to the other, not unlike the winding of the seminal ducts. After puzzling *all* the learned of his acquaintance, and the first practical, as well as speculative, opticians in Europe, *he discovered* 'that the appearance of these convoluted fibres was *an optical deception*.'" (7) But this was not all. He had also discovered exactly the same convoluted fibres in wood, and even in *inorganic substances*. Here, then, were objects of incomparably more appreciable size than thousands that are now defined with the greatest display of accuracy. "There are animalculæ of such minute size, that their entire bulk is not equal in volume to that ascribed by the advocates of the globular theory to one of their integrant or primitive molecules." (8) And yet those animalculæ are dissected; their stomach, liver, generative organs, nervous system, &c., all unfolded. Nor is this all. A multitude of new species of vermin are discovered in various tissues of the body; some observers developing them in the muscular, cellular,

(1) Magendie in Bichat's Rech. sur la Vie, &c. p. 192, *note*.
(2) On the Powers which Circulate the Blood, p. 71.
(3) Elements of Physiology, vol. i. p. 298.
(4) Hodgkin's Lectures on the Morbid Anatomy of the Serous and Mucous Membranes, p. 96.
(5) General and Patholog. Anat. vol. i. p. 302. (6) Hist. Evolut. Syst. Musc. Prolus. 1832.
(7) Edin. Med. and Surg. Jour. No. 58, p. 121. (8) Hodgkin, op. cit. p. 27.

cutaneous, and other structures; others in the urine, semen, &c.; whilst others, like M. Donné, seek their burrows in venereal chancres, or in the vaginas of chlorotic maids;

> "Which West shall paint, and grave Sir Joseph Banks
> Receive from thy historic mouth with thanks:"

Thus converting the most complex beings into laboratories of an inferior race, whose original existence requires as much an Act of Creative Power as the most perfect organization; (1) or, as Butler has it, —

> "Learned men oft greedily pursue
> Things that are rather wonderful than true."

"Sir E. Home's alleged discovery of nerves in the placenta," says another, "appears to be mere imagination." (2) Müller, and many others, deny the existence of orifices to blood-vessels, against the laws of physics; whilst they admit the existence of pores, from which the fluids are supposed to escape, though they are said to be equally "invisible." (See APPENDIX on *Endosmosis.*) — Then follows, upon this hypothesis, the necessity of ruptures whenever blood is effused, though the ruptured vessels cannot be detected; and other inductions which are contradicted by the phenomena of nature.

> "Strange tidings these to tell a world, who treat
> All but their own experience as deceit!"

Next, as to the globules of human blood. They have been regarded by Leuwenhoeck, and others, as perfect globules. De la Torré (3) considered them rings; Dr. Hunter thought them a little flattened, and Hewson (4) "as flat as a guinea." Müller (5) says they are never globular, but flattened in all animals. In this many others agree. Professor Amici (6) represents them after this fashion, ; being of different, and inexpressible shapes. This discovery was effected by "one of the most powerful and distinct instruments ever made," and was considered worthy of being announced, in behalf of the Professor, by his Royal Highness the Archduke Maximilian. Regal influence, however, is powerless in matters of science; and, —

> "They cursed Sir Joseph Banks, in quest of fame,
> At finding fleas and lobsters not the same."

Some observers have denied the existence of sanguineous globules. Babington, and others, endeavour to establish the "homogeneous" nature of the blood, which, of course, excludes the globules. Raspail (7) says the supposed globules are nothing but precipitations of albumen. Leuwenhoeck affirms, "id solum dicere habeo, quod globulos quosdam sanguineos, sed perpaucos, visu percepe-rim." (8)

And, then, as to the size of the human globules, Leuwenhoeck (9) makes them

(1) See APPENDIX on *Spontaneous Generation*, vol. ii. (2) Ingleby on Uterine Hemorrhage, p. 73.
(3) Philos. Trans. 1765, p. 252, &c. (4) Ibid. 1773, p. 303, &c. Also, Exp. Enquiry, c. 1.
(5) Elements of Physiol. vol. i. p. 99. (6) Edin. Med. and Surg. Jour. No. 50, 1819.
(7) Chimie Organique, p. 900, &c. (8) Philos. Trans. vol. xxxii. p. 341.
(9) Philos. Trans. 1674, and 1684, pp. 341, 436, &c. Also, Anat. et Contemp. p. 35; and Edin. Med. Essays and Obs. vol. i. p. 277, 1746. Leuwenhoeck assigns 7,301,384,000 red globules to a globe of one inch in diameter, whilst late observers reduce the number to about 10,000,000 to a square inch. It is also well known that L. supposed the red globules to be composed of six serous ones, of a yellow colour.

$\frac{1}{1,000,000}$ of a grain of sand; Kater(1) $\frac{1}{4000}$ to $\frac{1}{5000}$ of an English inch; Bauer(2), $\frac{1}{1700}$; Müller (3) $\frac{1}{4000}$ to $\frac{1}{3037}$; Young (4) $\frac{1}{5000}$; Mayo (5) $\frac{1}{3000}$; Hodgkin (6) Lister (7) $\frac{1}{3000}$; Home (8) $\frac{1}{1700}$; Prevost and Dumas $\frac{1}{3000}$; Weber $\frac{1}{4000}$; Wagner $\frac{1}{4000}$; Wollaston $\frac{1}{4000}$; Edwards $\frac{1}{3000}$; &c. The estimates are generally precise; but the latitude which Müller has given will reconcile the differences of only three observers. Even Young complains that, "hitherto the measures of the particles of blood, which have been considered by various authors as the most accurate, have differed no less than in the ratio of two to five, and there is an equal degree of uncertainty respecting their form, some admitting Hewson's opinions, and a greater number rejecting them." (9) Prevost and Dumas (10) find the red globules of the fœtus double the size of those of the adult; and this poor foundation is considered, by some, sufficient to settle in the negative the great physiological question as to the vascular communication of the placenta. Müller's English editor remarks, that "the observations of de la Torré are unworthy of notice"; and that "the statements contained in the memoires of Sir E. Home and Mr. Bauer, on this subject, were nearly all erroneous," who, "unfortunately, were too willingly credited." But we certainly have no other proof of their error than what arises from the nature of the subject. Other observers, indeed, as Prevost and Dumas, it is said by the editor, "in some respects more nearly coincide with the statements of Sir E. Home and Mr. Bauer, and here they are obviously in error." (11) Dr. Elliotson, a good authority when referring to the numerous contradictions respecting the globules of blood, remarks, that "one takes breath while reading M. Raspail, after the strange and varying statements of so many experimenters, *especially* of those *who use the microscope*." (12)

Numerous and eminent physiologists have discovered that the globules possess a self-moving power; (13) and Mayer, and Eber, affirm that they are infusoria animals! whilst all this is flatly denied by Purkinje, Müller, Koch, Rudolphi, and others. As to their motion, Raspail pronounces the appearance "a microscopical illusion," (14) as does, also, Müller. Ratier goes so far as to say that the globules of blood may be seen in old stains upon linen, after *soaking* them in water; as also the animalculæ of semen. (15)

Like the globules of blood, those of pus are represented by different observers as possessing different shapes and sizes; some making them twice as large as others. Mr. Gulliver finds them in the torrent of blood in most fevers, (p. 388,) whilst Home, (16) Hodgkin and Lister, (17) deny that they have any existence till the fluid comes in contact with the air. Many refer their production to the physical dissolution of organic matter; others, indiscriminately to this and to a vital process; others consider them the nuclei of the red globules, although

(1) Philos. Trans. 1818, p. 187. — (2) Ibid. 1818, p. 173, &c. — (3) Elements of Physiology, p. 102. — (4) Med. Literat. p. 581. — (5) Outlines of Human Physiology, p. 475. — (6) Op. Cit. — (7) Philos. Magazine, Aug. 1827. — (8) Philos. Trans. 1818.

(9) Medical Literature, p. 571. (10) Ann. de Chim. et Phys. t. 29, p. 108.

(11) Ut. Cit. pp. 107, 108, &c. (12) Human Physiology, p. 148.

(13) It may be consolatory to reflect that Lord Kaims supposes inorganic matter to possess a "self-motive power." — *On the Progress of the Sciences*, b. 3. But, his Lordship, being no physiologist, was reasoning from a supposed analogy supplied by living beings. He confounded the vital with the physical powers.

(14) Ut. Cit. (15) Journ. de Chim. Méd. March, 1837. (16) Ut. Cit. and Wilson on the Blood, p. 292.

(17) Elements of Surgery, p. 30. Hodgkin's Anatom. Catalogue of Guy's Hospi. Part I. sec. xi.

they admit them to be twice the size of the former; whilst, Mandl, (1) by the same process of microscopic observations, finds that they are not related to the red particles, but are generated from some other constituent of the blood.—See Essay on *Inflammation*.

Calori says, the shape of the cerebral globules "varies much with the *mode of preparing* the brain for examination, and with the *degree of illumination* to which they are subjected under the microscope, as well as with the magnifying power. Nevertheless, the globular is most probably the real form of the nervous matter." These circumstances will probably explain the reason why Dr. Berres found "the relative position and connection of the elements of the nerves, and the diameter of the canals and vesicles very diverse." This diversity naturally gives rise to a new distribution of these structures "into three natural classes, and of each class into several orders." (2)

Blainville, and others, discovered that the transparent border of blood-vessels is formed by a coating of serum; whilst Weber describes it as composed of lymphatic globules, whose diameters vary from 0,003 to 0,0051 of a Parisian line, which move ten or twenty times slower than the included blood; though Müller denies the existence of lymphatic globules. (3)

It was but yesterday that Müller discovered a remarkable peculiarity in the organization of the corpora cavernosa penis, requiring a peculiar law for the circulation in that organ, and which "throws new light on the phenomena of erection." A plate of this anomaly is, of course, given. (4) But, to day we are told that "Müller's investigations were certainly of great importance, not only as establishing the minute structure of the penis, but, in a general point of view, as relating to a distribution of vessels to which no similar arrangement had been presumed to exist in the animal body. We have now to notice, however, a complete refutation of the views there advanced, and since almost universally received." "The real distribution of the vessels of the corpus cavernosum follows in every respect the most simple laws." (5) What, then, may we not surmise as to the imputed closure of vessels?—Shall we depend implicitly upon the observations of Breschet, Roussel, and others, upon the "vastly complicated" organization of the skin, which is affirmed, in consequence, to have "no less than seven or eight different functions;" (6) upon the

(1) Anatomie Microscopique, Liv. 2. (2) See British and Foreign Med. Rev. vol. v. p. 220.
(3) Physiology, vol. i. p. 112. (4) Ibid. p. 227. (5) Lon. Med. Gaz. June 23, 1838.
(6) But this is not all. "The most distinguished authors on dermoid pathology have boldly fixed the seat of important and obstinate diseases in parts of the skin denied by equally distinguished anatomists to have any existence in reality." "The rete mucosum continues to be described, though not *demonstrated* by teachers in our schools."

Again, continues Plumbe; "in former editions of this work I took occasion to observe, that evidence obtained by the microscope, in order to be efficient, must, at least, have the advantages of light *posterior* as well as anterior to the object examined." Partly upon this ground he sets aside the observations of Chevalier upon the cuticle. Again, as to the anatomy of the skin, "true it is that the reporters pursued their inquiries under the eyes of a large portion of their scientific cotemporaries, and secured the confidence of the latter; but it is equally true that Chevalier, in a somewhat similar position, pursued his investigations here, and arrived at results *so different*, as to leave no doubt that either he or Breschet and his confrère must have been deceived by their microscopes." Our author, also, after variously expressing his distrust of the microscope, and enforcing that doubt by saying "if there be *any truth* in the representations of the structure of the skin we have before us," &c. he arrives, at last, at the remarkable conclusion, that, although "more than half which has been written respecting the structure of the skin *has been imaginary, and very seldom indeed admitting of proof*, M. Breschet and his confrère seem to have set the question at rest." (a) Credat Judæus; certainly Plumbe does not.

(a) Plumbe on Diseases of the Skin, pp. 17, 19, 23, 25, 27.

discoveries by Purkinje, Valentin, Sharpey, of the ever *vibrating* cilia of the mucous membranes; (1) upon the anatomical analysis of the retina by Ehrenberg; (2) Langenbeck, Müller, Treviranus, Gottsche, (3) Volkman, and other Germans; that of its plexuses, by Kronenberg, &c.—the discovery of the modes of termination of the primitive fibres of the nerves by Emmert, Treviranus, Prevost, Dumas, and Valentin; having thus advanced beyond our knowledge of the terminations of the blood-vessels; (4)—of the primitive fibres of the brain and nerves, by Weber, Treviranus, Schwann, Valentin, Ehrenberg, Kronenberg, and other Germans, who find the diameter of some 0,0013 millimètre, composed, according to a few, of a semifluid and transparent oily substance, of a cylindrical form, tubular, and containing, according to Valentin, Ehrenberg, and Burdach, a transparent, oily fluid, which bears an uncertain proportion to the oily walls of the tube; (5)—of the characteristic varicose tubes of the brain, spinal

(1) These vibrations are to be seen after the vital powers are extinguished by cutting out, and dissecting a minute part of the membrane. This is the mode of examination.

(2) Ehrenberg says the retina is composed of various tubes; but it has been shown that he located them in the serous membrane of Jacob.

(3) Gottsche describes the process of preparation; which consists in macerating the retina in water, then subjecting it to the action of corrosive sublimate, and finally to maceration in alcohol for a week, when it is fitted for the microscope. (*a*) Monro first discovered his ultimate fibre "in a roasted chicken."

(4) According to another accomplished microscopist, these 'primitive fibres' are nothing more than branches of nerves. Thus, we read that "it is not very probable that the *so-called* primitive fibres, which are of considerable size, form the actual termination of the nerves, in parts of which the ultimate elements are much more minute than them. Schwann, indeed, has seen in the mesentery of the frog, issuing from the *so-called* primitive fibres, numerous fine filaments, which here and there presented small ganglia, from which again *several twigs* were given off;" &c. (*b*) And may we not safely conclude that the 'ultimate elements' of all parts are much more minute than the '*so-called* primitive fibres,' and, therefore, that these pursuits after the 'modes of termination of the nerves,' &c. are wholly utopian?

(5) This appears to have been nearly the opinion of Fontana. (*Obs. sur la Structure des Nerfs, &c.*) But, how has this knowledge been obtained? Ehrenberg used a glass of 3,000 diameters, and because the interior of the fibres is quite clear, they "must," therefore, "contain a granular fluid, or water, or vapor." He then proposes the very moderate assumption that this supposed fluid shall be what has hitherto been only imagined, viz. the "nervous fluid," and Emmert goes for its circulation. Muller (*c*) is also half inclined to adopt the hypothesis, though, in another place, it is certainly discouraged. Burdach endeavours to contradict it by tying a nerve,—an old experiment. Remak denies the fluidity of the central part, and says it is a *solid fibre*. Treviranus and Schwann maintain that these apparent tubes in the nerves are a *bundle of fibres*. It is worthy of remark, also, that Burdach affirms that hemorrhage produces a flattened appearance of the "so-called" primitive nervous fibres; that in inflammation and dropsy, artificially produced in frogs, the primitive fibres of the nerves are distended with fluid to a state of bursting; and that after death from prussic acid, the fibres of the brain speedily break up into globules.

Here, with what we have stated in this neighbourhood, is ground enough for hypotheses without end. We have noticed many of the highest moment, in the course of our work, which have sprung from the microscope. The most mischievous of the whole, as it appears to us, is that which inculcates the dependence of all hemorrhages upon rupture of the blood-vessels.

Billing had anticipated the discovery of the tubular structure of the nervous filaments *a priori*. Thus; "I am gratified to find in their recent publications (the German) confirmation of opinions adduced in this work some years since; as, for instance, my explanation of the cause of a limb being *asleep*, *benumbed*, by reference to the medullary part of the nerves being in tubes (as asserted by Fontana) which has been demonstrated by Ehrenberg;" (*d*) and so on. And then we have the moderate hypothesis by Mr. Gulliver based entirely upon microscopical observation, that pus is "the proximate cause of the sympathetic inflammatory, the sympathetic typhoid, and hectic fevers." (*e*) (See p, 686, *note*.)

(*a*) Pfaff's Practische, &c. H. 24, 1826.
(*b*) Muller's Elements of Physiology, vol. i. p. 804.
(*c*) Physiology, vol. i. pp. 340, 728.
(*d*) First Principles of Medicine; *Advertis.* pp. ix. xviii.
(*e*) Philos. Magaz. vol. xiii. Sept. 1838.

cord, and of the optic, olfactory, and auditory nerves, by Ehrenberg, but denied by Treviranus, Volkmann, Lauth, and Remak; though admitted as probable by Müller, perhaps upon the ground, according to this observer, that "the primitive fibres of the brain, spinal cord, and nerves of special sense, examined before they are subjected to pressure, appear of uniform diameter, *not varicose;* and, the fibres of other nerves, again, are *rendered varicose by pressure;*" (1) — the observations of Panizza upon the lymphatics of amphybia, who has found that the larger vessels embrace many smaller ones; — of professor Weber, that the veins of the tadpole are enclosed in lymphatic vessels; — the conclusions of Weber, Baumgärtner, and other Germans, who revive the old doctrine that the globules of blood are composed of several smaller ones; — the discoveries of Lauth, (2) and Krause, (3) as to the organization of the testis; the latter of whom found 484 lobuli in a single testis, and the former from one to several tubuli in each lobule, varying from one to two feet three inches in length; (4) — of Schwann, Müller, and Eulenberg, that the circular fibres of the arteries are not muscular; — "the physiology of Rudolphi, the delightful Biologie of Treviranus, and many other systematic works on physiology which have lately issued from the German press, of a loftier pretension and a much wilder flight"?

The Germans agree no better in their microscopical discoveries than their predecessors. The latter were often harmonious. Leuwenhoeck, Prochaska, De la Torre, Soemering, the Wenzels, Home, &c. confirm the observation of each other as to the globular structure of the cerebral fibre. Are the former confident in their opinions, the latter were even more so. The general principle has always prevailed more or less, that "a house divided against itself cannot stand." Still there is sufficient clashing to show, alone, the illusory nature of the pursuit; and now and then one, like Professor Berres of Vienna, and Calori, (5) is utterly at variance in most of his observations. These are duly put under the ban, and their discoveries denounced as wholly incompatible with common usage. Burdach, Volkmann, Treviranus, Lauth, Remak, Valentin, Müller, and others, have variously impugned the discoveries of Ehrenberg, the great modern Leuwenhoeck. If the modern instruments are better than the older ones, it is agreed by the present observers, that the former were good enough, had they who used them seen more accurately. The discoveries depend not in the least upon cumulative knowledge. They require nothing but good eyes, good glasses, and stoical patience. With all these Leuwenhoeck, Fontana, De La Torre, Munro, &c. were amply provided:

> "So true we speculate both far and wide,
> And deem, because we *see*, we are *all-seeing:*
> For my part, I'll enlist on neither side,
> Until I see both sides for once agreeing." (6)

Or, as Lord Kaims has it, "what speculative men have admired as profound philosophy, is only a darkening of knowledge by words without understanding." (7)

(1) Muller's Physiol. vol. i. p. 599.
(2) Mém. de la Soci. de l'Hist. Nat. de Strasburg, t. 54, p. 2. (3) Muller's Archiv. 1837.
(4) Taking five as the average number, and two feet, the average length, this would give nearly one mile of tubuli for each testis, or a secreting surface equal to a common bed-blanket.
(5) Who have even gone back to the globular doctrine.
(6) Byron's Don Juan. (7) Progress of Sciences, vol. iii. *Appendix.*

The contagion has extended itself largely into France, and has even affected the sober-minded English.

That we may not be deficient in our record of these passing events, we should advert to the revival of the seminal animalculæ by Prevost and Dumas, Davy, &c.; (1) and their discovery in the urine of seminal imbeciles by Lallemand, and Donné; — to the reproduction of the itch-insect, (2) by Raspail, Gras, Donné, and others; — to the revival by Raspail of what Adams and others pronounce the speculations of Linnæus, viz. that "many, if not all, of the exanthemata are, in truth, the result of insect operation on the skin," as are also, "cancer, tubercles, cholera, influenza, variola, and certain disorganizations of the liver, kidneys, &c. the work of entozoa, or insects in the animal economy" (3) of which Owen (4) has found more than 15 distinct species, infesting, especially, the organization of muscles, 25 having been found in the *tensor tympani*. These discoveries have been prosecuted by Farre, (5) Hilton, (6) Wood, (6) Curling, (6) Paget, (6) Knox, (6) Harrison, (7) &c.; whilst united observation has clearly demonstrated that the human machine, in every part of

(1) We are informed by Dr. Hodgkin, that "no trace of organization has yet been discovered in the seminal animalculæ, probably on account of their *extreme minuteness*." (*a*)

Dr. Davy thinks, if the profession will agree to allow it, the seminal animalculæ may be convert od to important uses in medical jurisprudence; that dried spots of semen may convict of rape! (*b*) Nothing has recently transpired as to the conflicts which were imputed to them by Leuwenhoeck.

(2) This insect was celebrated by Linnæus, Morgagni, Mead, Trozzi, Moufet, Banks, Sauvages, Rochard, Crell, Wichman, Hauptman, Bonomo, Cestonio, Cianelli, De Geer, and others, during the ancient regime of the microscope. De Geer, Hauptman, Bonomo, and others, have given drawings of it in the Philos. Trans. &c. Hauptmann, unfortunately, represented in his drawing the *acari of cheese*. Linnæus clearly describes the animal on the authority of others; and, according to Adams, he "admits many more insects in the different *exanthemata*, than he pretends to have seen." The deficiency, however, has been lately made up in Great Britain.

The itch-insect totally disappeared after the microscope was dethroned; or, perhaps, we should rather say was annihilated by Hartmann, Meig, Volkmann, Alexander, Weise, and especially by Adams. "An old woman," says the last, "who had consulted me for her niece, found no difficulty in detecting several of these insects; but in vain I endeavoured to trace the appearances by which she was directed. My friend Banger, whose patience in every investigation of natural history infinitely exceeds whatever I have met with, even in professed naturalists, was, for the most part, as unsuccessful." Adams finally caught the itch himself, as he well deserved; when, he says, "I applied to my old woman, who readily drew two ouçoes from my arm, but not from the vesicles. She constantly answered to our inquiries, that where the bladder was formed, the ouçoes had left the spot." Adams, at length, found that the ouçoes only burrowed in the hardened pimples. It is manifest that they are nothing more than an indurated secretion which precedes the serous stage of the eruption; which accounts for the old woman's philosophy that "the ouçoes always leave the bladders." Adams states "that the late Mr. Hunter, in his lectures and conversations, always acknowledged that he could never discover the itch-insect, and went so far as to suspect that the opinion concerning its existence was derived from a preconceived theory, and supported by credulity." (*c*) Raspail (*d*) now finds it where Adams's old woman failed, — that is, in the vesicle itself; some of the indurated matter remaining beneath the fluid. Gras (*e*) contends for its spontaneous origin; though Bonomo, its original discoverer, "saw it drop white eggs, like a pineapple seed, from its hinder parts." (*f*) Yet Bonomo required the microscope to distinguish the insect; whilst others, like Adams, affirm that it "skips about like a flea."

(3) Med. Chir. Rev. Oct. 1838. See, also, *note* above.

(4) Trans. of Zoolog. Soc'y. vol. i. p. 315. (5) Lon. Med. Gaz. 1835, 1836.

(6) Edin. Med. and Surg. Journ. vol. xlvi. p. 89.

(7) Dublin Journ. Med. and Phys. Scien. vol. viii. p. 185.

[*a*] Lectures on the Morbid Anatomy of Serous and Mucous Membranes, vol. i. p. 218.

[*b*] Edin. Med. Surg. Journ. July 1, 1838.

[*c*] On Morbid Poisons, p. 213, 216.

[*d*] Bulletin Gen. de Therap. Sept. 1834.

[*e*] Journ. Connoiss. Med. Dec. 1838.

[*f*] Philos. Trans. Lond. vol. xxiii. p. 1236.

its organization, is mainly designed as a laboratory for insects, — not excepting the globules of blood, which Mayer and Eber have found to be entozoa. Nor should we neglect the discovery by Dutrochet, and others, that the red globules are, severally, a galvanic apparatus, and his generation of muscular fibre by the action of galvanism upon albumen.

Finally, our microscopical philosophers would have it, that all nature may be ultimately resolved into microscopical insects. The very rocks are composed of them, — nay, the earth itself.

> "The propagated myriads spread so fast,
> E'en Leuwenhoeck himself would stand aghast,
> Employed to calculate the enormous sum,
> And own his crab-computing powers o'ercome.
> Is this hyperbole? ————" (1)

Let our own times answer. Ehrenberg has discovered not less than 28 different species that make up the flint and opal of a part of the European continent. On weighing one of their shells, it was found to be the 187,000,000th part of a grain. In iron ochre, according to Prof. Hitchcock, a cubic inch contained, in round numbers, 1,000,000,000,000 of these animalculæ. Thus we are beginning to try our hand at it in America, having now got rid of animal magnetism. We believe, however, that our able Professor Hitchcock is alone in this glory, in the Western Hemisphere. After stating that "millions of animalculæ inhabit a *single drop* of stagnant water," he announces their discovery in a fossil state. "They have the shape of an *angle worm* or *earth worm*, and it would take probably millions to make up a cubic inch. Yet the deposit that contains them is probably two-thirds composed of their remains, and in many places it forms a stratum *several feet thick, covering many acres, and may be found*, I doubt not, *in every town in the state*." The probability of the foregoing is enforced by the fact that "their skeletons are so thin that the light passes through them so as to render them almost *invisible*."

The whole of the foregoing statement, as it appears to us, is conclusive against the supposed existence of the animalculæ. Evaporate the drop of water which is said to contain "millions of animalculæ," and there will not be a residuum visible to the naked eye. Next, the animalculæ composing the flint and iron did not generate those substances. Suppose, however, the ochre and flint to have been deposited in water as highly impregnated with animalculæ as the foregoing drop; but before the deposition takes place, let us evaporate this lake of water. The residuum, taking as our standard the foregoing drop, will form a pellicle of unmixed animalculæ of the thickness of a sheet of paper. Restore the water, and animalculæ, and establish the deposits of silex and iron — where is the animal material to be transformed into the solid mass? We need not say that to constitute the supposed result, the water should have been rendered originally as dense as mud by the animalculæ; whereas, the animalculæ, *per se*, do not affect even the transparency of the fluid. (2) M. Grass has supplied us with a solution of this mystery in his supposed creation of animals out of silex; and the pictural illustrations which he has offered demonstrate the power of imagination upon this subject. (3) As in his case, so in the others, the objects

(1) Cowper, On the Progress of Error.

(2) If stagnant water appear turbid, it is owing to an intermixture of inanimate matter.

(3) This has laid the foundation of a new hypothesis of spontaneous generation.

mistaken for fossil animalculæ are nothing but crystaline spiculæ of the earthy or metallic substances. (1) As to the oxyd of iron, whoever has examined the lenticular variety, will readily see how easily its component parts may be mistaken for *bugs* by an ardent imagination. And here we may advert to the singular coincidence, that the microscope, which contributed so much towards subverting the fanciful hypothesis of the spontaneous formation of animals out of dead matter, should have now become the means of reviving the old prejudice. "Ever since the invention of microscopes," it was stated 40 years ago, "the opinions formerly embraced in favour of equivocal generation, both with respect to animals and plants, have rapidly declined. This instrument demonstrates that they actually spring from seeds." (2) Even now, however, as we have shown in another place, the microscope, in the hands of Ehrenberg, and others, confirms this great principle in nature.

We are mostly interested in the foregoing subject, as it regards the innovations upon fundamental principles in physiology, and the art of medicine. Nobody can doubt the admirable organization of every part of the human body, who simply regards its sensible structure, and the phenomena that depend upon the insensible organization. The latter are conclusive against the practicability of microscopical analysis, and can be alone indicative of the laws and actions of life. No light can be reflected by the skill of the anatomist where the vital processes are carried on. Hunter, with all his intense interest in the blood, complained that, "anatomists paid more attention to the globular form of the red part than it deserves; as if they could thence explain any essential principle in the blood or animal economy." (3) Bacon was, of course, of the same way of thinking. "There are those, he says, "who commit themselves to mechanical experience, without any method of inquiry. The greatest part of these have no considerable views, but esteem it a *great matter* if they can make a single discovery, which is both a trifling and unskilful proceedure; as no one can justly, or successfully discover the nature of any one *thing* in that thing itself." (4)

Admitting, however, that the microscope is sometimes correct in its report; yet, since it has so often played us false, who shall say that in this instance it is right, or in that, it is wrong? You have seen those who have travelled this route to fame considering it a necessary part of their task to overturn the chimeras of others, and declaring themselves to be alone meritorious in the use of the instrument; and even like Ehrenberg, the better to defeat the success of others, admitting that it was only after laborious practice, that they could at

(1) We have seen how an indurated secretion has been mistaken for an itch-insect. Here is another illustration. "Some years ago," says Prof. Dunglison, "J. P. Frank was requested to see a Prince who had been attacked with epilepsy. His physician, a respectable old practitioner, assured Frank that he could, at pleasure, make his patient void thousands of filiform worms. As he was neither able to define the genus, nor species of worms, the quantity of which, from his account, seemed to be prodigious, Frank requested to be a witness of the phenomenon. The physician administered a dose of castor oil, which produced several evacuations, in which were thousands of whitish filaments resembling small eels; but, on an attentive examination, these supposed worms were found to consist entirely of castor oil, combined with the secretions." (a)

(2) New-York Med. Repository, vol. iii. p. 393, 1799.

(3) On the Blood, p. 40.

(4) Preliminaries, s. i.

(a) Dunglison's General Therapeutics, p. 216.

all depend upon their own results. (') Finally, you have seen one system after another, which had been founded upon "microscopical illusions," tumbling to pieces, and bringing satire upon science.

We think it may be safely affirmed, that the microscope has not added to our knowledge a *solitary fact* of practical use, (²) to atone for the evils which it has inflicted. In this respect, there is a very striking analogy with animal chemistry.

In another sense, however, the microscope has opened a vast field to our admiring wonder, where there is a mixture of reality and illusion; and when employed with this intention, it is, perhaps, a rational source of instruction and amusement. If the senses be deceived, their gratification is not the less. The credulity, which but lately lent its ear to the fictitious report of Herschell's telescope respecting the animals of the moon, was not less delighted than the child who confides in the exploits of Munchausen.

> "——— through thy Herschell's glass,
> That brings from distant worlds a horse, an ass,
> A tree, a windmill, blankets, that on hedges dry,
> Thine eyes, at evening late, and morning soon,
> Unsated feast on Wonders in the Moon." (3)

Few practical men, however, are even inclined to employ the microscope for the purpose of enlarging their conceptions of Almighty Power. They are disposed to take the invisible world upon trust; not doubting that He, who has thrown around us a world of sensible objects too vast for imagination, (descending by a connected chain of gradations from an infinity of orbs to the smallest visible insect,) and too full of design to allow our reverential awe to languish for a moment, has also created other worlds to which our senses are wisely rendered inaccessible. Their invisibility, as well as that of minute organization, is in itself conclusive that nothing useful can result from microscopical developments, whilst it is their obvious tendency to abstract the mind from the practical, and the more stupendous objects of creation. The imagination of the multitude is not only unduly excited through the ordinary channels of intelligence, but through more vivid media, in the form of pictural illustrations. These delineations of art strike with the same force as the exhibition of realities; and we come to believe without inquiring into the sources from which

(1) Thus Ehrenberg: "De la Torre, Monro, and Fontana, examined with *similar apparatus* quite the *same substances*, and yet announced *quite opposite and completely irreconcilable results*." "Monro concluded that the nervous matter is the individual fundamental element of all organs, viz. skin, nails, hair, plants, and even *metals*, *earth*, and *salts*. He subsequently discovered his mistake, and regarded the phenomena as the result of an *optical illusion*." "Treviranus declared that he never took up the microscope without apprehension of that instrument which had already imposed upon so many observers." And, finally, Ehrenberg himself, "after a long continued use of this instrument only now for the first time has been able to arrive, through trials cautiously pursued with increasing confidence, at an *entirely new* representation of these objects." (a)

(2) The only practical purpose to which the microscope has been yet directed is by M. Donné; who, in a late treatise upon milk, devotes an elaborate inquiry into its microscopical composition, and tells us we can obtain but a very imperfect knowledge of its purity without the aid of the microscope; which, with characteristic enthusiasm, he commends to the use of all true lovers of the undiluted globules. — *Du Lait, &c.* 1837.

(3) This was literally realized by what is known as the "American Moon Hoax," of 1836; by R. L. Locke.

(a) Ehrenberg on the Structure of the Nervous System, pp. 76, 78.

they spring. "The senses," says Bacon, "are like the sun, which displays the face of the earth, but shuts up that of the heavens." We humbly admit, that we once regarded these tangible objects as but little short of demonstration; and whenever they were tinted with the colours of the rainbow, conviction became as irresistible as was once our dream that Iris suspends at the end of her zone "a bag of gold;" nor did we give up the illusion, till, as in the latter case, we went in pursuit of the phantom.

The great founders and promoters of physiological science had either no knowledge of the microscope, or condemn its use. "Authors," says Bichat, "have been much occupied with the intimate structure of the glands. Malpighi admitted that there were small bodies in them, and Ruysch determined that they were all vascular. Let us neglect all these idle questions, in which *neither inspection nor experiment can guide us.* Let us begin to study anatomy where the organs can be subjected to the senses. The exact progress of the sciences in this age is not accommodated to these hypotheses, which made general anatomy and physiology a frivolous romance in the last." "I will not state the result of Leuwenhoeck's, and the observations of others upon the muscular fibre, because science can draw nothing from the knowledge." "No methodical mind will attend to the minute nature of this fibre, upon which so much has been written." "When the voluntary muscle contracts, does it diminish in volume? The inquiry deserves no attention." Bichat maintained that the whole of this subject is "concealed by an impenetrable veil." (1) His motto was, evidently, the sentiment of Lucretius, "nequeunt occulis rerum primordia cerni;" and it was this conviction which gave a direction to his inquiries that places him amongst the greatest benefactors of man.

Mr. Hunter has stated many objections to the microscope. We shall repeat but one more. "As the naked eye," he says, "when viewing an object rather too small for it, is not to be trusted, it is much less to be depended upon when viewing an object infinitely smaller, brought to the same magnitude by a glass. (2)

"When objects can only be detected by highly magnifying lenses," says Dr. Bostock, "it is so extremely difficult to avoid being misled by ocular deceptions, that *all descriptions of this kind* are to be received with the greatest caution." "Microscopical observations may be warped and accommodated to a preconceived theory, even by a person of skill, science, and integrity." (3)

"It requires much practice," says Dr. Hastings, "in the use of the microscope before any dependence can be placed on the experiments of the most careful observer." (4)

And thus Dr. Grangier: "In examining the roots of the nerves, I have always relied on the assistance of the naked eye only; avoiding, for fear of deception, the use of the lens." Mr. G. moves upon the great fundamental principle, that "success, when it is to be obtained by experimental inquiry, can only be commanded by making these manual operations subservient to com-

(1) Bichat's General Anatomy applied to Physiology and Medicine, vol. iii. pp. 199, 200; vol. ii. pp. 333, 335, 393.

(2) Hunter on the Blood, &c. p. 42.

(3) Bostock's Elements of Physiology, vol. i. ch. 1. pp. 304, 374.

(4) Hastings' Experimental Inquiry of the Nature of Inflammation, p. 46.

prehensive views of the laws which regulate the structure and the functions of organized bodies." (1)

"Every one," says a learned reviewer, "who has examined doubtful objects by a high magnifying power, must be aware how much is often left to the imagination of the observers; and it is not difficult to account for the great discrepancy which exists in the statements of animal as well as vegetable anatomists." (2)

"The microscope," says another eminent reviewer, "has often played us false in anatomy, — too often to challenge much respect from us." (3)

And yet another: "In microscopical observations, whether from the almost unavoidable aberrations of the rays of light, or the scarcely less inevitable prejudices of the observer, no branch of the sciences, depending on the evidences of the senses, affords such various and contradictory results." (4) Cavallo offers the same opinion and explanation. (5)

And many others: Thus, the Dublin Medical Press, (6) in speaking of Mr. Carpenter's "General and Comparative Physiology," remarks that, "there is a chapter on the elementary structure of vegetables, and another on that of animals, in which, according to the established usage, the improbabilities, and fictions of the microscopic observers are recorded." "The student must not place confidence in these wonders of the microscope." "But this is *entre nous;* for if it were known that we entertained such heretical notions, in these palmy days of the reign of microscopes, we might risk encountering some grievous *bodily harm.*"

The microscopists "present us with dim spectacles to see what we see clearly without any, and by the natural strength of our eyes; or else to see what is not by nature, nor can be made visible by art. They mingle the real, and connect it with so much fantastical knowledge, that they disgrace and weaken, as much as it is in their power, the former by the latter. It was this very practice which hindered the stoics from beating the epicureans out of the field of controversy, and from imposing silence on those babblers, the academicians." (7)

We should not close this discussion without adverting to the opinion of a Review which justly exercises a powerful influence in medical literature. In speaking of the discoveries of the fibrous, or tubular structure of the teeth, the reviewer observes, that "we hope that the knowledge of this fact will prove a satisfactory answer to those who refuse to use the microscope, and to listen to the results derived from its use, because it is liable to so many fallacies." (8) But, if we allow, as we do, the accuracy of the microscope in respect to the fibrous structure of the teeth, and perhaps the tubular, we see not how this particular fact is to set aside the universal admission by microscopic observers of the "fallacies" of the instrument, and the continual conflicts of their observations. It will be readily seen, also, that there is a vast difference between the solid and the soft structures of the body, and that minute observations may be perfectly practicable in the former case, when they would fail entirely in

(1) Grangier's Observations on the Structure and Functions of the Spinal Cord, pp. 37, 68.
(2) British and Foreign Med. Rev. No. 7. p. 6.
(3) Medico Chir. Rev., Lon., vol. xxxii. p. 123.
(4) Edin. Med. and Surg. Journ. No. 58, p. 120.
(5) On the Medical Properties of Factitious Air, p. 237, &c. (6) Jan. 23, 1839.
(7) Bolingbroke's Works, vol. v. p. 450. (8) British and Foreign Med. Rev. July, 1839, p. 159.

the latter. We are glad, however, to see that the senses are beginning to admit what all analogies in physiology should have led us to conclude to be certain; nor do we doubt that it will be yet conceded that the crystaline lens, nails, hair, &c., are organized and endowed with vital properties, and that no part of the animal machine can exist in a state of dead matter in connection with living parts.

SUPPLEMENT TO ESSAY ON THE VITAL POWERS.

PHILOSOPHERS of all sects, and at all ages, have generally ascribed the results of vital actions to a living principle, distinct from the mind, and the physical forces. This induction having been founded on the phenomena of life, is, therefore, impregnable from the coincidence of observations. Plato and other ancient philosophers considering the soul immaterial, found it necessary to suppose the existence of a principle, distinct from the soul and body, to account for many of the results of life. They could not, consistently with reason, admit that the soul itself is adequate to explain the vital actions. But, with them the vital principle was equally immaterial, so that the difficulty was in no respect overcome, nor did it any better explain the mutual action of the soul and body upon each other. If, however, on the other hand, the vital principle be supposed to be material like the rest of the organized system, nothing is gained in this sense; since it must then be admitted that common organic matter is life.

In our essay on the Vital Powers we have endeavored to remove the foregoing difficulties. (p. 82, — 102.) The difficulty of conceiving of the existence of the vital principle, and of the nature we have imputed to it, forms no ground of objection. The same, as is often done, might be urged against the soul, and against matter itself; whilst each are equally denoted by their peculiar phenomena.

The stoics maintained the existence of a vital principle; (1) and Sennert says it was recognized by most physicians under the name of *calidum innatum*. (2) It was mainly lost sight of during the dark ages; but reappeared amongst the earliest restorers of learning; (3) when it soon after took the name of *anima vegetans*. Paracelsus substituted that of *siderial spirit*, to suit his notions of planetary, and demoniac influence. Van Helmont next made his innovation of the Archæus, out of which grew Stahl's *rational soul*. Lord Bacon strongly defended the vital principle. (4) Next, we have the *vis nervea* and *vis insita* of Haller, which, indeed, had been suggested by Galen. Whytt also modified the Stahlian doctrine so as to exclude the vital principle. But Descartes has the honour of leading the way in rejecting altogether the vital powers, for which he was prepared by the hypothesis of the nervous *fluid or animal spirits*, which sprang up about that time. Then followed the doctrine that matter acquires vitality in virtue of a peculiar organization. The de-

(1) Barthez, Nouv. Elem. de l'Homme, c. 2. (2) Epitome Institut. Med. p. 81.
(3) Brucker's Historia Philosophica, t. 5, pp. 50, 136, &c. (4) Hist. Vit. et Mort.

parture from nature had now become so great, that Monro, sensible that the phenomena of life cannot be explained by physical laws, and seeing nothing of a tangible nature in the organized system, cut the knot by supposing that "the Power which created all things, which gave life to animals, and motion to the heavenly bodies, continues to act upon, and to maintain all, by the unceasing influence of a living principle pervading the universe, the nature of which our faculties are incapable of duly comprehending." (1) This doctrine, as we have seen, has been variously adopted by other philosophers. (pp. 10, 25, 38, 51, &c.) But, besides the objections we have made to it, we may state two others by Ferriar. "If," he says, "the living power be supposed to be an immediate act of the Deity, an opinion which has been held by many philosophers, this is liable to still stronger objections; for the consequence would be, as it is urged by one of Cicero's speakers, 'cum miseri animi essent, quod plerisque contigerit, tum Dei partem esse miseram, quod fieri non potest.' If it be said that the living principle, on this hypothesis, is the connecting medium between the mind and the body, this supposes the Deity to act in subordination to the human mind, which cannot be admitted."

Hunter, recurring to the Hippocratic observation of nature, restored and improved the ancient philosophy as to a vital principle; which was afterwards farther illustrated by Bichat. But we admit obscurities of style in relation to the former. The flood of light became now too dazzling for the multitude, and they turned to darker objects. Ambition, too, found a repast in controverting the most sublime institutions of nature; and materialism and atheism, of course, joined furiously in the crusade. Blows have continued to fall thick and heavy, till, in the language of the British and Foreign Medical Review, (2) the "writings of Bichat are almost forgotten;" or, if now and then a vitalist break the monotony of hosannas to the inorganic world, he confounds the spiritual with the vital part, and resolves the problem of micturition upon the principle of an "organic consciousness of vacuity." (3)

By the way, we had the honour of holding an argument with the British and Foreign Medical Review in our Essay on the Vital Powers. (p. 11, &c.) Nor shall we soon forget it; since the language of the reviewer was one of the most discouraging obstacles to our enterprise. We shall repeat the reviewer here, that it may appear in connection with our final remarks.

"Regarding life in the abstract, as synonymous with vital action," says the Review, (Jan. 1839, p. 171,) "or in any one living being, as the aggregate of phenomena by which that being is characterized, the author, (Mr. Carpenter,) shows, that, instead of looking for its cause in an imaginary vital principle, or organic agent, presumed to exist for the sake of explaining the phenomena, we ought to study the properties which organized structure enjoys, and the agents which produce their manifestation. Some observations are made in refutation of the doctrine of a vital principle, and we do not think them supererogatory; for, although the hypothesis could hardly be expected to survive the fine scientific thrusts of Dr. Pritchard's classic weapon, or the strokes of Dr. Fletcher's more truculent blade, it seems even yet not quite extinct."

Since the printing of our "Vital Powers," the October No. 1839, of the fore-

(1) Observations on the Nervous System. (2) July, 1839, p. 166.
(3) See Macartney's Treatise on Inflammation, p. 12, 1838.

going Review has been published, in which an able writer divides the organized machine between vital and physical forces. But what we are most interested in, is our attempt to contradistinguish entirely the different forces; and we bring to our aid, from this reviewer, an argument which we have employed in our Essay to show that the understanding must acknowledge the distinction which we have made. "How," says the reviewer, "do the changes of innervation affect the diameters of the (capillary) vessels? It can only be by modifying their *elasticity*, or by influencing *their vital properties*. Now what ground have we for supposing that the nervous influence can effect an immediate change in the elasticity, or any other physical property of a texture? None whatever; there is no fact in physiology which affords the shadow of support to such an opinion." If this be true, what becomes, also, of the mechanical doctrines of inflammation, the speculations as to chemical agencies in the formation of the secretions, &c., all of which results may be brought powerfully under the nervous influence?

Finally, we have in this No. of the British and Foreign what we had no doubt would soon be the case, a distinct recognition and defence of "*a vital principle*." Thus: "if his (Magendie's) opponents have assigned too much to the influence of a vital principle, it is certain he has assigned too little." At other times, the reviewer calls it the "*vital power*."

The London Medico-Chirurgical Review of July, 1839, (p. 169,) in speaking of Mr. Carpenter's physical theory of life, (see p. 22, &c.) remarks that, it is "not very new, it must be owned, — nor very conclusive, because nothing is proved, — nor satisfactory, because it leaves all the difficulties where they were." "Not only are there no new facts, but there are not even any new arguments." "In fairness to it, (the doctrine of a vital principle,) and its supporters, we do not quite perceive how Mr. Carpenter has demolished it."

We have spoken, (p. 21,) of the retention of the vital principle in the seeds which have been recently taken from Egyptian mummies. Van Sweiten states an instance of the vegetation of beans after having been kept "above two hundred years;" and remarks that "Reaumur has made it very clear, that the vital rudiments of an animal may remain unchanged for many years, and (buried, as it were, in a long and silent repose,) forbear to give the least sign of increase; yet, when occasion serves, is afterwards capable of producing another animal, in all respects according to its own likeness." (1) (See p. 17.)

We have justified ourselves in adding this Supplement, and the following upon the Humoral Pathology, by the importance of the subjects. The whole science of medicine, in our opinion, hinges upon a proper understanding of the vital powers; whilst the latter doctrine is not only in conflict with the philosophy of life, but with the well-being of mankind.

(1) Commentaries on Boerhaave's Aphorisms, vol. xiii. pp. 93, 94. See, also, Reaumur, Mémoire sur l'Hist. des Insectes, t. ii. p. 30.

SUPPLEMENT TO THE HUMORAL PATHOLOGY.

In our examination of the humoral pathology, we have taken the ground that the facts and arguments of its defenders must be fully met, or the doctrine must be admitted. We have felt no disposition to be ceremonious with assumptions, or with any data that have been sent forth without a proper examination of their merits. Of the latter description we have come in conflict with many:

"Pick'd from the chaff and ruin of the times
To be new varnished. ——————"

As one of the most notorious, we may say that the case by the "celebrated Du Hamel," of which we have spoken, (p. 534,) continues to be quoted by our best pathologists as the *ne plus ultra* in humoralism. Prof. Gross, in his excellent work just from the press, has it twice over, along with one similar to that related by Rush, in a note at the foregoing page. From these, and other facts, our able author thinks "it cannot be doubted, by any one who duly and impartially contemplates this interesting subject, that the blood is a fruitful source of disease," &c. (1)

We ought not to suffer the case from Du Hamel to pass, without saying that it not only appears to contain an abundance of internal proof of its fabulous nature; but that Morand, from whom it was originally copied, is not very clear as to its authenticity. This professor states, that "the flesh was sold to the better houses of Pithiviers and its environs, and no one who ate of it was in the least affected by it." So far, also, as the cooks were concerned who handled the flesh, no one was known to have sustained any injury. (2)

Again, it is said by Dr. Duncan, that "Morand subjoins" the foregoing "fact on the authority of the celebrated Du Hamel." (3) And so Andral; "for an account of which we are indebted to the celebrated Du Hamel." (4) And so others. But this was impossible; for, although the story was actually related to Morand (the son) by *one* Du Hamel at a lecture by the former in 1766 to *match* another with which the lecturer had entertained his audience, the "*celebrated* Du Hamel" had been then dead just sixty years. Nor does anything appear to enlighten us as to the real relator, or the degree of credibility to which he was entitled, or whether the story was not intended as a burlesque upon that of the lecturer.

But let us hear Morand, or rather his reporter; from which it may at least appear, that not only the whole account was a matter of hearsay, and exposed

(1) Patholog. Anat. vol. i. pp. 222, 223, 296. — 1839.

(2) "Cependant la chair en fut vendue aux meilleures maisons de Pithiviers et des environs, et personne de ceux qui en ont mangé n'en a été incommodé; il auroit peut-être été curieux de savoir si des animaux, qui en auroient mangé crue, ou qui en auroient bu le sang, en auroient été affectés." (a)

(3) Dr. Duncan, in Edin. Med. Chir. Trans. vol. i. p. 567. 1824.

(4) Patholog. Anat. vol. i. p. 400.

(a) Hist. de l'Acad. Roy. des Sciences, 1766, p. 104.

also to the treacheries of memory, but, that the other case by Morand himself, which is often cited in connection with Du Hamel's, is not entitled to that degree of credibility which may be well where important physiological principles are to stand or fall according to the nature and validity of circumstances. Thus, then, the Royal Academy's Transactions: "This fact was not reported to the Academy till after the expiration of a year; Morand having thought it necessary to allow that time to elapse to assure himself that the two butchers had not played him false. However this may be, the lecture, to which the statement of the butchers gave rise, *reminded* M. Du Hamel of another similar event which happened at Pithiviers, in Gatinois." (1) Then follows the story.

But the whole "chapter of accidents," according to Du Hamel, has not been given by the copyists. He stated, as a sequel to the first tragedy, that a surgeon, who plunged his lancet into the tumour on the face of the servant, and then stuck the lancet into his wig, ("mit sa lancette entre sa *perruque* et son front,") was seized with an inflammation of his head, of which he was ill for a long time, ("fut long-tems malade.") This part has been suppressed, notwithstanding its importance in humoralism, in showing the indefinite transmission of the poisonous influence of "overdriving."

"——— Thus we steer,
Stumbling o'er recollections; now we clap
Our hands, and cry 'Eureka!' *it is clear* —
When some false *mirage* of ruin rises near."

but

We must once more advert to Bichat. This philosopher, who has done so much for sound physiology, was sometimes strangely inconsistent. We have seen (p. 608) that he denies, entirely, the absorption of urine, bile, &c.; yet, at another time, having injected them in unknown quantities into the circulation, and the animals generally surviving the experiment, he concludes (in the same volume, iii.) from this artificial process that they may be absorbed. But, this is neither philosophy, nor legitimate humoralism.

It is worth saying, in reference to our remarks at p. 658, that Huxham, a great humoralist, allows the inutility of bloodletting in contagious diseases, because the whole blood is contaminated, and "you will very little lessen the whole contagion, which will have its effect whether you bleed or bleed not." (2) This principle is, theoretically, and even practically and hopelessly, carried out to other reputedly humoral diseases, by existing philosophers. (See p. 540, *note.*)

But, "this is the age of free thinking, and daring spirits treat the whole of this venerable doctrine as a MERE CHIMERA:" — (3)

"Thus much we've dared to do; how far our pen
Hath wrong'd these stirring times, but *where* or *when*,
This let the world, which knows not how to spare,
Yet rarely blames unjustly, now declare."

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(1) Histoire de l'Acad. Roy. des Sciences, 1766, p. 102 — 104. "Ce fait ne fut rapporté à l'Académie qu' après un an révolu; M Morand *crut ce temps necessaire pour s'assurer* que les deux Bouchers n'*avoient essuye aucune rechute:* mais la lecture qu'il en fit, *rapella* à M. Du Hamel un autre événement pareil arrivé à Pithiviers en Gatinois."

(2) Huxham on Fevers, p. 102. (3) Moore's Medical Sketches, p. 103.

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A REPLY

TO THE

MALICIOUS PERSONAL ATTACK

OF THE

British and Foreign Medical Review,

BY E. O. HOCKEN, M.D.

When reviewers condescend to make their articles (which receive weight from the character of the journal in which they are published), a vehicle for low, malignant, personal abuse, aimed at any one whom they may wish to injure—stabbing in the dark, or stinging like the viper in the grass—instead of fairly and *honestly* examining the publications entrusted to them to criticise, the medical public will, I think, bear me out in stating, that an author (so treated) is fully authorized in exposing the mischievous spirit, the falsehood, and the trickery of which his reviewer has been guilty. Having (as I trust I shall be able to show), just reason to complain of the scurrilous and ungentlemanly attack made on me by the reviewer in Dr. Forbes' Journal,* for January, 1845, I take this plan, the only one an author can adopt, to expose the untruth of his insolent insinuations, and the wilful mistatements and perversions which abound in every page.

The temper of the reviewer may be gathered even from the opening passage. "Dr. Hocken has been for some time known to those whose avocations require them to watch the proceedings of literary persons, as an individual of unquiet temperament, prone to write de omnibus rebus. The character cannot be said to conciliate readers of any description, when taking up a new specimen of Dr. Hocken's pamphleteering faculty. Even the mildest mannered critic feels disposed to look askance at each additional production of a man, who, as far as his writings justify an opinion, does not certainly rise above the average of his compeers in mental qualifications, and yet presumes to put himself perpetually forward as a teacher and guide in matters of science and practice."—p. 147. We have some clue to this uncalled for and personal attack, in a transaction which took place some three years back. We had then

* *British and Foreign Medical Review*, p.p. 147—55.

to complain of a flagrant untruth which appeared in the *British and Foreign Review*, in criticising an Essay of the Author's on Hysterical Amaurosis, to the effect that the subject had been more fully and ably described by almost every writer on Hysteria. We then received a note, in reply from the editor, couched in the most vindictive and personal language, referring us to Cooper's Surgical Dictionary for a proof of the statement. Fancy, reader, the full and able *description* of the disease, which justified so sweeping a statement—"One not unfrequent cause of amaurosis is hysteria and hypocondriasis, with which must be mentioned disease of one or more of the abdominal viscera, especially the liver."—*Boer, Lehre, &c.*, b. ii, p.p. 444, 446.—Cooper's Surgical Dictionary, p. 21, edi. 7th.

For some time past the covers of the periodical press have literally groaned beneath the self-congratulations of the British and Foreign editor.—"He does not hesitate to avow his conviction, that the eighteen volumes of the review, now before the public, contain a greater amount of important information in all departments of medical science, more of novel yet sound practical instruction, and more of just and enlightened criticism than are to be found in any similar work of the same extent." In proof of this he refers us to America, where, on landing, we observe certain pamphlets from one *Dr. Paine*, which speak highly in favor of the purity and "sincere desire to ascertain *the truth on* all subjects" of the said editor and his review. Perhaps he has entirely forgotten (but we will endeavour to refresh his memory) that Dr. Martin Paine most clearly and satisfactorily proved that the Review of his "Commentaries," besides being most illiberal and unjust, was an unblushing plagarism—a dishonest and impudent theft;—and this too in the immaculate pages of the *British and Foreign Medical Review*. Honest men felt shame for the editor of a British journal thus convicted, and public opinion demanded the name of a writer, who had added imperishable lustre to a work of such surpassing merit and faultless truth. Alas! "all is not gold that glitters." Some of the writers of its articles are men of straw! when sought for, they have gone to *Australia!!* All this too comes out—not in an open and honest manner, with manly indignation—but with the underhanded sneaking attempt of guilt to bribe Dr. Paine to silence, by privately flattering him, and offering him the office of reviewer for the very journal which had just before attempted, in the most dishonest manner, to rob him of his due meed of praise.

This is, as far as I know, the only case in which the name of a writer in the journal (which is such a source of "high gratification to the editor,") has been demanded, and as might have been anticipated from the nature of the review, he was in Australia. We would ask the editor, was it as a convicted felon? *If*, in the only enquiry which has been instituted, the reviewer has been found dishonest, in how many undiscovered instances may the same

pareil arrivé à Pithiviers en Gâtinois.

(2) Fevers, p. 102. (3) Moore's Medical Sketches, p. 103.

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have occurred? and who could, subsequently, place any faith r he honour of an editor, who has been proved to be deficient in this most necessary constituent of honest minds? We would also ask the sun and centre (according to his own account) of the medical literary system, whether (as the author "would, in all likelihood, be an excellent person to find a needle in a *bundle of straw*,") his reviewer is also a man of *straw?* whether he has yet paid a visit to Australia? or has not richly deserved to visit it long ere this? Can he also inform us whether our reviewer was born to greatness? or strutting in the borrowed plumes of more deserving individuals, has had greatness thurst upon him.

Is consumption curable? "We," says the reviewer (viz., Messrs. Forbes and Co.), "are far from saying that it is not so;" but "WE have ever been most anxious to keep inquirers in the right path"—not of course, if possible, to prolong the darkness and ignorance of superstitious scepticism, nor to monopolize the *fees* and consciences of phthisical patients; from such designs the immaculate reviewer is of course free. What, then, are we to learn from this bombastic flourish? This, namely—that any plan of treatment, which does not originate with Forbes and Co., (whether true or false) shall be written down, denounced, and the author (no matter how honestly he may act or write) be stigmatized as a quack, or branded as an impostor. Every honest reviewer will echo the sentiment, and perceive the drift of the following passage from a review in the *Provincial Medical and Surgical Journal, December 23rd*, 1843, *p*, 233.—"We do not belong to that class of critics amongst whom an eager and indecent haste to condemn everything novel, *which does not originate with themselves*, seems to pass current as the pink of critical acumen."

The author's "Practical Enquiry" was written solely for the profession, with the hope of inducing educated men to give the *Medicinal Naphtha* a fair trial, and not allow dust to be thrown in their eyes by the obstinacy and *avarice* of malicious and prejudiced reviewers. He defies an honest man to detect any passage which might give the slightest colouring to the unjustifiable and malignant personal attack of the British and Foreign reviewer. He has never in any of his writings (which he owns are numerous) written for the public, nor made any undue or unjustifiable use of means to spread his name. He has never advertised or puffed himself improperly, nor attempted to take to himself the credit and glory of others' labours, (as some have done) who themselves have never been guilty of a single original idea, nor added one line of the least practical value to medicine; although "prone to write" not only "de omnibus rebus," but also "de omnibus rebus et quibus-dam aliis."

It will be obvious to any one that he who writes an Essay for the use of the medical profession, which fully explains every particular, and is opposed to every prejudice and evil passion of those whose interests are concerned), so that any, who have the

honesty to try the remedy before they condemn it, can employ it without the advice or assistance of the author, is not likely to benefit self, but is certain to draw down the venom and torrent of abuse of the evil disposed. Impressed with these considerations the author prefaced his Essay with the assertion that he put it forth "with a sincere desire to benefit *others*, and entirely without the prospect or probability of benefitting *self*." The reviewer, with surpassing charity, and with a "sincere desire to ascertain the truth on all subjects, as far as practicable" comments on "Dr. Hocken's disinterestedness and abnegation of self on the one hand, and his divine charity and spirit of universal love on the other." "It might be contended that there was some similarity between this exordium and the affectionate phraseology wherewith Messrs. Goss and Co. urge the claims of the 'Silent Friend,' the 'Ægis of Life,' &c."

As the bad man* believes all bad, the thief all thieves, so the reviewer, who is himself the cage of every unclean and hateful thought, believes all equally dishonest and covetous as himself. The Jamaica wharfinger elevated to the dignity of senator, voted restrictions on his former brethren, saying, "They are all rogues to a man,—I was one of them myself for twenty years:" so the undeserving critic who has greatness improperly thrust upon him. As if he felt, to look people in the face he must *first prove himself no* worse than others—or rather, others no better than himself; he has a malignant pleasure—a deep policy of life or death to him—in making all as dishonest as himself.—He has a microscopic eye for resemblances—the beam in his own eye seems to sharpen his vision for the motes in others; and a beard in the fairest palm that lady ever delighted to show us, is a less startling phenomenon than the places in which he detects empiricism. Like bankruptcy to the insolvent, it is everywhere. Such delight in raising themselves, (not by their own mental superiority), but by maliciously slandering and depreciating their neighbours, although it is true that they generally defeat their own purpose, by the very venom in which they wallow being too apparent to be overlooked.

Because the author happens to use the words, "the consumptive falling victims, as many of them do, in the very pride of youth and beauty," the reviewer announces his conviction that "they who will it, may safely conclude, his doors are as open to the sufferers as his heart to the sufferings;" and because the author states his conviction that "naphtha is not to be regarded as a speedy and certain *cure* in all forms and stages of phthisis," he concludes that it "furnishes a valuable hint to Dr. Hocken's future patients (we mean, of course, his *dispensary patients*) not to lose confidence too soon, or give up attendance so long as they have time [or money?] to spend in improving their health and swallowing naphtha." The reviewer omits the conclusion of the passage—"yet it almost invariably effects

* Vide *Medical Times*, Dec. 28th. p. 284.

Fevers, p. 102. (3) Moore's Medical Sketches, p. 103.

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the most prompt and marked *relief* of the general symptoms, not only in the early, but in the advanced stages of the complaint." Are Forbes & Co., in fear that few patients will hereafter darken their consulting rooms, that the most simple and innocent passages are made to bear the most mysterious and guilty meaning? or is it because they have already condemned naphtha and its supporters without proof of any kind, in language fit only for the spotless pages of the *British and Foreign Medical Review*, and their ire being aroused by any one "*presuming*" to differ from them in opinion,—"for if THEY have said it, THEY have said it?"

"Dr. Hocken expatiates on the 'delicacy and accuracy in hearing,' and upon 'the taught and thoroughly practised ear' required for the additional subject on which he has chosen to enlighten the public." The reviewer is either uttering, in this passage, a premeditated falsehood, or he is ignorant of the meaning of the word "expatiate." As nothing but *truth* could by any means find its way into a journal so desirous of its investigation, we are compelled to adopt the latter conclusion, and to enlighten the ignorance of the reviewer by a quotation from Dr. Johnson's Dictionary,—"Expatiate, *v. n.*, to enlarge upon; range at large." The passage in the author's preface standing thus: "In apology for writing on a subject which requires both a natural *delicacy and accuracy in hearing* and appreciating sounds, as well as *a taught and thoroughly practised ear* to enterpret these sounds;" it will be obvious that the author has neither enlarged upon, nor wandered far into the wide field thus thrown open to him.

We very much doubt if the reviewer be capable of distinguishing between coal-tar naphtha and the hydrate of the oxide of methule, much less of distinguishing "*the* naphtha" from "the other naphtha." His remarks on that head are therefore of peculiar value.

In kicking down some of the mole-hills which the microscopic mind of the reviewer has magnified into mountains, the author would state once for all, that his only object was to give naphtha a fair trial, and report the *facts* he had collected, being as desirous to give unfavourable as favourable results. Had *all* died under the treatment, he would as candidly have confessed it as he has now done in stating his belief of its value. The conclusion of the reviewer that "the result of Dr. Hocken's treatment actually falls below the average," is only a proof (of the vanity and folly?) of his reasoning powers, inasmuch as the mortality occurred amongst the patients with very advanced disease, (cases "actually moribund," not being "eschewed," as the reviewer falsely asserts), in whom the author expressly states, that naphtha did not effect anything more than mere relief of symptoms,—and the qustion is, not how many *died*, but how many had the physical signs of their disease greatly ameliorated or cured, when, if we mistake not, the odds would tell in favour of naphtha.

To favour his *charitable* ends, the reviewer has purposely omitted

to show that the author commences his Essay with some brief but satisfactory proofs that phthisis is not always incurable—although rarely cured—either by medical means or the *vis medicatrix naturæ*, and consequently that naphtha was not to be regarded as a specific, but only as the most valuable medicine that we at present know. Hence, we can conscientiously assure our *honourable* reviewer, that we do not feel at all annoyed by his second proposition,—"that Dr. Hocken's work proves him to be perfectly incompetent to undertake the sound examination of a question in therapeutics." His vituperations are the best proofs of his own character and humility. When the author publishes a complete work on phthisis, he will take care not to omit "the natural course and progress of the disease, the proportion of persons it kills, and the average time in which it effects their destruction;" but when he merely publishes a clinical report of twenty-three cases, to show the operation of a particular medicine with *other* appropriate treatment on the disease—addressing his Essay to those whom he presumed to be already well acquainted with these matters, and stating in his preface that his object is to present the reader "with a certain number of *facts, from* which he may draw *his own conclusions,*" the necessity for such a comparitive view is at once removed, and the only remaining dilemma is with the reviewer, whose "sincere desire to ascertain the truth," reflects unbounded credit on the immaculate pages of *the journal*.

Our reviewer, in considering Case 1., is at a loss to divine what "hectic symptoms" are; we should have expected this from the learning already displayed. "*Ten* minims of the liquid" were ordered, he goes on to say, "thrice, daily." *Fifteen* were given. Is this carelessness, or wilful perversion of facts? Because the patient's general health was not much improved on the 6th of April, he finds the statement that—"the dulness on percussion was sensibly diminished, and the murmurs less harsh" "rather difficult to reconcile," simply because his own patients had never exhibited any improvement from the bigoted and routine treatment, under which (poor wretches) they were doomed to suffer and die. But "Dr. Hocken seems to have waxed doubtful of the influence of naphtha, for he now prescribed gentian and a *blister* to the chest, &c.," although the author has constantly inculcated from the very commencement of his Essay (p. 2.) the necessity in most cases of combining naphtha, "with such other treatment as the features of the individual case may indicate."

There existed no physical signs or general symptoms of disease on the 11th of May, yet the reviewer concludes of course "that the tuberculous disease itself had disappeared is not shown," and to imagine that it had for an instant, "savors either of imbecility of mind, or of impudence to a degree which it is painful to contemplate." Gentle critic, share, I beseech you, this intolerable load! Imbecility of mind seems to be your birth-right, and therefore becomes you exceedingly; the cast of your benign countenance is full of "abnegation of self on the one hand, and divine charity and the spirit of universal love on the other," and your words flow with

Fevers, p. 102. (3) Moore's Medical Sketches, p. 103.

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more tenderness than even "the affectionate phraseology of Messrs. Goss & Co.," it will, therefore, fit you admirably, even to a degree "which it is painful to contemplate."

In Case 2, the patient after a month's treatment stated "himself to be perfectly well." "Thou dost not say, Dr. Hocken, that the man was well?" The author does say it, however, and that even unappalled by the long string of Ignatius Loyolas, Eugène Sues, Juif Errants, &c., which flow like oaths from the pen of the learned critic. "Shall we inform fathers and mothers of families of these '*cures*,' in a neat pamphlet, (or in startling characters on the back of our journal), bearing our address in prominent type, and tell them how we did it all with 'vin. ipecac.' and mutton chops?"—"As the bad man believes all bad, &c." "Unfortunately," says the reviewer, (happy to catch even at a straw, so nearly engulphed is he in his own spirit of universal love), "energetic Dr. Hocken goes a little too far, or in struggling to avoid Charybdis, founders on Scylla." "The voice was *equally* resonant on both sides." "Now as it is well known, the vocal similarity on both sides can only exist when the natural resonance has either risen above par on the left side, or fallen below par on the right?" Doubtless thy opinion, learned critic, is better than that of Louis', whom we shall quote, nevertheless, —He says that, in examining twenty-two healthy females, he found resonance of the voice more marked on the *left* side beneath the clavicle in one instance than on the right—posteriorly *alike* on both sides. Louis also states* that, "just as the character of the respiratory murmur is not always the same at the apices of both lungs in the natural state, so likewise does the vocal resonance vary in those regions." He remarks also, that it is inferrible that increased resonance of the voice "always possesses a certain share of importance on the left side, more especially *if it be not present on the right*."

In stating that the voice was equally resonant *on both sides*, we merely stated a "*fact*," and allowed our readers to form their own conclusions of its meaning,—at the same time that we did not consider this sign of any importance, nor as any proof that the disease continued.

Omitting Cases 3 and 4, (why?) we are brought by our critic to Case 5. He states that, five months after the treatment was commenced, "she was in precisely the same state, if not worse, than when first seen. And yet in a tabular view of the cases at the end of the book, the 'Results' in this instance are stated to have been, '*symptoms much relieved; physical signs improved.*'" He (unintentionally, of course) omits to mention the author's "remarks," which fully explain his meaning. They are as follows:—"no flattening was present at the end of May in the case of Sly, but having omitted to continue her medicine from June to September, her disease, *which had been previously relieved but not cured*, advanced in the interval, and some decided flattening was present when she again came under my care."

* Walshe's Trans. p. 441.

The reviewer's comments on case 6 in "plain English" prove that he is ready at any moment's notice to swear black's white. In case 7 he remarks "But suppose the patient was actually cured of tuberculous disease, why might not the favourable result have been produced by the seton, the prussic acid, the cupping, the opium, the blisters, or the tartar emetic ointment, singly or combined!" Because they have been tried in ten thousand times ten thousand instances and have never once produced a like result. How many patients (victims?) has our reviewer seen cured by them?

The reviewer's eager search after truth is evident in his wilfully omitting the author's remarks on this very subject, and his *honour* from the indirect falsehood he is guilty of, by implying that he was silent on the subject. The author remarks (p. 59.) "*In* some of the cases it is true that other medicines were combined with it (naphtha), and part of the benefit might (and perhaps justly) be attributed to them, and to the counter-irritation used at the same time. I have no doubt of the value of counter-irritation, of hydrocyanic acid, or of opium, but still they do not give the same kind and amount of relief as the naphtha effects. Enough proof *also* will be found in the cases narrated of the power of naphtha, to occasion the same relief when employed *alone*." The reviewer states that this patient took "*three* minims of acid hydrocyan. dilut. ter die," willfully misquoting from the authors Essay where it is stated she took "*one* minim."

These are the fancied "flaws" which the critic has detected "in the narrative of the physical examinations"—"the very perfection of the details betraying the manner in which the whole effect was produced." "Probably Dr. Hocken imagined the alleged increase of loudness of the sounds (of the heart) in deference to systematic authors," when the critic himself doubts the fact, immediately before, from the very circumstance that such was opposed to the *usual* account of books! The reviewer belongs to that happy class who fancy that because the details of a case are *correct*, it must be ideal; but if in the slightest degree *incorrect* (according to his own preconceived and enlightened views) it is "introduced to make things square."

In the author's second class of cases, our reviewer goes on to say "but when Dr. Hocken adds 'expiration being audible on both sides, and slightly more prolonged than usual—*somewhere* about one third the length of inspiration, instead of about two to ten,' we confess we have misgivings and the blight of suspicion crosses our mind. Had Dr. Hocken made himself practically acquainted with the relative duration of inspiration to expiration, he would have known that Fournet's ratio of ten to two, was one of the errors into which this accomplished auscultator fell: he evidently supposed that ratio was generally admitted, and endeavours to produce the impression that he had ascertained it himself." Supposing the author were deceived as to the general admission of this ratio what alteration does that make in the fact that the expiration was

(1) [illegible]

Fevers, p. 102. (3) Moore's Medical Sketches, p. 103.

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as one to three? or what endeavour is there to prove that he had ascertained the ratio himself? we gave the ratio in deference to the opinion of Fournet; our own examination leading us to the conclusion that it varied so much in different instances as to be almost impossible to fix one; and that Fournet's (for the right side) although not absolutely correct was still the best.

The tabular view of the cases was drawn up from the author's note-book before the Essay itself was written; and as his object was to give "*the truth, the whole truth, and nothing but the truth,*" the further particulars of the cases were added up to the day at which they were written for the press, with additional information in the "Appendix" after the sheets were printed off. If any variations exist in the tabular "results," and in the state of the patient at the latest date mentioned, this fact will account for them. Case 9, which the reviewer thinks most in his favour, is in fact the strongest proof of his *honour, truth,* and *wisdom*—even to drivelling. "She" says the critic "was discharged in a worse general state (for emaciation was then on the increase, and the tongue covered with aphthæ) than when first seen. Yet the 'results' are 'great relief to general symptoms, and improvement of physical signs, *with occasional relapses!*" He does not add (from a sincere desire to ascertain the truth?) that the case is headed—"*relapse of the symptoms and occurrence of some laryngeal desease,*" nor that the author states "the case (p. 41.) of Cooper is still doubtful, *but will probably terminate fatally.* There is evidence nevertheless of benefit having been effected by naphtha, even in her case. All her symptoms have been invariably alleviated by the remedy, *till the occurrence of some laryngeal complication,* whilst at the same time the physical signs have on the whole steadily improved." The appendix contains an account of her death, and the post mortem examination, and yet the reviewer states that "her '*case*' (as if in mockery of *herself)* must serve to swell pages sufficiently well calculated to entrap the unwary, and bamboozle the ignorant—from the grave, she was summoned to bear mute witness to the powers of naphtha, and to aid in enticing sufferers like herself to confide in the honesty and skill of its vendors."

The relapse under which the patient laboured (and which we stated would probably terminate fatally,) was occasioned by the disease suddenly assuming an acute character—a rapid and abundant deposition of tubercle took place in both lungs, which underwent very speedy softening, at the same time that laryngeal disease was established. During this attack the author was unable (from the exhausted state of the patient) to examine the whole of the chest; contenting himself with the superior parts, and these less carefully than he desired. The physical signs thus discovered were less than he anticipated; but notwithstanding preconceived views, he gave them exactly as he found them. Had he been inclined to copy from systematic writers (as the critic states,) would not this have been a most favourable opportunity? The woman died, not

as the reviewer states, "from the ordinary progress of the disease but from the causes we have mentioned, and the post-mortem furnished abundant proofs of this being the case. The reviewer takes care to omit all mention of laryngeal disease, and to explain what portion of the apex was left as a "cap or hollow cone of tuberculous matter" when the lung was removed. It was "that portion which passes up beneath the clavicle," the author's Essay states. Of course this is not omitted with the intention of misleading the reader, and making him believe that it was more extensive! Notwithstanding the reviewer's appeal to the words "miracle" "painful dilemma" "sucking babe" and "flying colours," we are sure that good auscultators could have heard no more, (probably from the lungs admitting little or no air into their superior parts, except anteriorly where they were most healthy, especially on the right side,) although doubtless, our reviewer would have heard *all that he* wished or perhaps—the rustling of the bed-curtains with which his well informed brain would have been amply satisfied.

Why does the critic omit all mention of the ten cases of advanced consumption? simply because *he knows* it would have greatly weakened his argument to have shewn that the author found naphtha of no permanent use in those cases, (as a general rule); and therefore does not wish to overrate its value.

We dimiss the exceedingly coarse and ungentlemanly comments which follow on the author's statement, that "in certain quarters the employment of naphtha has been regarded merely as a piece of quackery and imposition," with the contempt it merits. The critic states that the author insinuates that "*regular*" practitioners regard "consumption as a disease necessarily running a steady and unchanging course to death." A more palpable *untruth* could not have been written, inasmuch as the author commences his Essay with proof to the contrary (which, as we have already remarked, our critic takes good care to omit,) at p. 3 of the Essay we quote from Dr. Graves, that "several remarkable cases of phthisis have occurred in my own practice, and in the practice of Dr. Stokes, in which the patients *recovered*, either *temporarily* or *permanently*, in a manner quite unforseen and unexpected." It may be looked upon as established, that phthisis like most other diseases does not *always necessarily* progress to a fatal termination."

"We know that people may with ease,
Twist words and meanings as they please."

"Four pages," says the review, "are stitched into the work, filled with every little scrap of commendation or simply courteous mention, gathered from every possible source, whether distinguished or obscure (!!) that has been bestowed upon any one of Dr. Hocken's literary productions. His success in this direction proves that he would in all likelihood be an excellent person to find a needle in a bundle of straw." As we have never had reason to complain of wilful falsehood in any reviewer but that of the British and Foreign Medical, we will allow others to judge of the value of their criticisms.

(3) Moore's Medical Sketches, p. 103.

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One would have thought that this list must have contained some quotations from political journals, &c. We will mention the names of *all the works* quoted, to allow the reader to judge:—Dr. Johnson's Journal, Edinburgh Medical and Surgical Journal, Edinburgh Monthly Journal of Medical Science, Northern Journal of Medicine, Dublin Medical Press, Lancet, American Journal of Medical Science, British and Foreign Medical Review, Medical Gazette, Provincial Medical Journal, Braithwaites Retrospect, Duffin on Strab, Walker's Oculist's Vade mecum, Lucas on Strabismus. So much for the *man of straw* who has professed to review our work! notwithstanding our "excellence" in "finding needles in *bundles of straw*" we have searched our *critic* in vain; we have found nothing but arrogant presumption, excessive ignorance, falsehood "to a degree which it is painful to contemplate," and a malicious intent to slander his neighbour.*

We feel ourselves happy in the conviction that we possess the esteem and friendship of some of the most distinguished men in all parts of the world, and that we have succeeded in the object with which our Essay was published, viz: to induce *honest* and *able* practitioners to give the remedy *a fair trial*. We have always been most anxious that the best auscultators should examine the cases under our care, and sift the truth of our diagnosis: accordingly some of them have been examined by Drs. G. Burrows, G. Bird and P. M. Latham. With this view we offered to send Dr. Forbes a patient, some short time since, who had been benefitted by naphtha. Fancy, reader, the latitude of his conscience to address the author as "Dear Sir" "I am much obliged for your offer," &c. "It is unnecessary for my own own satisfaction, as I have previously exhibited, and am exhibiting the ("the" or the "other"?) naphtha, with a view to ascertain its therapeutic effect." Was Dr. Forbes afraid of having his sense of hearing and diagnosis tested by a comparison with others?

It is a homely but true proverb, that "the proof of the pudding is in the eating," and I hereby publicly challenge my reviewer to a comparative trial of our skill in physical diagnosis—to be decided by a committee of *able men*, not selected from the clique of Drs. Forbes and Co.

* Of a piece with the insolent insinuations which abound in the (so called) review, is the indirect assertion that the author recommended a dispensary patient to place herself under the *private* care of Dr. Hastings, (Case 13). The facts of the case being, that the author met with a severe accident in the summer, and, in consequence, was compelled to relinquish practice. During this period the patient, (a daughter of Paget, the Cutler, 195, Piccadilly), who from her station in life ought not to have become a dispensary patient, placed herself under Dr. Hastings' care, quite unknown to the author.

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Fevers, p. 102.

(3) Moore's Medical Sketches, p. 103.

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N. Y. COMMERCIAL ADVERTISER.

Thursday Evening, April 15.

Report of the Physicians.

WASHINGTON, April 4, 1841.

DEAR SIR:—In compliance with the request made to us by yourself and the other gentlemen of the Cabinet, the attending and consulting physicians have drawn up the abstract of a report on the President's case, which I herewith transmit to you. Very respectfully, your obedient servant,

THOMAS MILLER, Attending Physician.

To the Hon. D. WEBSTER, Secretary of State.

On Saturday, March 27, 1841, President HARRISON, after several days' previous indisposition, was seized with a chill and other symptoms of fever. The next day pneumonia, with congestion of the liver and derangement of the stomach and bowels, was ascertained to exist. The age and debility of the patient, with the immediate prostration, forbade a resort to general blood-letting. Topical depletion, blistering, and appropriate internal remedies, subdued, in a great measure, the disease of the lungs and liver, but the stomach and intestines did not regain a healthy condition. Finally, on the 3d of April, at 8 o'clock, P. M., profuse diarrhœa came on, under which he sank, at thirty minutes to 1 o'clock, on the morning of the fourth.

The last words uttered by the President, as heard by Dr. Worthington, were these: "Sir, I wish you to understand the true principles of the Government. I wish them carried out. I ask nothing more."

THO. MILLER, M. D., Attending Physician.

CONSULTING PHYSICIANS.

FRED. MAY, M. D.,
N. W. WORTHINGTON, M. D.,
J. C. HALL, M. D.,
ASHTON ALEXANDER, M. D.

On the above "abstract of a report" I beg leave to offer a professional criticism for the consideration of medical men. The illustrious subject of the report, having fallen a victim to the disease, is beyond the reach of any benefit from these *post mortem* reflections; but the health and life of other illustrious men must be exposed to imminent peril if the pathology and therapeutics of these five physicians should become a precedent, by being permitted to pass in silence.

It appears that "after several days previous indisposition," President Harrison was "seized with a chill and other symptoms of fever," on Saturday, 27th March. The nature of this fever seems not to have been apprehended until "*the next day*," when "Pneumonia with congestion of the liver, and derangement of the stomach and bowels, *was ascertained* to exist." This diagnosis seems to have been made out on Sunday, twenty four hours after his being "seized with a chill," and it does not appear that any treatment had been pursued until this was the liver, and derangement of the stomach and bowels." This state of things, it will be recollected, had been preceded by an "indisposition of several days, and "a chill and other symptoms of fever" on the preceding day. That pneumonia, under such circumstances, was accompanied by a "congestion of the liver" is not wonderful, no appropriate treatment having been premised; and the "derangement of the stomach and bowels" would inevitably result from such congestion, if there had been no previous morbid state. So that thus far there is nothing remarkable in this case, except the delay in ascertaining what the disease was, when from their own showing it was so well characterized. No man of his senses can doubt that with the rational signs of pneumonia admitted to have been present, the physical signs would have enabled them to "ascertain" its existence on Saturday, instead of waiting till Sunday, when congestion of the liver and functional disturbances of other organs had supervened, and complicated the case.

But waiving these obvious blunders in the diagnosis, we come next to the treatment, and here we find this extraordinary statement. "The age and debility of the patient, with the immediate prostration, forbade a resort to general blood-letting." This appears to be an apology, on the face of it, for not resorting to *general blood-letting*, and most truly an apology is due to the profession, and to the public for omitting a remedy which all enlightened men in every country regard as their *sheet-anchor* in such circumstances. If the disease is correctly stated as *pneumonia*, this remedy was called for, imperatively, but complicated as it confessedly was in this case, by "congestion of the liver," its propriety and necessity were absolute, beyond the possibility of a doubt. Neither the "age, debility, nor prostration" of the patient offers any show of pretext for so flagrant an omission. To allege the age of 68 years as forbidding blood-letting in pneumonia is worse than puerile. Dr. Rush, in his treatise on the diseases of old age, has demonstrated that bleeding is more necessary in advanced age than in any other period of life, and every practitioner is familiar with the value of this remedy in such cases among aged people. Nor had the President reached an age which could forbid a resort to general blood-letting.

Indeed the authors of the report seem to be aware of the insufficiency of this apology, and hence connect with his age "*debility*" and "*prostration*" as "forbidding a resort to general bloodletting." But they do not discriminate with professional accuracy here, else they would ascribe this debility and

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